Understanding the (perverse) effects of disclosing conflicts of interest: A direct replication study

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ABSTRACT

Advisors are often subject to conflicts of interest—a potential clash between their professional responsibilities and personal interests. Such conflicts can increase bias in advice. Although disclosure is frequently proposed to manage conflicts of interest, it can have unintended consequences on both advisees and advisors. In seminal work, Cain, Loewenstein, and Moore (2005, 2011) demonstrated that advisors give more biased advice with disclosure than without, to the detriment of advisees’ financial payoffs. However, recent experiments by Sah (2018), have revealed that in certain contexts, advisors give less biased advice with disclosure relative to nondisclosure. To further understand the contradictory findings and increase confidence in the original results, I conducted a direct replication of the main study from Cain et al. (2011) on advisors (with slight changes for advisees). I replicated the original finding that advisors give more biased advice with conflict-of-interest disclosure (vs. without) when giving recommendations on the sale price of houses to advisees who have less information. Like Cain et al., I found little or no support for effects on advisees, such as advisees giving higher estimates, being less accurate, or discounting more, with conflict-of-interest disclosure relative to nondisclosure. However, a meta-analysis revealed that across the original study and this replication, advisees were financially worse off with disclosure than without. I discuss when conflict-of-interest disclosure can lead advisors to give more or less biased advice.

1. Introduction

Conflicts of interest (COIs) represent a potential clash between an advisor’s professional responsibilities (to place advisees’ interests first) and the advisor’s personal interests (Moore, Cain, Loewenstein, & Bazerman, 2005). Common across professions and industries, such conflicts often lead to biased advice. For example, physicians working in the U.S. fee-for-service model may recommend unnecessary tests or procedures (Gawande, 2009; Sah, 2015), financial advisors may recommend investments that give them larger commissions (Boatright, 2000), and bloggers who receive payment or products from a company may write overly positive reviews of the sponsoring company’s products (Sah, Malaviya, & Thompson, 2018). Among the possible policy responses to managing such conflicts, disclosure—informing recipients of their advisors’ COI—is ubiquitous (Sah, 2017).

Prior research, often using monetary incentives to create real financial COIs, has uncovered some unintended consequences of disclosing COIs for both advisees and advisors. Advice accompanied by a COI disclosure may lead advisees to feel greater pressure to comply with their advisors’ recommendations, even if they trust the advice less, so as to not signal that distrust to their advisor (Sah.
Loewenstein, & Cain, 2018) or to avoid appearing unhelpful in satisfying their advisors' interests (Sah, Loewenstein, & Cain, 2013). Advisees may even end up trusting their advisor more if they interpret the disclosure as an expertise cue (Sah, Fagerlin, & Ubel, 2016; Sah, Malaviya, et al., 2018), or they may discount too much, leading them to ignore or reject potentially valuable high-quality advice (Sah & Feiler, 2018).

For advisors, prior innovative research has revealed that COIs can lead them to give even greater biased advice with disclosure than without, at the detriment of advisees. Two spearheading papers show this effect (Cain et al., 2005, 2011). The first paper documents a study in which advisors and advisees estimate the values of jars of coins (Cain et al., 2005). Advisees were paid according to the accuracy of their estimates (and both advisors and advisees knew this). However, some advisors were paid based on how high (relative to the actual values) the advisees' estimates were. Advisors examined the jar of coins closely and then completed a report of their estimate that would be sent to the advisee. In one condition, the advisors' COI was disclosed prominently on the report. In another condition, the advisors did not disclose their COI. In all conditions, advisees received the report and then could view the jar in question but only from approximately three feet and for only about 10 seconds. Those advisors who had to disclose their COI gave significantly more biased advice than advisors who had a COI but did not disclose it. Advisees' estimates did not differ with disclosure condition nor did they discount more with disclosure relative to nondisclosure. Advisees' payoffs were less with COI disclosure vs. nondisclosure but this result was non-significant ($p = .052$).

The main study (Study 3) in the second paper by Cain et al. (2011) also revealed increased bias in advisors who disclosed their COI relative to advisors who did not disclose. This study had several improvements to the first paper, including richer stimuli materials, which gave advisees the opportunity to rely on their own judgments if they wished to discount the advisors' advice (Cain et al., 2011); in the first paper, it was harder for advisees to make accurate judgments on the value of the jars, which may have made them more dependent on their advisor despite knowing about the advisor's COI. The second study also employed a full two by two factorial design, i.e., advisors either had a COI or not and they had to disclose or not disclose.

In this second study, advisors, who had more information than advisees, gave advice regarding the sales price of several houses to advisees. Advisees were always rewarded for their accuracy in estimating sale prices, and again both advisors and advisees knew this. As in the first paper, advisors who had to disclose their COI gave more biased advice than advisors who did not disclose their COI. Again, advisees' estimates did not differ with disclosure vs. nondisclosure in the conflicted conditions nor did they discount more with COI disclosure than without COI disclosure. However, advisees' payoffs were significantly lower with COI disclosure than without.

These ingenious experiments make an important contribution to the literature. The fact that COI disclosure could increase bias in advisors and leave advisees financially worse off, has, rightly so, raised concerns about the use of such disclosures in practice. Indeed, the influential finding has gained traction with both academics and policymakers, who now understandably question the effectiveness of COI disclosures (Ben-Shahar & Schneider, 2011; Editors, 2012; Prentice, 2011).

Recent experiments, however, have revealed that increased bias with COI disclosure may not exist in all contexts. Specifically, Sah (2018) found that when participants gave advice in simulated medical contexts, advisors actually gave less biased advice with disclosure relative to nondisclosure. At least two possible explanations exist for this finding. One is that the original findings were the result of Type 1 errors (false positives). Another is that context may be a moderator of the effect and COI disclosure may lead to less bias in advice in some contexts and more bias in advice in other contexts. A direct replication of the original finding is thus valuable to differentiate between these possibilities and to make sense of the contradictory findings, especially given the importance and impact of the original studies.

I conducted a direct replication of the main study (Study 3) in Cain et al. (2011) to examine whether advisors give more or less biased advice with disclosure as compared to nondisclosure. Although the outcomes of replication studies are often difficult to interpret, such studies are nonetheless informative in establishing the robustness of, and increasing the confidence in, an effect (Earp & Trafimow, 2015). A direct replication may be defined as an experiment intended to be as similar as possible to the original (Schmidt, 2009); the purpose is to reproduce the original predicted effect which increases confidence in the veracity of the effect. In contrast, a conceptual replication seeks to test the underlying theory or phenomenon as well as to establish boundary conditions by intentionally changing some variables (Earp & Trafimow, 2015), such as the context in which participants give advice (Sah, 2018). Both direct and conceptual replications are important. However, to conduct informative conceptual replications, we must first be sure that the original finding is reliable. In my direct replication, I use a similar sample of participants from a (different) private U.S. university community, similar stimuli materials, procedures, and sample sizes. As in the original study, I used monetary incentives to create real financial COIs for advisors. Cain et al. (2011) made three predictions for their study:

**Hypothesis 1:** Advisors with COIs will give more biased advice with disclosure than without disclosure.

**Hypothesis 2:** Advisees will be less reliant on the advisors’ advice with disclosure vs. nondisclosure in the conflicted conditions.

And, because advisees will not discount sufficiently enough with COI disclosure:

**Hypothesis 3:** Advisees will give estimates that will be (a) higher and (b) more dispersed (and therefore less accurate) and thus will (c) lead to lower payoffs for advisees with disclosure vs. nondisclosure in the conflicted conditions.
The original study found support for Hypotheses 1 and 3c but did not find support for Hypothesis 2 nor Hypotheses 3a and 3b. I examine the same hypotheses in this replication study.

2. Method

2.1. Participants

One hundred and forty-five participants (89 women, 55 men, 1 gender unreported; $M_{\text{age}} = 25.97, SD = 10.74$) at a large Eastern U.S. university played the role of advisors in a study in which they could earn a bonus payment (awarded to them via online gift cards after the study was completed) in addition to a show-up fee of $10. A further 145 participants (79 women, 66 men; $M_{\text{age}} = 19.54, SD = 1.30) played the role of advisees and were paid according to the accuracy of their responses.¹

2.2. Design and procedure

Advisors were seated at different computer stations and randomly assigned via a computer-generated link to one of four conditions in a 2 (accurate vs. conflicted) x 2 (disclosure vs. nondisclosure) between-subject design.²

As in Cain et al. (2011), advisors had to give advice to advisees on the (final completed) sale prices of four houses, presented in randomized order and ranging from $193,000 to $423,000. See Appendix A for the advisors’ instructions in this replication study. Advisors, like advisees, had an information pack for each house, including a photo and basic information about the property (e.g., year of construction, square footage, number of bedrooms, bathroom, floors, etc.); see Appendix B for an example house (houses were updated from the original stimuli to houses that had sold in more recent years). Advisors, however, had additional information in their packets regarding recent sale prices and market values of comparable houses and the market value of the house in question, and were aware that the advisees did not have this information. They were also aware that advisees would be rewarded for their accuracy in estimating the actual sale price of the house (e.g., advisees received $5 if they were within $2500 of the sale price, $4.50 if within $5000 of the sale price, and so on, down to $0.50 if they were within $25,000 of the sale price). These monetary incentives followed a similar pattern to the original study but were increased for advisees, with the highest reward for accuracy increasing from $2 to $5.

In the two accurate-reward conditions, advisors were rewarded for how accurate the advisee was, i.e., their reward followed the same reward structure as that for advisees ($5 if the advisee was within $2500 of the sale price, $4.50 if within $5000 of the sale price, etc.). In the two conflicted/high-reward conditions, advisors were paid more if the advisee gave a higher sale price relative to the actual sale price ($1 if the advisee gave an estimate $10,000 higher than the sale price, $2 if it was $20,000 higher, and so on, increasing $1 for every $10,000 up to $5 if the estimate was $50,000 higher than the sale price; again, this monetary incentive followed a similar pattern to the original study, in which the highest payoff for advisors was $5.50). See Appendix A for further details on the incentive structure used in this study.

Advisors were randomized to either disclose or not disclose their reward structure. As in Cain et al. (2011), the disclosure statement read, “As an advisor, I am required to inform you that I am paid based on how [high/accurate] your estimate of the property sale-price is relative to the actual sale-price.” For the nondisclosure conditions, no statement was given to the advisees.

For each of the four houses in turn, advisors reviewed the materials and then entered their suggested sale price on an online “Advisor’s Report,” which they understood would be sent to the advisee. After giving advice, advisors were asked to enter their own private estimate and were assured that the advisee would not have access to, or ever see, this estimate. Unlike Cain et al., these private advisor estimates were not rewarded based on accuracy. As explained to the advisors, at the end of the study, one of the four houses was selected at random, and the advisor’s report for that house, along with the advisee information pack, was presented to an advisee.

The procedure for advisees differed slightly from the original study. In the original study, all participants were recruited at the same time and randomized to the role of advisor or advisee. Both advisors and advisees were in the same room during the study. As in the original study, I paired advisees with advisors who had better information. However, in the present study, advisees were recruited and completed the study after advice had been collected rather than at the same time as the advisors. Also, in the original study, advisees received advice for all four houses and made four different estimates, and after the first two rounds of estimates, advisors and advisees received feedback on the actual sale price of the house and advisors were informed of the estimate of their advisee. As Cain et al. (2011) state in their paper, this setup was not ideal to examine the impact of feedback and feedback had no significant effect on the results. At the end of the study, one round of estimates was randomly selected for computing actual payoffs for advisors.

¹ The original study (Cain et al., 2011) had 126 advisors and 135 advisees. In this replication, there are 145 advisors and 145 advisees; 152 advisors were recruited, and seven advisors were removed prior to analyses for either indicating that they did not understand the study or giving responses that were extreme outliers, e.g., house sale prices of less than $35,000. The sample sizes of Cain et al. (2011) and this replication study are substantially increased from the first study (Cain et al., 2005) in which there were approximately 22 advisors per condition. Replication studies usually contain much larger samples than original studies to account for any publication bias and to detect effect sizes that are smaller than the original study (Brandt et al., 2014). Due to budgetary constraints, the sample size for this replication study was based on at least matching the sample size from the Cain et al. (2011) study. All analyses were conducted after data collection was completed.

² A further 137 participants were randomized into an unrelated (medical scenario) paradigm. This paradigm is not part of the replication and not reported here.
and advisees. In the present study, although advisors again gave advice four times for four different houses, only one house was randomly selected to be presented with the advice for that house to an advisee. This was to simplify and shorten the study procedure for advisees. In all other aspects, the procedure for advisees was the same as in the original study. After receiving the advisee information pack for the selected house and the advisor's report, the advisees gave an estimate of the sale price and were rewarded based on accuracy, as described above. Advisors were rewarded based on the advisees' responses and paid via online gift cards that were sent to them after advisees had completed the study.

3. Results

As in the original paper (Cain et al., 2011), I report statistical analyses using a one-way analysis of variance (ANOVA) on all four conditions with planned contrasts. For a robustness check, I also conducted the traditional two-way ANOVA analysis for a 2 (reward structure: accurate vs. conflicted) × 2 (disclosure vs. nondisclosure) between subject-design; these analyses followed the one-way ANOVAs unless noted in the footnotes.

3.1. Advisor decisions

The results for the advisors' recommendations and personal estimates replicate previous research (Cain et al., 2011). As in the
original study, I conducted a one-way ANOVA with planned contrasts, which compared the reward conditions (accurate vs. conflicted) and the effect of disclosure vs. nondisclosure in the conflicted conditions. Also, as in the original study, I verified that the effect of disclosure vs. nondisclosure in the accurate conditions were not significantly different for any of the analyses.

Table 1 presents for each condition mean (across all four houses) advisor personal estimates, omnibus $F(3, 141) = 0.45, p = .72$. 

Fig. 2. Advisors recommendations by condition. Note: Horizontal lines show the median (within the box), 25th and 75th percentiles (top and bottom of the box), 10th and 90th percentiles (small outer whiskers) and minimum and maximum values.

Fig. 3. Advisors recommendations relative to their personal estimates by condition. Note: Horizontal lines show the median (within the box), 25th and 75th percentiles (top and bottom of the box), 10th and 90th percentiles (small outer whiskers) and minimum and maximum values.
## Table 2
Advisee estimates and payoffs.

<table>
<thead>
<tr>
<th></th>
<th>Accurate-undisclosed</th>
<th>Accurate-disclosed</th>
<th>High-undisclosed</th>
<th>High-disclosed</th>
<th>Effect of condition (Omnibus test) Acc, Acc-D, High, High-D</th>
<th>Effect of incentives Accurates vs. Highs</th>
<th>Effect of disclosure High vs. High-D</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adviser estimates</td>
<td>$248,433 (74,648)</td>
<td>$222,375 (35,304)</td>
<td>$296,375 (93,493)</td>
<td>$314,142 (129,733)</td>
<td>(p &lt; .001)</td>
<td>(p &lt; .001)</td>
<td>(p = .39)</td>
</tr>
<tr>
<td>Absolute advisee discounting</td>
<td>$55,097 (61,894)</td>
<td>$44,114 (47,627)</td>
<td>$42,786 (39,315)</td>
<td>$14,927 (15,180)</td>
<td>(p = .002)</td>
<td>(p = .005)</td>
<td>(p = .007^a)</td>
</tr>
<tr>
<td>Absolute (Adviser estimate – actual sales price)</td>
<td>$104,700 (81,897)</td>
<td>$74,675 (75,185)</td>
<td>$67,500 (58,633)</td>
<td>$66,514 (70,870)</td>
<td>(p = .12)</td>
<td>(p = .06)</td>
<td>(p = .95)</td>
</tr>
<tr>
<td>Adviser payoffs</td>
<td>$0.48 (1.05)</td>
<td>$1.08 (1.74)</td>
<td>$0.89 (1.43)</td>
<td>$0.34 (0.87)</td>
<td>(p = .08)</td>
<td>(p = .47)</td>
<td>(p = .08)</td>
</tr>
<tr>
<td>Advisor payoffs</td>
<td>$0.48 (1.05)</td>
<td>$1.08 (1.74)</td>
<td>$2.30 (2.26)</td>
<td>$2.17 (2.29)</td>
<td>(p &lt; .001)</td>
<td>(p &lt; .001)</td>
<td>(p = .77)</td>
</tr>
</tbody>
</table>

Note: Means across all four conditions (Standard deviations are in parentheses).

Acc = Accurate reward condition; High = Conflicted/high reward condition, D = Disclosure condition.

\(^a\) In opposite direction to Hypothesis 2.
Table 3
Support for hypotheses in the original Cain et al. (2011) study and the replication study.

<table>
<thead>
<tr>
<th>Participant focus</th>
<th>Hypothesis</th>
<th>Original study</th>
<th>Replication</th>
<th>Meta-Analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td>Advisor</td>
<td>Hypothesis 1: More biased advice with COI disclosure vs. nondisclosure</td>
<td>Supported</td>
<td>Supported</td>
<td>Supported</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 2: More discounting of advice with COI disclosure vs. nondisclosure</td>
<td>Not supported</td>
<td>Not supported</td>
<td>Not supported</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 3a: Higher estimates with COI disclosure vs. nondisclosure</td>
<td>Not supported</td>
<td>Not supported</td>
<td>Not supported</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 3b: Greater variance of estimates with COI disclosure vs. nondisclosure</td>
<td>Not supported</td>
<td>Supported</td>
<td>Supported</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 3c: Lower payoffs for advisees with COI disclosure vs. nondisclosure</td>
<td>Supported</td>
<td>Not supported</td>
<td>Supported</td>
</tr>
</tbody>
</table>

Fig. 4. Advisee estimates by condition. Note: Horizontal lines show the median (within the box), 25th and 75th percentiles (top and bottom of the box), 10th and 90th percentiles (small outer whiskers) and minimum and maximum values.

Fig. 5. Payoffs for advisees and advisors by condition. Error bars: ± 1 SE. Note: D = Disclosure condition.
(row 1), advisor recommendations, $F(3, 141) = 7.76, p < .001$ (row 2), advisor recommendations minus the actual sales price of the house, $F(3, 141) = 7.76, p < .001$ (row 3), and advisor recommendations minus their personal suggestions, $F(3, 141) = 8.35, p < .001$ (row 4).

In the two accurate conditions, advisor personal estimates (row 1) were not significantly different from advisor recommendations (row 2). In contrast, in the two high (conflict of interest) conditions, advisor recommendations were significantly higher than their personal estimates ($p ≤ 0.002$).

Advisors gave more biased recommendations if they were in the conflicted/high reward conditions than if they were in the accurate reward conditions, $t(141) = 4.23, p < .001$ (see Fig. 1 for advice in all four rounds by condition, and Fig. 2 for a violin plot of each condition averaged across the four rounds). For advisors in the high-reward conditions, disclosure increased recommendations of the houses’ sales prices as compared with nondisclosure, $t(141) = 2.20, p = .03$, Cohen’s $d = 0.41$ (the same effect size, $d = 0.41$, as in the original study). Advisors’ personal estimates of the sale prices of the houses did not differ with disclosure versus nondisclosure, $t(141) = −0.29, p = .77$, but advisors increased their advice relative to their personal estimates significantly more in the disclosure condition as compared to the nondisclosure condition, $t(141) = 2.88, p = .005$, $d = 0.50$ (see Fig. 3 for the distributions of advice relative to personal estimates by condition). Thus, consistent with Hypothesis 1, disclosure led to an increase in advisor recommendations as compared to nondisclosure when advisors had a COI.

### 3.2. Advisee decisions

**Advisee estimates.** Advisees gave an estimate of the sales price of one house (selected at random), $F(3, 141) = 8.40, p < .001$, see Table 2 (row 1). As in Cain et al. (2011), advisees gave higher estimates if they were in the conflicted/high reward conditions than if they were in the accurate reward conditions, $t(141) = 4.68, p < .001$, and the variance of the estimates was greater for advisees in the conflicted vs. the accurate conditions, Levene’s test of homogeneity of variances, $F(3, 141) = 15.54, p < .001$ (see Fig. 4). For advisees in the conflicted conditions, there was no effect of disclosure on their estimates, $t(141) = 0.86, p = .39$. Thus, contrary to Hypotheses 3a (and similar to Cain et al., who also did not find any effect of COI disclosure on advisees’ estimates), COI disclosure did not result in higher estimates due to the higher recommendations as compared to nondisclosure. However, the advisees’ estimates were more dispersed (that is, they had higher variance) with COI disclosure vs. non-disclosure (Levene’s test of homogeneity of variances for conflicted conditions: $F(1, 73) = 4.54, p = .04$). Thus, in contrast to Cain et al., this study did find higher variance in advisees’ estimates with COI disclosure vs. nondisclosure, as predicted by Hypothesis 3b.

**Advisee discounting.** Because discounting downwards by the advisee is predicted to occur only in the COI disclosure condition (in which advisees are informed of their advisor’s incentives to give biased advice), as in Cain et al. (2011), I conducted analyses of the absolute value of discounting (i.e., movement in any direction away from the advisors’ recommendations). This value revealed a significant effect of condition, $F(3, 141) = 5.21, p = .002$, see Table 2 (row 2). Contrary to expectations, advisees were more likely to discount their advisor’s recommendation if they were in the accurate vs. conflicted conditions, $t(141) = 2.85, p = .005$. Also contrary to Hypothesis 2, advisees in the conflicted conditions discounted more in the nondisclosure condition than in the disclosure condition, $t(141) = −2.76, p = .007$. Thus, in this study as in the original, there was no support for Hypothesis 2, which predicts higher discounting with disclosure of a COI vs. nondisclosure (the results were in the opposite direction).

This measure of advice discounting was used in the original study to investigate whether advisees were less reliant on their advisors’ advice in the conflicted disclosure condition relative to the conflicted nondisclosure condition. However, there are potential problems with this operationalization of advisee discounting; in particular, advisees’ initial judgment on the house sales price (prior to seeing the advice) was not recorded. A common and superior method of assessing how much advice has influenced an advisee is by using the weight on advice (WOA) measure which measures an advisee’s judgment before and after receiving advice and the movement towards (or away) from the advice relative to the initial judgment (Bonaccio & Dalal, 2006; Gino & Moore, 2007; Gino, 2008; Harvey & Fischer, 1997; Sah, Moore, & MacCoun, 2013; Yaniv, 2004). As others have noted, unless an advisees’ initial judgment is known, it is not possible to correctly determine how much advice has influenced the advisee (Rader, Sah, & Larrick, 2018).

**Advisee accuracy.** Cain et al. (2011) do not report the results of advisee accuracy. However, for completeness, these are reported in Table 2 (row 3) for this replication study. There was no effect on advisee accuracy by condition, $F(3, 141) = 2.00, p = .12$. Accuracy did not differ by reward structure, $t(141) = 1.90, p = .06$, nor via disclosure vs. nondisclosure in the conflicted conditions, $t(141) = −0.06, p = .95$. Thus, although the advisees’ estimates were more dispersed with disclosure relative to nondisclosure in the conflicted conditions, advisees were not less accurate with disclosure.

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3 Advice was generally lower than the actual sale prices in the accurate conditions revealing that advisors were underestimating the sales price on average across the four houses (row 3).

4 Effect sizes for the original Cain et al. (2011) study were estimated from the means, standard deviations, and sample sizes given in the manuscript.
3.3. Payoffs

**Advisee payoffs.** There was no effect on advisee payoffs by condition, $F(3, 141) = 2.35, p = .08$ (see Table 2, row 4 and Fig. 5). Payoff did not differ by reward structure, $t(141) = 0.73, p = .47$, nor via disclosure vs. nondisclosure in the conflicted conditions, $t(141) = −1.75, p = .08, d = 0.46$ [however, accounting for unequal variances, this result becomes significant, $t(65.5) = −2.01, p = .048$]. Given the overall omnibus result ($p = .08$), based on an a priori alpha of $p = .05$, Hypothesis 3c (COI disclosure will lead to lower payoffs for advisees) was unsupported in this study. Note that most advisees (73%; $N = 106/145$) received $0$ bonus due to house estimates that were greater than $25,000 difference (plus/minus) of the actual sales price.

**Advisor payoffs.** There was a significant effect of condition on advisor payoff, $F(3, 141) = 7.06, p < .001$ (see Table 2, row 5). Advisors had higher payoffs if they were in the conflicted conditions as opposed to the accurate conditions, $t(141) = 4.51, p < .001$. However, similar to the original study, advisors in the conflicted conditions did not receive more payment with disclosure than without, $t(141) = −0.29, p = .77$.

3.4. Meta-analyses

I conducted meta-analyses (to examine all the hypotheses aside from Hypothesis 3b which makes predictions about the dispersion of advisee estimates) across both the original Cain et al. (2011) study and this replication study. Using the process outlined by Lipsey and Wilson (2001), the effect size for advice given in the conflicted disclosure condition relative to the conflicted non-disclosure condition across the two studies was $0.40, 95\%$ confidence interval (CI) $(0.09, 0.73)$, a medium effect size, with a z-test value of $2.48, p = .01$. This significant result provides further support for Hypothesis 1, that advisors give more biased advice with COI disclosure than without. Similarly, in support of Hypothesis 1, a meta-analysis for advice minus personal advisor estimates revealed an effect size of $0.48, 95\%$ CI $(0.15, 0.81), z = 2.92, p = .003$.

Meta-analyses for advisee discounting and advisee estimates revealed nonsignificant effect sizes with $95\%$ CIs that included zero, providing no support for Hypothesis 2 (greater advisee discounting with COI disclosure vs. nondisclosure) and Hypothesis 3a (higher advisee estimates with COI disclosure vs. nondisclosure). However, the effect size for advisee payoffs was $−0.46, 95\%$ CI $−0.78, −0.15), z = −2.86, p = .004$, providing support for Hypothesis 3c (advisees are worse off financially with COI disclosure vs. nondisclosure).

4. General discussion

Table 3 lists each hypothesis and whether support was found in the original study (Cain et al., 2011), this replication study and with the meta-analyses. Hypothesis 1 states that advisors will give more biased advice with COI disclosure vs. nondisclosure. This hypothesis was supported in the original study, the replication, and the meta-analyses. The results on advisors therefore appear reliable, valid, and robust. With a different independent researcher from the original study, a different sample of participants (albeit still drawn from a university community), and the same paradigm but with updated stimuli materials (and some of these presented online), the main result of increased bias in advice with COI disclosure was replicated. The result was replicated for both advice and personal estimates, regardless of the fact that the advisors’ personal estimates were not incentivized for accuracy in this study. It also did not appear to matter to advisors if advisees were present in the room at the same time as the advisors with regard to their advice-giving behavior. This is consistent with the “Many Labs” Replication Project, which found that replicability is more dependent on the effect itself than on the sample and setting used to investigate the effect (Klein et al., 2014). With only small increases in sample size from the original study, this replication suggests a strong reliable result.

In contrast, the results for advisees show more variability, as in the original study. Cain et al. (2011) found no support for Hypothesis 3a (that advisees will give higher estimates with COI disclosure vs. nondisclosure). I replicated this null result and the meta-analysis confirmed no support for this hypothesis. Similarly, there was no support in the original study, the replication and the meta-analysis for Hypothesis 2 (that advisees will be less reliant on the advisors’ advice with COI disclosure vs. nondisclosure of the COI). In fact, the results in the replication study support the opposite pattern: advisees in the nondisclosure condition discounted the advice more than advisees in the COI disclosure condition. As noted in the results section, there are potential problems with this operationalization of advisee discounting. More empirical work is needed to examine if the level of discounting by advisees is actually greater in conflicted conditions with disclosure vs. nondisclosure or if advisees are relatively unresponsive to the COI disclosure or react erratically to it.

Note that although the original study did not find support for Hypothesis 3b (that COI disclosure will result in a greater variance of estimates vs. nondisclosure), the present replication did find support for this effect. COI disclosure may result in greater variability in estimates (Fig. 4), as advisees may be uncertain as to how to respond to the disclosure.

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5. Cain et al. (2011) mention accounting for unequal variance once (in their advisee discounting results). Therefore, I include the finding with unequal variances here to note that it changes the significance of the results. The two-way ANOVA also revealed a significant finding; an interaction between incentive condition and disclosure, $F(1, 141) = 6.35, p = .01, \eta_p^2 = 0.04$. Contrasts revealed no significant results; however, the direction of results explains the significant interaction: disclosure led to directionally higher advisee payoffs in the accurate conditions, $F(1, 141) = 3.30, p = .07, \eta_p^2 = 0.02$, but lower advisee payoffs in the conflicted conditions, $F(1, 141) = 3.05, p = .08, \eta_p^2 = 0.02$.

6. Note that due to the potential issues with the measure of advisee discounting, more empirical work is needed to assess whether advisees are less reliant on advice with COI disclosure.
In sum, the results for advisees are not as reliable or robust as those for advisors. This is consistent with variable responses from consumers for other types of disclosures. For example, some studies that examine disclosure of nutritional information at restaurants report a modest decline in caloric value per transaction (Krieger et al., 2013), while other studies show no change (Downs, Loewenstein, & Wisdom, 2009; Wisdom, Downs, & Loewenstein, 2010) or even increased caloric intake (Seenivasan & Thomas, 2016). Mixed responses to COI disclosure by advisees have also been documented across prior research papers: COI disclosure has led advisees to report decreased trust (Hwong, Sah, & Lehmann, 2017; Kesselheim et al., 2012; Sah & Feiler, 2018; Sah, Loewenstein, et al., 2013, 2018), or increased trust if the disclosure is interpreted as an expertise cue (Sah et al., 2016; Sah, Malaviya, et al., 2018), or no effect on trust (Rose, Sah, Raed, Schmidt, Mercer, Michum, & Robertson, 2018). Advisees have also reported pressure to help their advisors (the panhandler effect; Sah, Loewenstein, et al., 2013) or pressure to comply to avoid signaling distrust (insinuation anxiety; Sah, Loewenstein, et al., 2018) with COI disclosure, which can lead to increased compliance with advice they do not trust. These psychological processes and perhaps others (for example, a prosocial or altruistic desire to help the advisor receive a greater bonus) may lead advisees to respond quite differently to disclosures than policy-makers may predict. Future research can continue to explore the situations in which these psychological processes are present or absent. Whatever the reaction of advisees, these mixed findings of COI disclosure on advisees demonstrate that we may not be able to rely on consumers to use disclosures as intended, discount sufficiently, or make the best decisions when informed of their advisors’ COI.

Finally, Cain et al. (2011) found support for Hypothesis 3c (that advisees would be worse off financially with COI disclosure vs. nondisclosure). Advisee payoffs were nonsignificant in the replication study ($p = .08$), but the meta-analysis revealed a significant effect size providing support for Hypothesis 3c.

This study and the meta-analyses suggest that in this context, COI disclosure is likely to lead advisors to give worse advice relative to nondisclosure and leave advisees in a financially worse position. This is an important finding that makes a valuable contribution to the COI disclosure literature. However, recent work on the effects of COI disclosure on advisors offers a more optimistic view, suggesting that advisors do not always give more biased advice with COI disclosure; in fact, in some contexts, and with actual practicing real-world advisors (for example, registered investment advisors or physicians), advisors actually give less biased advice with disclosure, suggesting that COI disclosure could have beneficial effects (Sah, 2018).

Sah (2018) finds that disclosure prompts advisors to consider the ethical dilemma that arises with a COI: to give either self-interested advice or place advisees first. Advisors consider what they should do (the injunctive norm) and what they believe most other advisors would actually do (the descriptive norm). COI disclosure acts as a reminder of these perceived norms and guides advisors’ behavior. Therefore, for non-experts (research participants asked to play the role of advisors), COI disclosure tends to increase bias in settings where self-interested advice is deemed to be the norm (e.g., giving financial advice, or advice in settings such as the one in this paper) and decrease bias in settings in which placing advisees first is deemed to be the norm (e.g., giving medical advice). However, for experts (actual practicing financial and medical advisors), who often have professional norms that emphasize placing advisees’ interests first, disclosure can have the beneficial effect of decreasing bias in advice because it reminds advisors of this norm. Thus, norms that place advisees or self-interest first play an important role in determining whether COI disclosure increases or decreases advice quality. In this paradigm (giving advice on house sale prices) and similar ones, such as advisors giving recommendations on the amount of money in a jar (Cain et al., 2005) or the number of filled dots in a grid (Sah & Loewenstein, 2014), advisors may perceive that the norm is to be self-interested; thus, COI disclosure is likely to lead advisors to give even more biased advice.

5. Conclusion

In a direct replication study, I replicated the main findings from Cain et al. (2011) which shows that conflict of interest disclosure can lead advisors to give more biased advice on the sale prices of houses relative to nondisclosure. The effect size was also similar to the original study. As in Cain et al. (2011), there was no effect of conflict of interest disclosure on advisees’ estimates. However, there was some support that disclosure led to increased variance in advisees’ estimates and, as in the original study, that disclosure left advisees financially worse off relative to nondisclosure. This replication provides confidence about the veracity of the effect of conflict of interest disclosure on advisors in these paradigms. Note, however, that recent conceptual replications in other contexts/paradigms and with real-world advisors (as opposed to research participants playing the role of advisors) reveal that COI disclosure could decrease the bias in advice improving advice quality (Sah, 2018).

6. Declarations of interest

None.

Acknowledgements

I thank Christina Rader for her comments on an earlier draft of this paper and Adrian Camilleri for his help in producing the violin plot figures in this paper. This project was funded by a fellowship award from the Edmond J. Safra Center for Ethics at Harvard University.

Appendix A:. Advisor instructions
As the Advisor, you have been paired with another participant, who will be an ‘Estimator’. Your advice will be on the suggested sale-price of a house. This is an actual house that was on the market and sold a few years ago.

You will be provided with information on the house, but not the final sale price of the house. Your Estimator will receive similar information, but you will have access to some information that the Estimator will not possess, such as recent sale prices, comparable homes, and the market value of the home in question. In summary, you will have more information about the house than your Estimator.

[Next page]

[Accurate Reward Conditions]

Payment

You and the Estimator will both be rewarded based on the accuracy of the Estimator’s estimate, i.e., how close the Estimator’s estimate is to the actual sale price. The reward is outlined below:

If the Estimator’s estimate is within (plus or minus) --

<table>
<thead>
<tr>
<th>Sale Price</th>
<th>Reward</th>
</tr>
</thead>
<tbody>
<tr>
<td>$2,500</td>
<td>$5.00</td>
</tr>
<tr>
<td>$5,000</td>
<td>$4.50</td>
</tr>
<tr>
<td>$7,500</td>
<td>$4.00</td>
</tr>
<tr>
<td>$10,000</td>
<td>$3.50</td>
</tr>
<tr>
<td>$12,500</td>
<td>$3.00</td>
</tr>
<tr>
<td>$15,000</td>
<td>$2.50</td>
</tr>
<tr>
<td>$17,500</td>
<td>$2.00</td>
</tr>
<tr>
<td>$20,000</td>
<td>$1.50</td>
</tr>
<tr>
<td>$22,500</td>
<td>$1.00</td>
</tr>
<tr>
<td>$25,000</td>
<td>$0.50</td>
</tr>
</tbody>
</table>

It is important to remember that you and the Estimator will both be rewarded based on how close the Estimator’s guess is to the actual sale price.

[Conflicted / High reward Conditions]

Estimator’s Payment

The Estimator will be rewarded based on the accuracy of their estimate – i.e. how close they are in their estimate to the actual sale price. Their reward is outlined below:

If the Estimator’s estimate is within (plus or minus) --

<table>
<thead>
<tr>
<th>Sale Price</th>
<th>The Estimator earns</th>
</tr>
</thead>
<tbody>
<tr>
<td>$2,500</td>
<td>$5.00</td>
</tr>
<tr>
<td>$5,000</td>
<td>$4.50</td>
</tr>
<tr>
<td>$7,500</td>
<td>$4.00</td>
</tr>
<tr>
<td>$10,000</td>
<td>$3.50</td>
</tr>
<tr>
<td>$12,500</td>
<td>$3.00</td>
</tr>
<tr>
<td>$15,000</td>
<td>$2.50</td>
</tr>
<tr>
<td>$17,500</td>
<td>$2.00</td>
</tr>
<tr>
<td>$20,000</td>
<td>$1.50</td>
</tr>
<tr>
<td>$22,500</td>
<td>$1.00</td>
</tr>
<tr>
<td>$25,000</td>
<td>$0.50</td>
</tr>
</tbody>
</table>

Your payment

You, on the other hand, will be rewarded based on how HIGH the Estimator’s estimate is relative to the sale price. Your reward is outlined below:
If the Estimator’s response is at least—

- $5,000 higher than the sale price: $0.50
- $10,000 higher than the sale price: $1.00
- $15,000 higher than the sale price: $1.50
- $20,000 higher than the sale price: $2.00
- $25,000 higher than the sale price: $2.50
- $30,000 higher than the sale price: $3.00
- $35,000 higher than the sale price: $3.50
- $40,000 higher than the sale price: $4.00
- $45,000 higher than the sale price: $4.50
- $50,000 higher than the sale price: $5.00

It is important to remember that the Estimator is being rewarded based on how close he or she is to the actual sale price, but you will be rewarded based on how much higher the Estimator guesses than the actual sale price.

[Next page]

[Nondisclosure conditions]
Note: The Estimator will NOT know how you are being paid.

[Disclosure conditions]
Note: The Estimator WILL KNOW how you are being paid.
This statement will be presented with the advice that will be given to your Estimator:

As the Advisor, I am required to inform you that I am paid based on how [high / accurate] your estimate of the property sale-price is relative to the actual sale-price.

[Next page]

Now, we will ask you view your information regarding the property. Remember, you have more information on the property than the estimator. Specifically, you will have information on the market value of the home in question, comparable neighborhood homes, and recent sale prices. This information is highlighted in gray in your information and replaced with “Only advisors have this information.” in the estimator’s view.

You will give advice on 4 houses. Your advice will go to the same Estimator for all 4 houses. We will select one of the houses at random and you and the Estimator will be paid according to the Estimator’s response for that house. Each house will be displayed but you also have the information in the pack if you want to refer to it.

[House 1: see Appendix B]

The report below will be given to your Estimator. This is the advice he or she will see and take into consideration when making his or her estimate. Please fill it out with your suggested sale-price, using only numbers.

**Advisor’s Report**

I have carefully examined the property information, along with its tax-assessed value and the sale-price of comparable houses. I suggest that it is worth

For property coded: 500,
the suggested sale-price is
Appendix B: Example house from the advisor's information pack

A property is listed below:

**APPRAISER INFO: 500 Stinhurst Drive**
(Codes: 500)

---

**Sale Date:** 05/31/2009  
**Sale Price:** $???????  
**Total Market Value:** $253,500  
**Land:** single lot

---

**Building Information**

<table>
<thead>
<tr>
<th>Total Rooms:</th>
<th>9</th>
<th>Full Bathrooms:</th>
<th>2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bedrooms:</td>
<td>4</td>
<td>Half Bathrooms:</td>
<td>1</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Exterior Finish:</th>
<th>Masonite</th>
<th>Heating:</th>
<th>Central Heat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Roof:</td>
<td>Shingle</td>
<td>Cooling:</td>
<td>Central Air</td>
</tr>
<tr>
<td>Basement:</td>
<td>No</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Fireplace(s):</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Garage:</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Finished Living Area:</td>
<td>2160 Square Foot</td>
</tr>
</tbody>
</table>

**Condition:** Good

**Comparable Houses**

<table>
<thead>
<tr>
<th>Address: 202 Stinhurst Drive, 212 Landreth Court</th>
<th>Year Built</th>
<th>Sale Price</th>
<th>Sale Date</th>
<th>Total M.V.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1996</td>
<td>$237,000</td>
<td>08/04/2005</td>
<td>$286,418</td>
</tr>
<tr>
<td></td>
<td>2000</td>
<td>$299,900</td>
<td>08/30/2000</td>
<td>$278,724</td>
</tr>
</tbody>
</table>

Note: Your estimator will receive most of this information, except that highlighted in gray.  
Total Market Value (TMV), sale-price and Total M.V. of comparable homes is replaced by  
"Only advisors have this information."
Appendix C. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.joep.2018.10.010.

References


Sah, S. (2015). Investigations before examinations: This is how we practice medicine here. JAMA Internal Medicine, 175(3), 342–343.


