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Exaggerated, Mispredicted, and Misplaced: When "It's the Thought That Counts" in Gift Exchanges

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Gift-giving involves both the objective value of a gift and the symbolic meaning of the exchange. The objective value is sometimes considered of secondary importance as when people claim, "It's the thought that counts." We evaluated when and how mental state inferences count in gift exchanges. Because considering another's thoughts requires motivation and deliberation, we predicted gift givers' thoughts would increase receivers' appreciation only when triggered to consider a giver's thoughts, such as when a friend gives a bad gift. Because gift givers do not experience this trigger, we expected they would mispredict when their thoughts count and when they do not. Three experiments support these predictions. A final experiment demonstrated that thoughts "count" for givers by increasing social connection to the receiver. These results suggest that mental state inferences are not automatic in social interactions and that inferences about how much thoughts count are systematically miscalibrated.

Keywords: theory of mind, mind perception, social exchange, social cognition, social connection

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When people exchange gifts, it is often said, "It's the thought that counts." This common wisdom suggests that the appreciation and gratitude that comes from receiving a gift are based on more than a gift's objective quality. It also depends on how much thought was put into the gift. Gifts chosen thoughtfully, with time and effort taken to identify the perfect gift, are presumably appreciated more than gifts chosen thoughtlessly, such as by choosing a gift randomly or from a receiver's wish list. Not only does a gift receiver value the gift itself, but also feels appreciation and gratitude for the thought that went into the gift as an independent source of value. This research examined when, how, and for whom a gift giver's thoughts actually count in gift exchanges.

At first glance, the common wisdom that "thoughts count" appears not only intuitively credible but also empirically reasonable. Indeed, psychological research demonstrates that people care a great deal about others' thoughts and intentions, using inferences about others' mental states as a fundamental guide to social judgments and behavior (Epley & Waytz, 2010). For example, people appear more responsible for actions they perform intentionally than accidentally (Alicke, 2000), are liked more when they help with prosocial intentions than with selfish intentions (Ames, Flynn, & Weber, 2004), appear more prejudiced when they are intentionally sexist than unintentionally sexist (Swim, Scott, Sechrist, Campbell, & Stangor, 2003), and are punished more severely for intentional harm than for accidental harm (Hogue & Pebbles, 1997; Kleinke, Wallis, & Stadler, 1992). Even being shocked with electricity hurts more when done intentionally than when done accidentally (Gray & Wegner, 2008). Others' thoughts, in many domains of social life, do indeed seem to count.

We suggest, however, that the psychological processes that lead people to consider others' mental states calls the common wisdom on the importance of thoughts in gift exchanges into question. In particular, we suggest the presumed importance of a gift giver's thoughts is miscalibrated in three ways: (1) It is exaggerated for receivers, (2) it is mispredicted by givers, and (3) it is misplaced by focusing only on gift receivers when a gift giver's thoughts actually count reliably for the givers themselves.

Exaggerated Impact: Thoughts Count for Receivers When Mental State Reasoning Is Activated

First, we predicted that a gift giver's thoughts—the time, effort, and care expended to choose an ideal gift—would count for gift receivers by increasing their gratitude and appreciation, but only when receivers are triggered to think about a giver's thoughts.

Inferences about others' intentions, motives, goals, or thought processes cannot influence a person's subsequent judgment, evaluations, or choices if those preceding mental state inferences have never been made. Mental states are, after all, inherently invisible, making them relatively easy to overlook. Although most human beings have the capacity to reason about others' mental states, we argue that this capacity is not used unless it is activated implicitly by the social context itself (e.g., Young & Saxe, 2009) or explicitly

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by a direct request to consider another's thoughts or perspective (e.g., Back & Apperly, 2010; Epley, Caruso, & Bazerman, 2006; Galinsky, Wang, & Ku, 2008). In contrast, the objective quality of a gift is directly visible and likely to capture a gift receiver's attention immediately. Once capturing attention, the object's overall quality—the extent to which it is positive or negative, liked or disliked—tends to be evaluated automatically (Bargh, Chaiken, Govender, & Pratto, 1992; Duckworth, Bargh, Garcia, & Chaiken, 2002; Fazio, 2001; Greenwald, Klinger, & Liu, 1989). We therefore predicted that the objective quality of the gift would be primary in the receiver's evaluations and that the thought a gift giver expended would be of secondary importance. Gift receivers would consider a gift giver's thoughts only when triggered to go beyond the automatic evaluation of the gift itself.

Existing research suggests that the process of thinking about the minds of others is activated when these agents behave negatively (Morewedge, 2009) or unexpectedly (Waytz et al., 2010), as part of a more general attempt to explain the causes of an agent's behavior (Pittman & Pittman, 1980; Wong & Weiner, 1981). In gift exchanges, the clearest unexpected negative outcome occurs when a close other gives a bad gift. We therefore predicted that a gift giver's thoughts would count—that is, would affect a receiver's gratitude and appreciation positively—when the receiver was given a disliked gift by a close other, but would not count when the receiver was given liked gifts from anyone or disliked gifts from distant others.

This prediction highlights an important theoretical distinction between the activation of a psychological process, such as activating the capacity to reason about the minds of others, and the application of that process, such as using inferences about others' mental states in judgment, evaluation, or choice. The distinction between activation and application is fundamental in research on the influence of stereotypes in social judgment (e.g., Gilbert & Hixon, 1991; Kunda, 1999), the influence of stored associations in behavior (Higgins, 1996), or the impact of systematic versus heuristic thinking in decision making (Chen & Chaiken, 1999). However, in research investigating mental state inferences—typically referred to as *using one's theory of mind*—the first stage of activation is often overlooked. Instead, the bulk of research on theory of mind investigates the subsequent application of this capacity once people are implicitly or explicitly triggered to make inferences about others' minds, such as by being asked to predict another person's behavior (Birch & Bloom, 2007; Wimmer & Perner, 1983) or to explain another person's behavior (Ames et al., 2004; Heider & Simmel, 1944). Even the widely cited Heider and Simmel (1944) study that is commonly used (e.g., Guthrie, 1993) to show how people attribute minds spontaneously to almost anything, in this case to geometrical shapes, did so in a context where people were asked to explain the behavior of these shapes.

In other research, mental state inferences can appear to be activated automatically, but these results arise in paradigms where participants are presented simple and unambiguous sentences in which a person's behavior or mind is the only thing that needs to be evaluated (Van Overwalle, Van Duynslaeger, Coomans, & Timmermans, 2012). When the person is the focus of attention in a social setting, mental state inferences do indeed appear to be relatively faster than other judgments made about a person (Malle & Holbrook, 2012). But this does not mean that mental state inferences are activated automatically. In a more complicated social interaction where an object is likely to be the focus of attention, such as a gift exchange, another person's mental states may not be considered at all.

The existing research strategies used to study mental state inferences could therefore make them appear more spontaneous than they actually are or even suggest that they represent a distinct neural module. According to Scholl & Leslie (1999), for instance, the "theory of mind mechanism is essentially a module which spontaneously and post-perceptually attends to behaviors and infers (i.e., computes) the mental states which contribute to them" (p. 147). And according to Frith & Frith (2012), "keeping track of these mental states . . . occurs automatically and without the need for awareness" (p. 299; see also Cohen & German, 2009; Sperber & Wilson, 2002; Stone, Baron-Cohen, & Knight, 1998; Uleman, Saribay, & Gonzalez, 2008).

We believe the experiments reported in this article provide a theoretical test of the automaticity of mental state inferences. If reasoning about other minds is spontaneous and automatic as some researchers argue, then the amount of thought should influence a receiver's appreciation regardless of the gift's objective quality. If, however, reasoning about others' mental states must be triggered or activated (Apperly & Butterfill, 2009; Epley, Keysar, Van Boven, & Gilovich, 2004; Epley, Morewedge, & Keysar, 2004; Keysar & Barr, 2002; Zaki & Ochsner, 2011), then a gift giver's thoughts should count only when they are activated for gift receivers.

Mispredicted Impact: Gift Givers Fail to Anticipate When Their Own Thoughts Count

If gift receivers consider a gift giver's thoughts only when triggered to do so, then gift givers are likely to have difficulty correctly anticipating the impact of their own thoughts and intentions. Gift givers, after all, have a very different perspective on the exchange than do gift receivers. Gift givers do not experience the automatic evaluation that comes from receiving a gift and thus would not be triggered to use their thoughts to predict a receiver's feelings and evaluations in the same way as gift receivers. Thoughts may indeed count for gift receivers, but not necessarily in ways that givers will predict. The capacity to anticipate a receiver's feelings and evaluations is a critical aspect of gift exchanges because maximizing the receiver's happiness and satisfaction is arguably the most common objective in gift exchanges (Cheal, 1986, 1988). Any gap between a gift giver's predictions and a receiver's actual evaluations will undermine the primary goal of gift exchanges.

Misplaced Benefits: Thoughts Count for Gift Givers by Increasing Relational Connection

The common wisdom underlying "it's the thought that counts" places the impact of a gift giver's thoughts clearly within the reactions of a gift receiver, but we predicted that the impact of a gift giver's thoughts should count most consistently on the person actually having the thoughts, namely the gift giver. In particular, we predicted that putting a lot of thought into a gift would strengthen the social relationship by making the gift giver feel socially closer to the gift receiver for two main reasons. First, people typically spend more time and effort trying to please those

they like than those they dislike. The amount of time and effort spent trying to identify an ideal gift, even when experimentally induced, may therefore be interpreted by gift givers as a sign of liking and relationship closeness. Just as people can use their behavior as a guide when assessing their own attitudes and preferences (Bem, 1972), so too can people use the nature of their own thoughts as a guide (Critcher & Gilovich, 2010). Second, spending time and effort trying to identify an ideal gift is also likely to require a considerable amount of perspective taking, trying to put oneself in another's shoes and imagine his or her evaluation. Perspective taking tends to strengthen relational bonds (Galinsky, Ku, & Wang, 2005), increasing perceived closeness and similarity between people (Davis, Conklin, Smith, & Luce, 1996).

We predicted that a gift giver's thoughts would not, however, influence relational closeness for gift receivers simply because gift receivers do not experience the effort of expending thoughts or the process of perspective taking themselves. In this way the common wisdom is somewhat misplaced. Putting careful thought and attention into a gift benefits the giver, in different and potentially more consistent ways than it benefits the receiver.

The Current Experiments

In everyday life, putting thought into a gift may be systematically confounded with choosing a better gift, making it appear that a giver's thoughts count when in fact it is the quality of the gift that really counts. Gift givers may therefore think that "thoughts count" in gift exchanges simply because it leads them to choose a better gift, rather than producing additional gratitude and appreciation for the gift receiver beyond the quality of the gift itself.

To test whether people truly believe that "thoughts count" in gift exchanges beyond improving the quality of the gift itself, we conducted an experiment with 44 visitors to the Museum of Science and Industry in Chicago (the MSI) who agreed to participate in exchange for a small prize. These participants predicted how much appreciation and gratitude a gift giver would feel (on separate scales ranging from 0 to 10) after receiving a birthday gift from a friend in each of four different circumstances (order was randomized) in a 2 (gift quality: liked vs. disliked) $\times$ 2 (thought: thoughtful vs. thoughtless) within-participants design. Participants were asked to imagine that someone bought a gift for a friend thoughtfully ("putting a lot of careful thought into what the friend would like and what the friend would dislike, choosing very carefully from the large number of gifts that he or she could purchase for the friend") or thoughtlessly ("without putting any careful thought into what the friend might like or dislike, and simply choos[ing] a gift while at a store almost randomly from the gifts that are available"). In each scenario, participants also learned that when the friend opens the gift, it turns out that the friend either "likes the gift very much" or "dislikes the gift very much." These scenarios allow a direct test of how much participants from this population (that we sample from in Experiment 1–4) think their thoughts count in gift exchanges relative to the quality of the gift itself.

Participants predicted appreciation and gratitude was highly correlated ($r = .69$), so we collapsed these measures into a single positive evaluation composite. A 2 (gift quality: liked vs. disliked) $\times$ 2 (thought: thoughtful vs. thoughtless) repeated-measures analyses of variance (ANOVA) on positive evaluations revealed significant main effects of similar magnitude for both the quality of the gift, $F(1, 43) = 138.22$, $p < .01$, $\eta_p^2 = .76$, as well as the amount of thought put into the gift, $F(1, 43) = 104.27$, $p < .01$, $\eta_p^2 = .71$. Not surprisingly, participants believed that a liked gift would produce more appreciation and gratitude ($M = 7.51$, $SD = 2.42$) than a disliked gift ($M = 4.38$, $SD = 2.51$). More important, and consistent with the common wisdom that thought counts, participants also believed that gifts chosen thoughtfully would produce more appreciation and gratitude ($M = 7.44$, $SD = 2.37$) than gifts chosen thoughtlessly ($M = 4.45$, $SD = 2.63$). The interaction was nonsignificant, $F(1, 43) = 0.31$, $p = .58$. These participants believed a gift giver's thoughts would count for gift receivers above and beyond simply choosing a better or worse gift.

We designed four experiments to test our three main hypotheses about precisely when and for whom thoughts actually count in gift exchanges, using procedures that separate perceptions of the quality of a gift and a gift giver's thoughts in order to disentangle the impact of each in both givers' and receivers' evaluations. We utilized naturally occurring gift exchanges recalled from memory in Experiments 1 and 2 and utilized experimentally manipulated gift exchanges in Experiments 3 and 4. Experiment 1 tested our hypothesis that a giver's thoughts would count for receivers only when they received a disliked gift, and our hypothesis that givers would be unable to predict correctly when their thoughts count and when they do not. Experiment 2 tested our prediction that thoughts count in gift exchanges only when mental state inferences are activated by leading one group of participants to evaluate a gift giver's thoughts explicitly before evaluating the exchange. Experiment 3 tested a more focused prediction that thoughts only count when receivers are given a bad gift from a friend or loved one, but not from a stranger. Experiment 4 tested our prediction that thoughts count for givers, but not for receivers, by increasing relational closeness. Collectively, these experiments sharpen our understanding of the role of mental state inferences in social interactions, identifying when inferences about a person's thoughts and intentions influence evaluations and when they do not in an important domain of everyday social life.

Experiment 1: Givers and Receivers

In Experiment 1, we surveyed a broad range of naturally occurring gifts by asking gift givers and gift receivers to recall a liked or disliked gift. Receivers reported how much appreciation and gratitude they felt, and givers reported how much appreciation and gratitude they believed the receivers felt. We expected that the amount of thought gift givers expended would "count" for receivers—that is, would be correlated with appreciation and gratitude—only when the gift was undesirable, but that givers would not anticipate correctly when their thoughts counted and when they did not.

Method

Ninety-nine visitors (48 female; $M_{age} = 37.50$ years) to the MSI were randomly assigned to recall a time that they either gave or received a gift. Givers recalled a gift that they believed the receiver liked or disliked. Receivers recalled a gift that they actually liked or disliked.

To measure how much thoughts "counted," we asked gift receivers to report how much they appreciated the gift, how grateful they felt, and how much they liked the gift giver after receiving the gift on a scale ranging from 1 (*not at all*) to 9 (*very much*). Gift givers answered the same three questions in terms of how they believed the receiver felt. To measure "thoughts," gift givers reported how much thought they put into the gift and how much they cared about whether the receiver would like the gift on the same 1 to 9 scale. Gift receivers answered the same two questions in terms of how much thought and care they believed givers expended.

Finally, participants reported (or estimated) the gift's cost. As a manipulation check, receivers reported how much they liked the gift and givers estimated how much the receiver liked the gift on the same 1 to 9 scale.

Results and Discussion

All relevant means, standard deviations, and correlations within experimental conditions are reported in Table 1.

Manipulation check. The recall manipulation was effective. A 2 (role: giver vs. receiver) × 2 (gift quality: liked vs. disliked) ANOVA on participants' reported liking for the gift itself yielded only a significant main effect for gift quality, $F(1, 95) = 272.65$, $p < .01$, $\eta_p^2 = .74$. Participants reported liking the liked gift significantly more ($M = 8.44$, $SD = 0.94$) than the disliked gift ($M = 3.28$, $SD = 2.02$). No other effects approached significance.

Thoughts and evaluations. We averaged the three positive evaluation measures ($\alpha = .87$) and the two thought measures ($r = .55$) into two composites. A 2 (role: giver vs. receiver) × 2 (gift quality: liked vs. disliked) ANOVA on participants' positive evaluations yielded a significant main effect for gift quality, $F(1, 95) = 62.67$, $p < .01$, $\eta_p^2 = .40$. Receivers who recalled a liked gift reported more positive evaluations ($M = 8.48$, $SD = 1.09$) than those who recalled a disliked gift ($M = 5.39$, $SD = 1.77$), $F(1, 95) = 61.41$, $p < .01$, $\eta_p^2 = .39$. Similarly, givers who recalled a gift liked by the receiver predicted more positive evaluations from receivers ($M = 7.81$, $SD = 1.24$) than those who recalled a gift disliked by the receiver ($M = 6.45$, $SD = 1.41$), $F(1, 95) = 11.52$, $p < .01$, $\eta_p^2 = .11$. This main effect was qualified by a Role × Gift Quality interaction, $F(1, 95) = 9.49$, $p < .01$, $\eta_p^2 = .09$, indicating that the difference in evaluations was larger for receivers than for givers. This interaction was not predicted, does not bear directly on our hypotheses about how much thoughts count in gift exchanges, does not replicate in an identical condition in Experiment 2, and does not replicate in conceptually similar conditions in Experiment 3 and 4. We will therefore not discuss it further.

Our main prediction was that ratings of gift givers' thoughts would be significantly correlated with gift receivers' positive evaluations only when recalling a disliked gift. As predicted, the amount of thought that receivers believed their givers expended was significantly correlated with their positive evaluation of the exchange when recalling a disliked gift, $r(23) = .80$, $p < .01$, but not when recalling a liked gift, $r(23) = -.14$, $p = .50$, $z = 4.11$, $p < .01$. Controlling for the perceived cost of the gift did not alter the results meaningfully (rs = .79 & −.18, disliked vs. liked, respectively, $p < .01$ & $p = .39$, $z = 4.16$, $p < .01$).

We also predicted that gift givers would fail to anticipate correctly when their thoughts counted in receivers' evaluations and when they did not. Consistent with this prediction, gift givers showed a very different pattern of expected evaluations than gift receivers. In particular, gift givers believed that their thoughts would count—that is, would increase a receivers' positive evaluations—when giving a liked gift, $r(25) = .47$, $p = .01$, and showed a similar but nonsignificant pattern when giving a disliked gift, $r(20) = .28$, $p = .20$, $z < 1$. Controlling for the cost of the gift did not alter the results meaningfully (rs = .41 & .13, liked vs. disliked, respectively, ps = .04 & .56, $z < 1$.). Gift givers expected their thoughts to count when giving a good gift but to count somewhat less when giving a bad gift. For receivers, it was precisely the opposite.

One alternative interpretation is that a gift giver's thoughts did not count when receiving a good gift because of a ceiling effect or a restricted range. That is, liked gifts were evaluated so positively that there was not enough variability in our measures for a positive correlation to emerge, whereas disliked gifts were evaluated more negatively with enough sensitivity and variability around the mean for a significant correlation to emerge. This alternative posits that the correlations in these two distributions do not actually differ significantly from each other, but that the correlation in the liked gift condition is suppressed because of censored data at the ceiling of the positive evaluation or thoughts measures.

Indeed, we have markedly more evaluations at ceiling among gift receivers in the liked gift condition (72.0%) than in the disliked gift condition (0.0%), and more than among gift givers in both the liked gift (44.4%) and disliked gift (13.6%) conditions. This result alone is not inconsistent with our account. Our prediction is that gift receivers do not consider a gift giver's thoughts when receiving a good gift. This could produce positive evaluations at the ceiling regardless of gift receivers' presumed thoughts. However, the result would be entirely inconsistent with our account if these ceiling effects are masking a strong positive corre-

Table 1
Average Positive Evaluations, Thoughts, and the Correlation Between Evaluations and Thoughts for Receivers and Givers Evaluating Liked Versus Disliked Gifts (Experiment 1)

	Liked gift			Disliked gift		
Group	Positive evaluation	Thoughts	r	Positive evaluation	Thoughts	r
Receivers	8.48 (1.09)	7.50 (1.84)	−.14	5.39 (1.77)	5.50 (2.29)	.80*
Givers	7.81 (1.24)	7.52 (1.60)	.47*	6.45 (1.41)	6.14 (1.75)	.28

Note. Values in the parentheses are standard deviations. Asterisks indicate correlations significantly larger than zero. $p < .05$.

lation between thoughts and evaluations that could emerge with more sensitive measures in the liked gift condition.

We assessed the validity of this alternative interpretation in three different ways. We address each of these ways concisely here but provide a full description of all of our additional analysis and the scatterplots for all 12 conditions in both Experiments 1 and 2 in the supplemental online materials.

First, we assessed whether the range restrictions in our data are so severe for liked gifts among receivers that a positive correlation could not even theoretically emerge in that condition. To do so, we calculated the maximum possible correlation between positive evaluations and thoughts by breaking the dependence of the two measures within participants and then rank ordering the observations on each measure to identify the maximum positive and negative correlations. Given the data, a large positive correlation was possible in both the liked gift ($r = .95$, $p < .01$) and disliked gift ($r = .95$, $p < .01$) conditions among receivers, suggesting that it is possible to achieve a highly significant correlation for liked gifts, even when a large number of data points are at ceiling. The maximum negative correlation for gift receivers was smaller in the liked gift condition ($r = -.41$, $p < .05$) than in the disliked gift condition ($r = -.95$, $p < .01$). At the very least, a meaningful positive correlation was possible for gift receivers recalling liked gifts given the range of evaluations we observed.

Second, the alternative interpretation based on a ceiling effect makes one prediction that our account does not. In particular, it predicts that if one was able to raise the ceiling on either the positive evaluation or thoughts measures, then a positive correlation would emerge. A Tobit regression is designed for exactly this situation in which some data points are censored by ceiling (or floor) effects by treating data points at the ceiling as the maximum value *or greater*. This analysis conceptually lifts the ceiling on a potentially truncated distribution. Conducting Tobit regressions on our data did not support the existence of a suppressed positive correlation for gift receivers evaluating a liked gift. As before, the amount of thought that receivers believed their givers expended was significantly correlated with their positive evaluation of the exchange when recalling a disliked gift ($B = .55$, $p < .001$), but there was still no significant relationship between thoughts and positive evaluation when receivers recalled a liked gift ($B = -.38$, $p = .41$). The directionally negative relationship for liked gifts among gift receivers further suggests that no positive correlation is suppressed by a ceiling effect. For gift givers, Tobit regressions again suggest patterns very different from gift receivers. The amount of thought givers reported expending was not significantly correlated with their predicted positive evaluations when recalling a disliked gift ($B = .23$, $p = .17$), but was significantly correlated when recalling a liked gift ($B = .43$, $p = .02$).

Another way to increase the ceiling on our measures would be to log-transform the data to normalize a skewed distribution. Because our data are negatively skewed, with a relatively large percentage of receivers rating either thoughts or positive evaluations at ceiling, we conducted the log transformation on the two measures by first reflecting the data, adding 10 to bring the lowest possible value to 1, taking the log, reflecting back to the original order of the measure, and adding another constant to ensure the transformed data were positive (that is, $Measure_{\text{transformed}} = -\log(-Measure_{\text{original}} + 10) + \text{constant}$). Normalizing the data in this way did not meaningfully alter the correlation between thoughts and positive evaluation for gift receivers in the liked gift condition, $r(23) = -.05$, $p = .80$.

Third, the alternative interpretation based on a ceiling effect makes a second prediction that our account does not. In particular, it predicts that the distribution for disliked gifts would look the same as it does for liked gift if it, too, was subject to a ceiling effect. We tested this possibility by *lowering* the ceiling on the positive evaluation and thoughts measures for disliked gifts to see if artificially creating a ceiling effect in these conditions produces results similar to those in the liked gift conditions. We truncated the distribution for gift receivers in the disliked condition in many different ways, from the most mild truncation that matches the number of data points at the most extreme points of the distribution in the liked gift condition, to a more severe truncation that matches the percentage of observations at ceiling in the liked gift condition, to even more severe truncations than we actually observe in the liked gift condition. At no point does the correlation we observe in the disliked condition resemble the correlation of $-.14$ that we observe in the liked gift condition. The lowest correlation we observe is .64, with a very severe truncation that censors 80% of the observations on the positive evaluation measure and 76% of the evaluations on the thoughts measure (far more censoring than we actually observe among gift receivers in the liked gift condition). All other truncations yield stronger positive correlations. None of these results suggest a suppressed correlation resulting from ceiling effects or range restrictions.

Experiment 2: Activating Thoughts

Collectively, we believe the results from Experiment 1 suggest that thoughts "count" for gift receivers only when they are triggered to think of a gift giver's thoughts. However, the results of Experiment 1 are also consistent with an alternative interpretation based on the application of mental state inferences rather than on their activation (e.g., Gilbert & Hixon, 1991). It could be that gift receivers actively considered a gift giver's thoughts when receiving a good as well as a bad gift in Experiment 1, but only applied those inferences to their evaluations when considering a bad gift. Simply measuring the correlation between inferences about a gift giver's thoughts and evaluations of the exchange cannot distinguish between the activation of mental state inferences and the application of those inferences in judgment.

Unlike the application account, our activation account suggests that a gift giver's presumed thoughts do not influence gift receivers' evaluations because they are less accessible than the objective quality of the gift itself, not because gift receivers think a giver's thoughts are irrelevant to their evaluations. Indeed, the pretest data we reported in the introduction suggests that people, on average, have a strong theory that gifts chosen thoughtfully will be appreciated above and beyond the objective quality of the gift itself. These data suggest that a gift giver's thoughtfulness is indeed seen as applicable to a gift receiver's appreciation and gratitude.

To test whether or not the results from Experiment 1 stemmed from the activation of mental state inferences, we manipulated whether or not a gift giver's thoughts were activated when receivers evaluated the gift exchange. We did so in Experiment 2 by asking one group of gift receivers to evaluate a gift giver's thoughts *before* reporting their appreciation and gratitude and another group to do so *after* reporting their appreciation and

gratitude. If a gift giver's thoughts count only when inferences about them are activated at the time of evaluation, then gift receivers' thoughts should count for both liked as well as disliked gifts when people are led to think of them *before* reporting appreciation and gratitude, but should only count for disliked gifts when receivers are led to think of them *after* reporting appreciation and gratitude. If, however, the results of Experiment 1 were produced by differences in the application of a gift receiver's thoughts in evaluations of appreciation and gratitude, then a gift giver's thoughts should count only for disliked gifts regardless of whether the gift receiver was asked to think about a gift giver's thoughts before or after evaluating appreciation and gratitude.

Method

A total of 161 MSI visitors (91 female, M_{age} = 39.5 years) volunteered to participate in the experiment in exchange for a small prize. Experiment 2 was identical to Experiment 1, except that we manipulated the order of the questions about a gift giver's thoughts and the questions about positive evaluations of the exchange. Participants were randomly assigned to the role of giver or receiver and then recalled either a liked gift or a disliked gift. After that, participants randomly assigned to the thought-first condition evaluated the thought items before the positive evaluation items, whereas those in the thought-last condition answered the items in the opposite order (as in Experiment 1).

Results and Discussion

All relevant means, standard deviations, and correlations within experimental conditions are reported in Table 2.

Manipulation checks. As in Experiment 1, the recall manipulation was effective. A 2 (role: giver vs. receiver) × 2 (gift quality: liked vs. disliked) × 2 (order: thought first vs. thought last) ANOVA on participants' reported liking for the gift itself yielded only a significant main effect for gift quality, $F(1, 153) = 311.11$, $p < .01$, $\eta_p^2 = .67$. Participants reported liking the liked gift significantly more ($M = 8.44$, $SD = 1.20$) than the disliked gift ($M = 3.73$, $SD = 2.26$). No other effects approached significance.

Thoughts and evaluations. Because they were highly correlated, we again averaged the three appreciation measures ($\alpha = .90$) and the two thought measures ($r = .66$) into two composites. A 2 (role: giver vs. receiver) × 2 (gift quality: liked vs. disliked) × 2 (order: thought first vs. thought last) ANOVA on participants' positive evaluations yielded a significant main effect for gift quality, $F(1, 153) = 184.88$, $p < .01$, $\eta_p^2 = .55$, and a marginally significant main effect for role, $F(1, 153) = 3.49$, $p = .06$, $\eta_p^2 = .02$. Givers expected more positive evaluations when recalling a liked gift ($M = 7.89$, $SD = 1.49$) than when recalling a disliked gift ($M = 4.72$, $SD = 1.76$), $F(1, 153) = 84.11$, $p < .01$, $\eta_p^2 = .35$, and gift receivers provided a similar pattern ($M_{liked} = 8.47$, $SD = 0.80$ vs. $M_{disliked} = 5.03$, $SD = 1.84$), $F(1, 153) = 101.34$, $p < .01$, $\eta_p^2 = .40$, but were slightly more positive overall than gift givers. No other effects approached statistical significance.

Most important again were the correlations within conditions between the thought and positive evaluation measures. Results from the thought-last condition replicated the main findings of Experiment 1. Specifically, the amount of thought that receivers believed their givers expended was significantly correlated with their positive evaluations (appreciation, gratitude, and liking of the gift giver) when recalling a disliked gift, $r(19) = .59$, $p < .01$, but not when recalling a liked gift, $r(17) = .01$, $p = .95$, $z = 1.95$, $p = .05$, suggesting that a giver's thoughts only counted by increasing a receiver's appreciation for bad gifts but not for good gifts. Controlling for the perceived cost of the gift did not alter the results meaningfully (rs = .57 & −.004, disliked vs. liked, respectively, $p < .01$ & $p = .99$, $z = 1.90$, $p = .06$).

For gift givers, however, the evaluation of their own thoughts was not correlated with their predicted positive evaluations from receivers either when recalling a disliked gift, $r(19) = -.14$, $p = .54$, or a liked gift, $r(18) = .29$, $p = .24$, $z = 1.30$, *ns*. Controlling for the cost of the gift did not meaningfully alter the results (rs = −.14 & .25, disliked vs. liked, respectively, ps = .57 & .31, $z = 1.17$, *ns*). Again, thoughts only counted for those who received bad gifts, an effect not anticipated by gift givers.

A very different pattern of correlations emerged among participants in the thought-first condition. In this condition, the amount of thought that receivers believed their givers expended was significantly correlated with their positive evaluation for both disliked gifts, $r(20) = .61$, $p < .01$, and liked gifts, $r(17) = .62$, $p < .01$, $z < 1$, suggesting that a giver's thoughts counted by increasing a receiver's appreciation for both disliked and liked gifts. Controlling for the cost of the gift did not meaningfully alter the results (rs = .70 & .63, disliked vs. liked, respectively, ps < .01, $z < 1$). Interestingly, a roughly similar pattern emerged for gift givers as well: the amount of thought givers expended was positively cor-

Table 2
Average Positive Evaluations, Thoughts, and the Correlation Between Evaluations and Thoughts for Receivers and Givers Evaluating Liked Versus Disliked Gifts Among Those Who Considered a Gift Giver's Thoughts First Versus Last (Experiment 2)

	Liked gift			Disliked gift		
Condition/group	Positive evaluation	Thoughts	*r*	Positive evaluation	Thoughts	*r*
Thoughts first						
Receivers	8.32 (0.98)	7.58 (1.38)	.62*	4.83 (1.88)	4.95 (2.13)	.61*
Givers	7.84 (1.86)	7.74 (1.28)	.49*	4.32 (1.39)	6.18 (1.92)	.35
Thoughts last						
Receivers	8.62 (0.58)	7.68 (1.08)	.01	5.24 (1.82)	5.07 (2.45)	.60*
Givers	7.93 (1.07)	7.78 (1.04)	.29	5.10 (2.00)	6.93 (1.76)	−.14

Note. Values in the parentheses are standard deviations. Asterisks indicate correlations significantly larger than zero. $p < .05$.

related with their predicted evaluations for both disliked gifts, $r(17) = .35$, $p = .14$, and for liked gifts, $r(17) = .49$, $p = .03$. Controlling for the cost of the gift did not meaningfully alter these results (rs = .28 & .52, disliked vs. liked, respectively, ps = .26 & .03, $z < 1$).

As in Experiment 1, we further assessed whether these patterns of correlations could be produced by a ceiling effect or range restriction. Please see the supplemental online materials for a complete description of these analyses. First, the maximum possible positive correlations given the data we observed were equally high in all experimental conditions (all maximum positive rs $> .9$). Second, conducting Tobit analyses on our data did not alter the results in any meaningful way. Third, normalizing the data by a log transformation did not meaningfully alter the observed correlations. Fourth, creating a ceiling effect in the distribution for gift receivers recalling a disliked gift in the thought-last condition did not reduce the correlation to the level we observed among gift receivers recalling a liked gift in the thought-last condition. Most important, however, the significantly different pattern of correlations among gift receivers evaluating liked gifts in the thought-first versus thought-last condition was inconsistent with a ceiling effect explanation. When the process of reasoning about a giver's thoughts was activated by triggering gift receivers to think of a gift giver's thoughts, their positive evaluations were correlated with the giver's presumed thoughts. When the process was not explicitly activated, positive evaluations and presumed thoughts were unrelated. The range of responses in these two conditions on the thought measure was nearly identical, but the relation of responses to the positive evaluation of the exchange differed markedly.

We believe these results are interesting for two reasons. First, they are consistent with our theory that thoughts count in gift receivers' evaluations only when mental state inferences are activated. In everyday life, we believe a common trigger for this process is the automatic evaluation of the gift itself. If receivers like a gift, then a giver's thoughts are not considered. But if receivers dislike the gift, then additional cognitive processing of the exchange is activated in order to explain the behavior, leading people to consider the giver's thoughts. Experiment 2 replicated the basic finding from Experiment 1, but also demonstrated that evaluating a gift giver's thoughts explicitly before reporting appreciation and gratitude led gift receivers to take into account a giver's thoughts. These results suggest that a gift giver's thoughts do not count in a gift receiver's evaluations because the receiver fails to consider them at the time of judgment, rather than because the gift receiver thinks that the giver's thoughts are irrelevant or because they are unable to consider them.

Second, these results support our hypothesis that giver and receiver evaluations may naturally diverge because of different perspectives on the gift exchange. When both gift givers and gift receivers were focused on the gift givers' thoughts before reporting or predicting positive evaluations of the exchange, gift givers' thoughts counted at least somewhat for givers and receivers for both liked as well as disliked gifts. When receivers were not asked to consider a gift giver's thoughts before evaluating the exchange, perspectives between givers and receivers diverged. "It's the thought that counts" for gift receivers only when those thoughts are activated at the time of evaluation. Otherwise, it is the quality of the gift that counts first and foremost.

Although these results are consistent with our hypotheses, one caveat deserves mentioning. In Experiment 2, we manipulated whether a gift giver's thoughts were accessible at the time of judgment but measured how inferences about those thoughts were related to evaluations of gratitude and appreciation. We did not measure the activation of mental state inferences directly. It is theoretically possible that gift receivers did consider how much thought a gift giver expended in all conditions, but that those activated inferences did not influence appreciation and gratitude for liked gifts in the thought-last condition. We think this is implausible because it requires an existing theory among gift receivers that a gift giver's thoughts are relevant only for relatively bad gifts. The pretest described in the introduction showed no evidence for such a belief whatsoever. Instead, thoughtful gifts were expected to be appreciated more than thoughtless gifts in all cases. We therefore believe that Experiment 2 is most parsimoniously explained by differences in the activation of mental state inference, but note that a completely conclusive test would require both manipulating *and* measuring the activation of these inferences directly.

The strength of Experiments 1 and 2 is that they utilized evaluations of naturally occurring gifts, thereby providing a reasonable degree of ecological validity. The weakness of Experiments 1 and 2 is that they both relied on memory for gift exchanges rather than on the immediate experience of giving or receiving a gift. It is possible that gifts recalled from memory at a distance are evaluated differently than gifts experienced directly in an online exchange. In particular, it is possible that the relative accessibility of a gift giver's thoughts compared with the objective quality of the gift itself may vary, with a gift giver's thoughts being less accessible to both givers and receivers in memory than they might be immediately. The gifts recalled from memory in Experiments 1 and 2 also varied between the liked and disliked conditions, making it possible that some variable other than the objective quality of the gift (whether it was liked or disliked) is responsible for the differences we observed. It is therefore critical to conduct experiments involving real gift exchanges in which evaluations are measured immediately instead of recalled from memory and in which the actual gifts exchanged do not vary systematically by condition. The final two experiments utilized an experimental procedure designed to meet these goals.

Experiment 3: Actual Gift Exchange

In addition to providing an experimental design that allowed for tighter control and stronger causal inferences, Experiment 3 also provided a further test of our prediction that gift givers' thoughts would count only when mental state reasoning was activated by manipulating whether the gift giver was a friend or stranger. In Experiments 1 and 2, gifts were exchanged between friends or family members, as occurs routinely in everyday life. We presumed people would have relatively strong expectations about getting good gifts from friends but have relatively weaker expectations about getting good gifts from strangers. As a result, we predicted that a bad gift from a friend would activate the process of considering the gift giver's thoughts but that a bad gift from a stranger would not. This friend versus stranger manipulation would provide not only another test of our suggested psychological

mechanism, but also test another important boundary condition for when thoughts count in gift exchanges.

To test our critical assumption that gift receivers expect better gifts from friends than from strangers, we recruited 44 visitors to the MSI in Chicago to participate in a short experiment. Each participant named a close friend and then reported the extent to which they expected to like a birthday gift from this friend on a scale ranging from −5 (*strongly expect to dislike*) to 5 (*strongly expect to like*) and also how likely this friend would be able to choose a gift that they really liked on a scale ranging from 0 (*not likely at all*) to 10 (*very likely*). Participants did exactly the same for a distant acquaintance, with the order of consideration counterbalanced.

Consistent with our assumption, participants expected to like the gift from a close friend ($M = 3.31$, $SD = 1.62$), but had no consistent expectation for a gift from a distant acquaintance ($M = 0.33$, $SD = 1.88$), $t(43) = 8.42$, $p < .01$, $r = .79$. Participants also predicted that a close friend would be better able to choose a gift that they would like ($M = 8.02$, $SD = 1.82$) than would a distant acquaintance ($M = 4.66$, $SD = 2.29$), $t(43) = 10.04$, $p < .01$, $r = .84$. If mental state reasoning is activated by unexpected events, then it should be activated when a friend gives a disliked gift but not when a stranger does so. And if mental state inferences count only when activated in judgment, then a gift giver's thoughts should count only when a close friend gives a disliked gift.

We tested this prediction by asking MSI visitors to exchange gifts either with an acquaintance or with a stranger. Gift givers selected a gift from a set of five options after seeing an example gift that was either very desirable or very undesirable to make the actual gifts seem relatively bad or good by comparison. We expected, consistent with evidence on the importance of comparison standards in judgment (e.g., Mussweiler, 2003), that our participants would like the actual gifts more after considering the undesirable example gift than after considering the desirable example gift. In this way, the very same gift could be considered desirable for some but undesirable for others.

To manipulate the amount of thought expended on the gift, we asked gift givers in the thoughtful condition to choose a gift carefully and with a lot of thought, but asked gift givers in the thoughtless condition to choose randomly without any thought at all. Receivers were told how givers were instructed to choose the gift in detail, mirroring the gift giver's instructions precisely.

To investigate whether this particular paradigm leads people to believe a giver's thoughts count, we conducted another pilot experiment with 47 MSI visitors. After having Experiment 3's procedure explained in detail and viewing the good or bad example gift, these visitors acted as observers and predicted the receivers' appreciation and gratitude after receiving a thoughtful versus thoughtless gift from a friend versus stranger (on 0 to 10 scales, averaged into a composite, $r = 72$). As shown in Figure 1, a 2 (gift: good vs. bad) × 2 (thought: yes vs. no) × 2 (relationship: acquaintance vs. stranger) mixed-model ANOVA on positive evaluation revealed a main effect of thought, $F(1, 45) = 30.69$, $p < .01$, $\eta_p^2 = .41$. Specifically, these observers believed that a gift giver's thoughts would count, increasing receivers' appreciation and gratitude regardless of whether the gift was good or bad by comparison or whether the gift was coming from an acquaintance or a stranger ($M_{\text{thoughtful}} = 8.28$, $SD = 1.96$ vs. $M_{\text{thoughtless}} = 6.76$, $SD = 2.31$). The observers also believed that gifts from an acquaintance would be appreciated more than gifts from a stranger, ($M_{\text{acquaintance}} = 7.96$, $SD = 2.32$ vs. $M_{\text{stranger}} = 7.08$, $SD = 2.14$), $F(1, 45) = 8.29$, $p < .01$, $\eta_p^2 = .16$, but this main effect did not interact with the gift givers' thoughts, $F(1, 45) = 2.89$, *ns*. The conventional wisdom—"It's the thought that counts"—appears alive and well in this gift exchange paradigm.

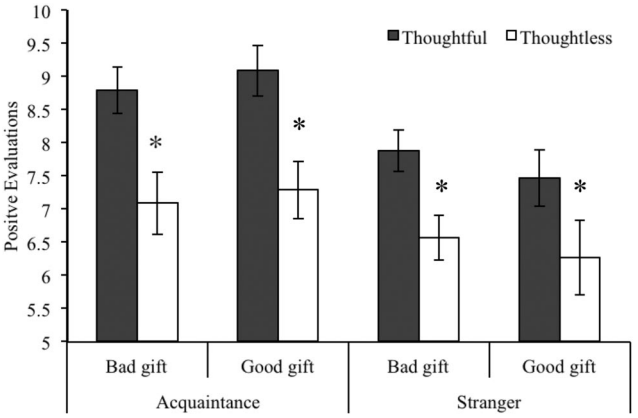

Figure 1. Observers' predicted positive evaluations (appreciation and gratitude, combined) when givers chose a good gift or a bad gift either thoughtfully or thoughtlessly for an acquaintance or a stranger (pretest to Experiment 3). Asterisks indicate a statistically significant simple effect ($p < .05$) between thoughtful and thoughtless conditions. Error bars report standard deviations.

Method

MSI visitors ($N = 336$, 171 female, $M_{\text{age}} = 33.3$ years) were first randomly assigned to participate with either an acquaintance (a friend or family member attending the museum with them) or a stranger (also attending the museum, but with another group of friends or family members). Participants were then randomly assigned within the pair to be either a gift giver or gift receiver and then led to separate rooms within the museum.

To manipulate whether a gift seemed relatively good or bad, we first showed both givers and receivers either a very desirable example gift (a "Newton's Cradle") or a very undesirable gift (a small wooden ruler). These gifts were rated the most and least desirable, respectively, out of eight gifts in a pretest we conducted with 60 MSI visitors. The experimenter then explained that this gift was merely an example of the kinds of gifts we used in the experiment, but that it was not being used on that particular day. Gift givers were then shown a set of five gifts to choose from (a tote bag, a deck of cards, a big pen, a MSI keychain, and a MSI magnet) of roughly equivalent desirability (based on the pretest of 60 MSI visitors). We expected that these gifts would seem relatively bad compared with the desirable example gift, but relatively good compared with the undesirable example gift.

Gift givers were then instructed to choose a gift for the receiver. Givers in the thoughtful condition were asked to think very carefully about their choice, to put a lot of thought into choosing what the receiver would like most. Givers in the thoughtless condition were told that they should not think hard about their choice and that it was fine to even choose randomly. Receivers were told

about the giver's choice instructions in detail, using the same instructions given to the givers.

After the experimenter delivered the chosen gift to the receiver, givers reported how appreciative and grateful the receiver would feel, whereas receivers reported how appreciative and grateful they actually felt. Both groups used scales ranging from 0 (*not at all*) to 10 (*very much*). As a manipulation check, givers reported how much thought they expended and how much they cared whether the receiver would like the gift on the same 0 to 10 scale. Gift receivers answered the same two questions in terms of how much thought and care they believed givers expended. Receivers also reported how much they liked the actual gift and how much they liked the example gift, whereas givers predicted how much receivers would like the actual gift and the example gift. Finally, both givers and receivers estimated the cost of the actual gift.

Results and Discussion

Manipulation check. Both givers and receivers liked the good example gift significantly better than the bad example gift. A complete 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) × 2 (gift quality: good vs. bad) × 2 (relationship: acquaintance vs. stranger) mixed-model ANOVA with role treated as a repeated measure returned a significant main effect for example gift, $F(1, 160) = 125.23$, $p < .01$, $\eta_p^2 = .44$. Specifically, givers liked the Newton's Cradle ($M = 6.15$, $SD = 1.82$) more than the ruler ($M = 2.89$, $SD = 1.59$), $F(1, 160) = 154.34$, $p < .01$, $\eta_p^2 = .49$. Receivers also liked the Newton's Cradle ($M = 6.31$, $SD = 2.07$) better than the ruler ($M = 4.18$, $SD = 2.56$), $F(1, 160) = 34.41$, $p < .01$, $\eta_p^2 = .18$. This main effect was qualified by a Gift Quality × Role interaction, $F(1, 160) = 7.41$, $p < .01$, $\eta_p^2 = .04$, indicating that the difference in example gift evaluations was larger for givers than for receivers. No other effects approached significance.

A similar ANOVA on liking of the actual gift also produced a significant main effect for gift, $F(1, 160) = 15.35$, $p < .01$, with the actual gift rated more favorably in the good gift condition (i.e., compared with the negatively rated ruler) than in the bad gift condition (i.e., compared with the positively rated Newton's Cradle). This result, however, was qualified by a Gift Quality × Role interaction, $F(1, 160) = 8.81$, $p < .01$. This indicates that receivers liked the actual gift significantly more in the good gift condition ($M = 7.10$, $SD = 1.95$) than in the bad gift condition ($M = 5.73$, $SD = 2.07$), $F(1, 160) = 19.38$, $p < .01$, but that the difference in predicted liking by givers was nonsignificant between the good gift condition ($M = 6.13$, $SD = 1.54$) and the bad gift condition ($M = 5.87$, $SD = 1.59$), $F(1, 160) = 1.14$, $p = .28$. Gift receivers' liking of the actual gift was more sensitive to the comparison gift than were gift givers' predictions of the receivers' liking.

The thought manipulation was clearly effective. We averaged the two thought measures into a composite because they were highly correlated ($r = .59$). A 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) × 2 (gift quality: good vs. bad) × 2 (relationship: acquaintance vs. stranger) mixed-model ANOVA returned a significant main effect for thought, $F(1, 160) = 22.55$, $p < .01$, $\eta_p^2 = .12$. As intended, givers in the thoughtful condition reported that they invested more thought ($M = 6.28$, $SD = 1.86$) than did givers in the thoughtless condition ($M = 5.14$, $SD = 2.39$), $F(1, 160) = 19.52$, $p < .01$, $\eta_p^2 = .11$. Also as intended, receivers were equally aware of this difference ($F_{\text{interaction}} < 1$). Receivers in the thoughtful condition believed that givers expended more thought ($M = 5.99$, $SD = 2.04$) than did receivers in the thoughtless condition ($M = 4.95$, $SD = 2.29$), $F(1, 160) = 8.21$, $p < .01$, $\eta_p^2 = .05$.

Positive evaluations. We averaged ratings of appreciation and gratefulness into a single positive evaluation composite ($r = .68$). As predicted, gift exchanges between acquaintances replicated the main finding from the prior experiments. A 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) × 2 (gift quality: good vs. bad) mixed-model ANOVA on positive evaluations yielded a main effect of role, $F(1, 79) = 13.71$, $p < .01$, $\eta_p^2 = .15$, and a main effect of thought, $F(1, 79) = 4.00$, $p < .05$, $\eta_p^2 = .05$, qualified by a three-way interaction, $F(1, 79) = 5.08$, $p < .05$, $\eta_p^2 = .05$. As shown in Figure 2, the givers' thoughts had no influence on receivers' evaluations for a relatively good gift ($M_{\text{thoughtful}} = 7.07$, $SD = 1.38$ vs. $M_{\text{thoughtless}} = 7.47$, $SD = 1.42$), $F < 1$. However, the givers' thoughts influenced receivers' appreciation and gratitude when receivers were given a relatively bad gift ($M_{\text{thoughtful}} = 7.38$, $SD = 1.03$ vs. $M_{\text{thoughtless}} = 5.74$, $SD = 2.11$), $F(1, 79) = 11.99$, $p < .01$, $\eta_p^2 = .13$, $F_{interaction}(1, 79) = 9.19$, $p < .01$,

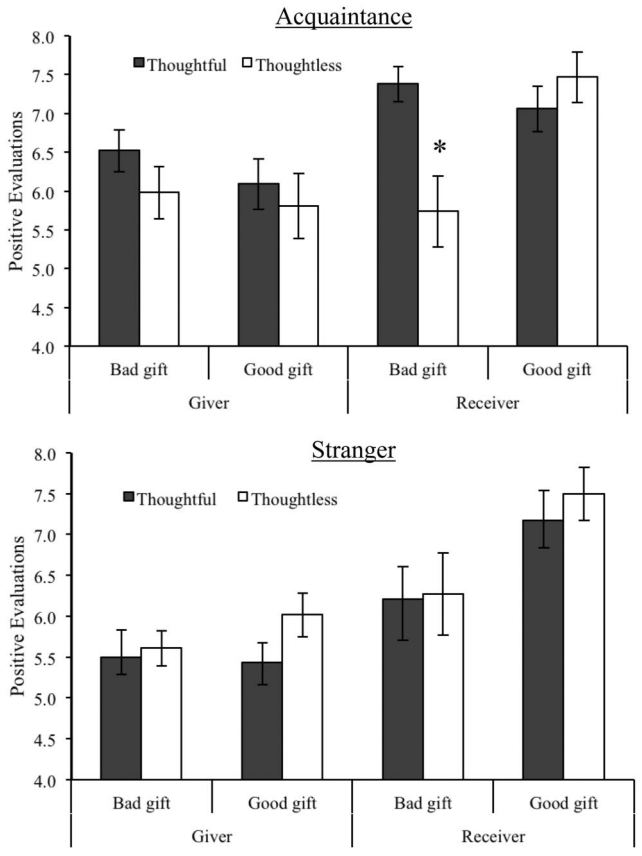

Figure 2. Expected and actual positive evaluations (appreciation and gratitude, combined) when givers chose a good gift or a bad gift either thoughtfully or thoughtlessly for an acquaintance (top panel) or a stranger (bottom panel; Experiment 3). The asterisk indicates a statistically significant simple effect ($p < .05$) between the thoughtful and thoughtless conditions. Error bars report standard deviations.

$\eta_p^2 = .10$. As before, gift givers' thoughts only counted for receivers when they received a relatively bad gift.

Among givers, in contrast, no significant main effects or interactions emerged, $Fs < 1.5$, *ns*. As in the prior experiments, givers did not anticipate correctly when their thoughts would influence receivers' appreciation and gratitude. In addition, a main effect of role indicates that givers systematically underestimated receivers' positive evaluations ($M_{giver} = 6.11$, $SD = 1.54$ vs $M_{receiver} = 6.90$, $SD = 1.66$), $F(1, 79) = 13.71$, $p < .01$, $\eta_p^2 = .15$, replicating prior research (Flynn & Brockner, 2003). This misprediction was especially large for good gifts chosen thoughtlessly ($M_{giver} = 5.82$, $SD = 1.83$ vs. $M_{receiver} = 7.47$, $SD = 1.42$), $F(1, 20) = 7.33$, $p = .01$, $\eta_p^2 = .27$.

When participants received a gift from a stranger, in contrast, only the quality of the gift counted. Participants reported or predicted significantly more appreciation and gratitude when receiving a relatively good gift ($M = 6.53$, $SD = 1.62$) than when receiving a relatively bad gift ($M = 5.90$, $SD = 1.75$), $F(1, 81) = 6.69$, $p = .01$, $\eta_p^2 = .08$. This main effect was qualified by a marginally significant Role × Gift interaction, $F(1, 81) = 3.57$, $p = .06$, $\eta_p^2 = .04$, indicating that the quality of the gift had a somewhat larger effect on receivers' evaluations ($M_{good} = 7.33$, $SD = 1.59$ vs. $M_{bad} = 6.24$, $SD = 2.09$), $F(1, 81) = 7.16$, $p = .009$, $\eta_p^2 = .08$, than it did on givers' predicted evaluations ($M_{good} = 5.73$, $SD = 1.20$ vs. $M_{bad} = 5.56$, $SD = 1.27$), $F < 1$, *ns*. Once again, the receivers also reported more gratitude and appreciation ($M = 6.78$, $SD = 1.93$) than givers expected ($M = 5.64$, $SD = 1.23$), $F(1, 81) = 22.16$, $p < .01$, $\eta_p^2 = .21$. No other significant main effects or interactions emerged for either givers or receivers. Most relevant for our hypotheses, a gift giver's thoughts did not count for gifts exchanged between strangers.

As in Experiments 1 and 2, putting careful thought into a gift influenced receivers' evaluations only when we predicted that receivers would be triggered to activate the psychological process of considering a giver's thoughts, namely, when a friend gave them a relatively bad gift. As before, givers could not predict this correctly. The common wisdom that "it's the thought that counts" expressed by the observers in the pilot test for this experiment was both miscalibrated and mispredicted. A gift giver's thoughts did not count in any measureable way when participants received a good gift from anyone or a relatively bad gift from a stranger.

Experiment 4: Thoughts Count for Gift Givers

Gift receivers need to be triggered to think about a gift giver's thoughts in order for those thoughts to count in receivers' evaluations following the exchange. Gift givers, however, experience their thoughtfulness directly and may therefore be influenced in unique ways by the thought they put into a gift. Although the common wisdom that "it's the thought that counts" typically refers to a receiver's evaluations, existing psychological evidence led us to predict that thoughts would count for gift givers by influencing how connected they feel to the receiver. Thinking carefully about another person can serve as a self-perception cue that makes a person infer that he or she is more connected to someone else (Bem, 1972). It also involves considering another's perspective carefully. Perspective taking tends to increase how closely connected people feel to others (Davis et al., 1996; Galinsky et al., 2005). We therefore predicted that gift givers would feel closer to receivers after putting careful thought into a gift than after choosing randomly. But because gift receivers do not experience the effort of expending thoughts or the process of perspective taking, we predicted that a gift giver's thoughts would have no effect on a gift receiver's feelings of social connection. Because we expected that people would already feel very close to their friends and family members, making it difficult to detect any experimentally induced increase in social connection, we only investigated gift exchanges between strangers in Experiment 4. Our theory makes no prediction about how the quality of the gift would influence a gift giver's sense of connection, so we did not manipulate whether the gift seemed relatively good or bad as in Experiment 3.

Method

One hundred-fifty MSI visitors (93 female, one did not indicate gender, $M_{age} = 35.9$ years) participated in a procedure identical to Experiment 3 except that no comparison gift was used and that all participants were paired with a stranger. After a brief introductory meeting, participants were randomly assigned to be givers or receivers and then separated into different rooms. Givers were instructed to choose their gift either carefully or randomly, and receivers were told in detail how givers were asked to choose.

After receiving their gift, receivers reported how much they appreciated the gift, how grateful they felt, and how much they liked the gift giver. Gift givers, in contrast, reported how much appreciation, gratitude, and liking they expected the receiver of their gift would feel. Both participants also reported their feelings of social connection to their partner: how connected they felt, how similar they felt, and how much they felt they understood the other's likes and dislikes. Gift givers also predicted how connected their partner felt to them on the same measures. All responses were made on scales ranging from 0 (*not at all*) to 10 (*very much*). Item order (appreciation vs. connection) was counterbalanced but did not influence any of the following analyses. As a manipulation check, givers reported how much thought they expended and how much they cared whether the receiver would like the gift on the same 0 to 10 scale. Gift receivers answered the same two questions in terms of how much thought and care they believed givers expended.

Results and Discussion

Manipulation check. The thought manipulation appeared to be effective. We averaged the two thought measures into a single composite because they were highly correlated ($r = .62$). A 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) mixed-model ANOVA on the thought composite showed that the estimated amount of thought was higher in the thoughtful condition than in the thoughtless condition, $F(1, 73) = 42.07$, $p < .01$, $\eta_p^2 = .37$. Specifically, givers reported that they expended more thoughts when asked to choose thoughtfully ($M = 7.89$, $SD = 1.10$) than when asked to choose thoughtlessly ($M = 4.76$, $SD = 2.20$), $F(1, 73) = 59.70$, $p < .01$, $\eta_p^2 = .45$. Receivers were well aware of this difference, reporting that gift givers expended significantly more thought in the thoughtful condition ($M = 6.22$, $SD = 1.97$) than in the thoughtless condition ($M = 5.07$, $SD = 2.49$), $F(1, 73) = 4.90$, $p = .03$, $\eta_p^2 = .06$. This main effect was qualified by a significant

interaction between role and amount of thought, $F(1, 73) = 8.97$, $p < .01$, $\eta_p^2 = .11$, indicating that the difference between the thoughtful and thoughtless conditions was larger for givers than for receivers.

Positive evaluation. We averaged the three positive evaluation measures (appreciation, gratitude, and liking) into a single composite ($\alpha = .86$). A 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) mixed-model ANOVA returned a significant main effect of role, $F(1, 73) = 38.48$, $p < .01$, $\eta_p^2 = .35$, qualified by a predicted interaction between role and thought, $F(1, 73) = 6.27$, $p = .01$, $\eta_p^2 = .08$. As shown in Figure 3, the amount of thought gift givers expended did not influence receivers' positive evaluation in any way ($M_{\text{thoughtful}} = 7.22$, $SD = 1.86$ vs. $M_{\text{thoughtless}} = 7.39$, $SD = 1.59$), $F < 1$. Gift givers, however, expected a more positive evaluation when they chose thoughtfully ($M = 6.23$, $SD = 1.79$) than when they chose thoughtlessly ($M = 5.06$, $SD = 1.90$), $F(1, 73) = 7.44$, $p < .01$, $\eta_p^2 = .09$. Givers in this experiment, like givers in previous experiments, did not predict correctly when their thoughts would count.

An analysis on the main effect of role again demonstrated that givers underestimated receivers' positive evaluations ($M_{\text{giver}} = 5.63$, $SD = 1.93$ vs. $M_{\text{receiver}} = 7.30$, $SD = 1.72$), both for thoughtful gifts ($M_{\text{giver}} = 6.23$, $SD = 1.79$ vs. $M_{\text{receiver}} = 7.22$, $SD = 1.86$), $F(1, 73) = 6.75$, $p = .01$, $\eta_p^2 = .08$, and for thoughtless gifts ($M_{\text{giver}} = 5.06$, $SD = 1.90$ vs. $M_{\text{receiver}} = 7.39$, $SD = 1.59$), $F(1, 73) = 38.42$, $p < .01$, $\eta_p^2 = .34$.

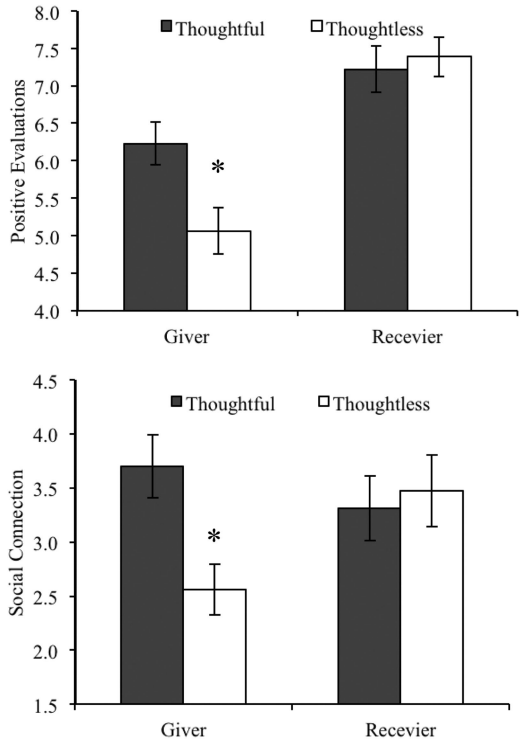

Figure 3. Ratings of positive evaluations (top panel) and social connection (bottom panel) by givers and receivers when a gift was chosen thoughtfully or thoughtlessly (Experiment 4). Asterisks indicate a statistically significant simple effect between the thoughtful and thoughtless conditions. Vertical lines within and above the bars indicate standard deviation.

Social connection. We averaged the three social connection measures (connection, similarity, and preference) into a single composite ($\alpha = .74$). A 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) mixed-model ANOVA on the connection composite measure returned a significant interaction between role and thought, $F(1, 73) = 4.24$, $p < .05$, $\eta_p^2 = .05$. As predicted, givers felt closer to receivers when they chose thoughtfully ($M = 3.70$, $SD = 1.79$) than when they chose thoughtlessly ($M = 2.62$, $SD = 1.44$), $F(1, 73) = 8.29$, $p < .01$, $\eta_p^2 = .10$. However, the givers' thoughts had no influence on the receivers' feelings of connection ($M_{\text{thoughtful}} = 3.31$, $SD = 1.81$ vs. $M_{\text{thoughtless}} = 3.41$, $SD = 2.08$), $F < 1$. Thoughts did indeed count in this experiment, but for the person having the thoughts rather than the person receiving the gift.

Gift givers would appear to be surprised that receivers did not feel closer to them when they chose carefully. A 2 (role: giver vs. receiver) × 2 (thought: yes vs. no) mixed-model ANOVA comparing a receiver's connection predicted by givers and a receiver's actual connection returned a significant interaction between role and thought, $F(1, 73) = 3.59$, $p = .06$, $\eta_p^2 = .05$. Givers predicted that the receivers' evaluations would match their own, expecting that receivers would feel more socially connected when they chose thoughtfully ($M = 3.38$, $SD = 1.94$) than when they chose thoughtlessly ($M = 2.34$, $SD = 1.61$), $F(1, 73) = 6.24$, $p = .01$, $\eta_p^2 = .08$. Receivers, however, did not feel significantly more connected to givers who chose thoughtfully ($M = 3.31$, $SD = 1.81$) than to givers who chose thoughtlessly ($M = 3.41$, $SD = 2.08$), $F < 1$. Gift givers expected that their thoughtfulness would also make receivers feel more socially connected. They did not.

General Discussion

Choosing the perfect gift is difficult, involving costs of time, thought, and money. Gifts are meant to be liked but also appreciated in a way that strengthens a relationship. Putting a lot of thought into getting just the right gift would seem to count considerably for receivers and increase appreciation, because such thoughts signal affection from the giver. It seems, after all, that "it's the thought that counts." Our experiments suggest that this common wisdom is exaggerated, mispredicted, and misplaced.

Exaggerated: The (Non)automaticity of Mental State Inferences

The problem with counting thoughts in a gift exchange—or in any other social context—is that thoughts are inherently invisible. Because people do not have direct access to others' thoughts, considering them requires attention, effort, and motivation (Lin, Keysar, & Epley, 2010). In the absence of any trigger to activate mental state inferences, others' thoughts are likely to remain unconsidered and therefore have no impact on social evaluations. In our experiments, a giver's thoughts counted in receivers' evaluations only when a friend gave a bad gift, the only condition we believed would trigger receivers to consider a gift giver's thoughts. We observed this pattern both when gifts varied naturally in the good versus bad gift conditions (Experiment 1 and 2) and when gifts were held constant and manipulated only by comparison (Experiment 3). In this way, the adage of "it's the thought that

counts" is exaggerated. Thoughts count only in particular circumstances and count for nothing in many others.

We believe these results provide an important theoretical contribution to the theory of mind literature by demonstrating an important moderator of its impact in social interactions. These results suggest that mental state inference, or theory of mind reasoning, is not automatic or even primary in social judgments, but instead must be activated by the social context. This evidence comes from a novel paradigm for this literature in which we measured the extent to which inferences about another person's thoughts were related to other consequential social judgments, in this case to feelings of appreciation and gratitude. Our procedure follows the logic of a classic experiment (Strack, Martin, & Schwarz, 1988) in which participants were asked,"How happy you are with your life in general?" and "How many dates did you have last month?" When the happiness question preceded the dating question, the two items were uncorrelated, $r = -.12$. However, when the dating question preceded the happiness question, the correlation was positive and significant, $r = .66$. All participants in this experiment were perfectly able to think about the number of dates they had and also considered this number relevant to their happiness, but this life event did not spring automatically to mind when assessing happiness unless these participants were explicitly led to think of it.

Our results show a similar result for mental state inferences. Although the gift receivers in our experiments were perfectly capable of considering the thought a gift giver put into his or her gift and applying the information to their evaluations, they did not do so unless they were triggered to do so either implicitly (Experiments 1 and 3) or explicitly (Experiment 2). In Experiment 2, we demonstrated this most clearly by adopting the Strack et al. (1988) procedure. When gift receivers reported their appreciation and gratitude *before* considering a gift giver's thoughts, there was no relationship between these measures when recalling a good gift. But when gift receivers reported their appreciation and gratitude *after* being asked to consider a gift giver's thoughts, there was a strong relationship between a gift giver's thoughts and a receiver's positive evaluations. Although this procedure does not measure the activation of mental state reasoning directly, it does allow for a direct manipulation of mental state reasoning. These results suggest that failures to consider another person's mental states may not stem from an inability to do so or from a belief that another person's mental states are irrelevant, but rather stem from the lack of sufficient motivation or cues to activate mental state reasoning.

Interestingly, those with high-functioning autism, usually considered to have a deficiency in the *ability* to think about the minds of others, appear to show a similar pattern (Chevallier, Kohls, Troiani, Brodkin, & Schultz, 2012). In one recent set of experiments (Begeer, Malle, Nieuwland, & Keysar, 2010), people with high-functioning autism were considerably less likely than normal controls to describe another person's actions using mental state terms spontaneously. However, in a communication game in which people were required to consider another person's visual perspective in order to perform the task (Keysar, Lin, & Barr, 2003), those with high-functioning autism showed no deficits in mental state reasoning compared with healthy controls. These results suggest that individual differences in mental state reasoning may not stem from differences in the ability to think about the mind of another person once an individual is trying to do so, but rather from differences in the activation of mental state inferences. Similar results have also been reported for the differences in mental state reasoning between men and women. When given no strong motivation to read another person's thoughts accurately, men tend to perform significantly worse than women. But when triggered to read another person's mind accurately by increasing the incentives for doing so, men and women tend to perform similarly (see Ickes, Gesn, & Graham, 2000, for a meta-analysis).

We think the important question for psychologists to address is not *who* is better or worse at using their capacity to reason about other minds, but rather *when* this capacity is activated. A full understanding of mental state inferences, we believe (see also Epley & Eyal, 2011), would consider four critical components: activation (*when* people are triggered to reason about the minds of others), application (*how* people reason about the minds of others once they attempt to do so), accuracy (*how well* people reason about others' actual mental states), and adaptive functioning (how the accuracy of mental state inferences is related to important consequences). We believe the existing theory of mind has largely overlooked the first stage of activation, often by suggesting that mental state inferences are automatic or spontaneous and that this activation stage is therefore unimportant.

We believe that is a mistake because it could exaggerate the influence of mental state inferences in social interactions. We also believe it is a mistake because it overlooks factors that moderate the impact of mental state inferences. Our experiments investigated the impact of one moderating factor—the extent to which a gift is unexpected—on mental state inferences in evaluations. Our experiments also suggested that contexts that make a gift giver's thoughtfulness more readily accessible could increase the impact of mental state inferences. Gifts that make a giver's thoughtfulness relatively more transparent, such as highly personalized gifts, or gifts that do not include a material object that could "crowd out" mental state inferences, such as experiences rather than a material possession, could increase the impact of a giver's thoughts on receiver's evaluations.

Mispredicted: Mistaken Predictions by Gift Givers

Although gift givers anticipated the influence of gift quality on receivers' appreciations correctly, givers from all four experiments consistently failed to predict correctly when their thoughts would count and when they would not. Observer participants in the pretest of Experiment 3 even predicted that a gift giver's thoughts would count uniformly across all conditions, overestimating quite dramatically how much a gift giver's thoughts actually count in receivers' evaluations. Because gift givers and outside observers do not experience the same psychological processes that activate mental state inferences among gift receivers, it can be challenging to know when spending time and careful thought on a gift will matter and when it will not. In this way, "it's the thought that counts" is mispredicted.

These results add to the large existing literature demonstrating the difficulties people can have reading the minds of others accurately (Epley, 2008; Ickes, 2003; Nickerson, 1999). We believe it is interesting that gift givers were inconsistent across the experiments in the extent to which they believed their thoughts would count for gift receivers. In Experiment 1, gift givers appeared to believe that gift receivers would appreciate a thoughtful gift more

than a thoughtless gift, significantly so for a liked gift ($r = .47$) and directionally so for a disliked gift ($r = .28$). We observed a similar result in Experiment 4, where gift givers believed that receivers would appreciate a gift chosen thoughtfully more than one chosen thoughtlessly. Although results for gift givers in the other experiments tended to show directionally similar effects, they did not do so in all cases and certainly did not do so reliably. Gift givers do not consistently think that thoughtful gifts will be appreciated more than thoughtless gifts, but they sometimes think so. The more distant observer participants in both the pretest reported at the end of the introduction and the pretest to Experiment 3, however, consistently showed in all conditions the conventional wisdom that thoughts will count in gift exchanges.

We think these inconsistencies may stem from differences in the natural accessibility of a gift giver's thoughts versus the objective quality of the gift in different contexts. Observers in Experiment 3, for instance, did not actually have to go through the lengthy process of choosing a gift and therefore did not consider the actual quality of the gifts with great care. Instead, they only predicted the impact of a thoughtful versus thoughtless decision on gift receivers' evaluations in relative isolation. Similarly, in Experiment 4, gift givers were not shown a comparison gift that would highlight the relative desirability of a gift and instead were asked only to choose either thoughtfully or thoughtlessly. Such contexts that provide an isolated focus on a gift giver's thoughts may therefore increase their perceived importance in the social exchange. The conventional wisdom that "it's the thought that counts" may not come from gift givers' actual experience in social exchanges, but rather may come from a natural focus on a gift giver's thoughtfulness when these social exchanges are considered abstractly. This suggestion is purely speculative, as we did not examine when people would expect others to consider their thoughts and when they would not.

Misplaced: Thoughts Count for Gift Givers

Although gift givers did not consistently think that their thoughts would count for gift receivers, we did find evidence that a gift giver's thoughts counted in a different way for gift givers themselves. In particular, we found that thoughts counted for gift givers by making them feel more closely connected to gift receivers. This occurred, we believe, because putting careful thought into a gift is an effortful process that serves as a signal to the quality of a relationship for gift givers and is simultaneously an act of perspective taking that previous research has suggested increases relational bonds.

We believe these results for gift givers may shed some insight into an important consequence of giving gifts to others. In particular, people report being significantly happier spending small amounts of money on other people than spending the same amount on themselves (Dunn, Aknin, & Norton, 2008). Our data suggest a likely mechanism: that spending time thinking about a gift for another person increases the sense of connection to that person. Social connection is a reliable determinant of happiness (Argyle, 1999) and possibly the only necessary determinant (Diener & Seligman, 2002), meaning that the sense of connection that comes from expending thought on another person may be one cause of the resulting happiness. Spending money on others may not be necessary for increasing a giver's happiness; happiness can come from being thoughtful toward others. In this way, the common wisdom that "it's the thought that counts" is misplaced.

Conclusion

Although we have spent this General Discussion addressing the theoretical implications of our research, any research on gift giving naturally raises applied questions about giving better gifts. If anything, our findings suggest that gift givers should give priority to choosing gifts that receivers actually like rather than gifts that reveal thoughtfulness. To the extent that gift givers falsely believe that their thought processes count in a gift receiver's evaluations, they are likely to pay more attention to the symbolic meaning of an exchange than they should. Although being thoughtful could lead people to choose better gifts, it could also lead people to choose worse gifts. For instance, when a gift is obviously good but does not reveal a gift giver's thoughtfulness, focusing on being thoughtful could lead givers to choose a different gift in order to signal their thoughtfulness. By doing so, gift givers could choose a systematically worse gift. In one experiment (Gino & Flynn, 2011), participants believed that gifts chosen from a receiver's wish list would be considered less thoughtful and meaningful than gifts that were not requested explicitly. This turned out to be precisely wrong, and gift receivers actually appreciated receiving a requested gift more than an unrequested yet thoughtful gift. Even when thoughtfulness leads to choosing better gifts, it does not increase gift receivers' appreciation and gratitude beyond the quality of the gift itself. Gift receivers in our experiments did not consistently appreciate thoughtful gifts. In fact, they only appreciated thoughtful gifts if they were otherwise bad gifts.

Our research suggests different consequences of putting thoughts into gift exchanges for givers and receivers. The practical implications from this, we think, are fairly clear. If you want to give a gift that someone will appreciate, then you should focus on getting a good gift and ignore whether it is a thoughtful gift or not. But if you want to feel closer to the person you are giving gifts to, then put as much thought into your gift as you possibly can and do not be offended when your thoughtfulness is overlooked.

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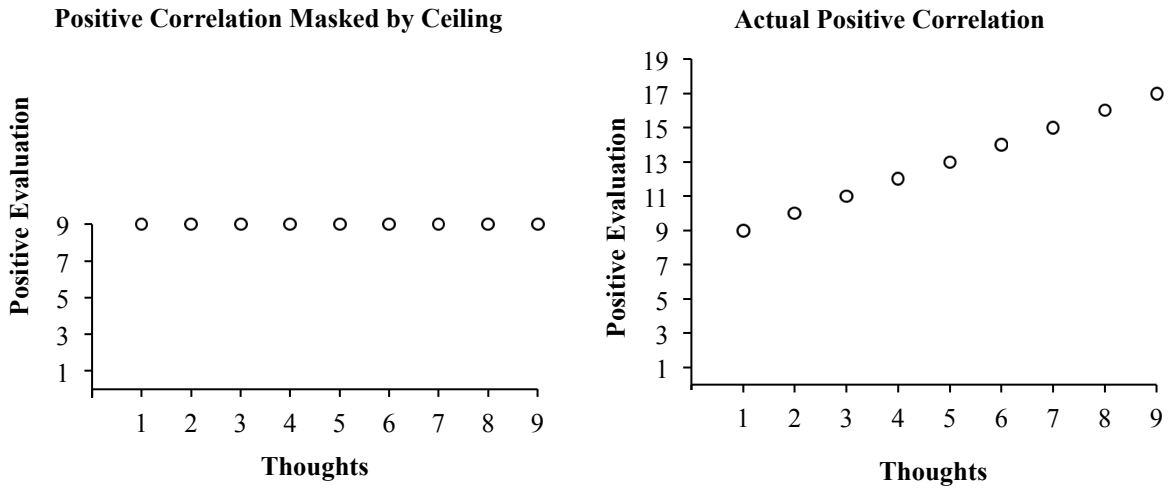
Supplemental Materials for Zhang & Epley

We observed significantly different patterns of correlations in Experiments 1 and 2 across our experimental conditions. One possible concern is that these different patterns emerged because of ceiling effects or range restrictions on one or both of our dependent measures in some of the conditions but not others. In these supplemental materials, we present evidence assessing the validity of this concern in considerably more detail than space allows in the main document.

In particular, it is possible that gift receivers evaluated liked gifts so positively that a significant positive correlation equivalent to what we observed in the disliked condition was suppressed due to a ceiling effect. Like a person who measures the correlation between shoe size and height with a ruler that only measures up to 4 feet, a positive correlation could be suppressed by a measure that does not include the full range of observations. If the ceiling of our positive evaluation measure is somehow artificially low (perhaps one person's maximum rating of 9 is only half as positive as another person's positive evaluation of 9), then a positive correlation could be suppressed. For instance, in Figure 1 below, the panel on the left is a theoretical distribution in which the ceiling of the positive evaluation measure captures none of the actual variability at the top of this scale. The panel on the right is the actual distribution that would emerge if the ceiling was lifted in a way that allowed true sensitivity to the complete range of positive evaluations. The correlation on the left is 0, but the correlation on the right is 1.

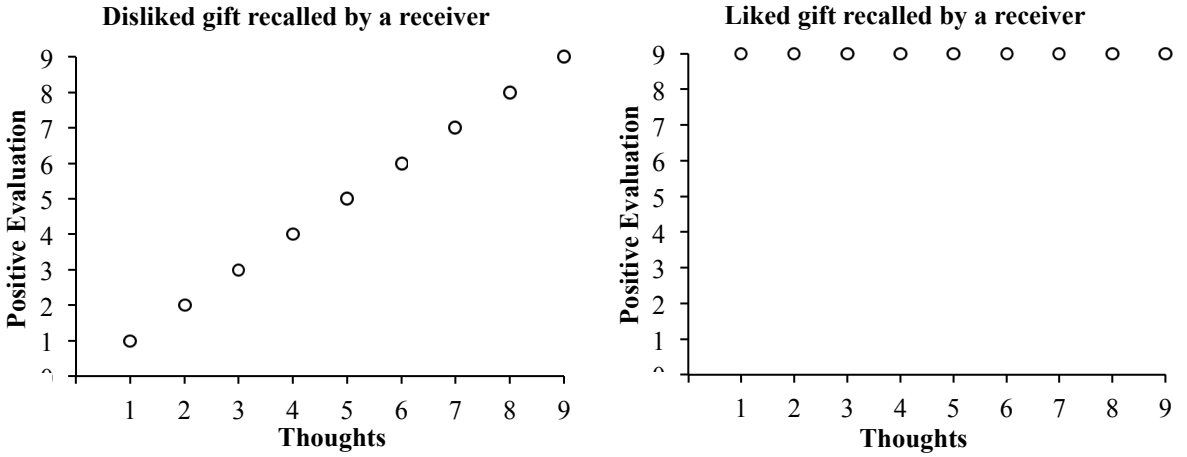
Figure 1. **Positive correlation masked by a ceiling effect vs. actual positive correlation.**



Our account, in contrast, is that thoughtful gifts produce more appreciation and gratitude than thoughtless gifts, but that receivers consider a gift giver's thoughts only when triggered to do so. A failure to evaluate thoughtful gifts more positively than thoughtless gifts therefore comes from failing to consider a gift giver's thoughts. Theoretically, this account could yield a pattern of observations at the ceiling of positive evaluations across the entire range of presumed thoughts, as shown in Figure 2 below. The panel on the left is the theoretical relationship between a receiver's positive evaluations and a gift giver's thoughts when receivers are triggered to consider those thoughts. The panel on the right is a theoretical relationship between thoughts

and positive evaluations when gift receivers fail to think of a giver's thoughts. Notice the relationship on the right is at ceiling of positive evaluations across the entire range of presumed thoughtfulness. The correlation on the left is 1 and the correlation on the right is 0. Our acount does not assume any additional variability in evaluations beyond the ceiling of our measures.

Figure 2. **Theoretical prediction for disliked gifts vs. liked gifts.**



On our account, if one was to theoretically lift the ceiling of the positive evaluation measure as in Figure 1, the correlation would remain unchanged. Like untethered helium-filled balloons, positive evaluations would simply rise to the new ceiling. There would still be no significant relationship between a receiver's positive evaluation and a gift giver's presumed thoughtfulness.

In these supplemental materials, we assess in as many ways as we can identify whether the pattern of distributions we observe looks more like those in Figure 1, consistent with the alternative that a positive correlation is suppressed by a ceiling effect, or Figure 2, consistent with our account that a null correlation is produced by failing to be triggered to think of a gift giver's thoughts.

Experiment 1

As can be seen in Table 1 below, there is clear evidence of more evaluations at the ceiling in some conditions than others. In particular, 72% of evaluations are at the ceiling of one of the two measures for gift receivers recalling a liked gift.

Table 1. **Percentage of responses at ceiling of either positive evaluation or thoughts measure.**

	Percentage of responses at ceiling	
	Liked gift	Disliked gift
Receivers	72.00%	0.00%
Givers	44.44%	13.64%

The exact distributions for all four experimental conditions are presented in the scatterplots below as Figures 3, 4, 5, and 6. In these scatterplots, red markers indicate duplicate points. The number to the right of each red marker indicates the number of observations at that point.

Figure 3.

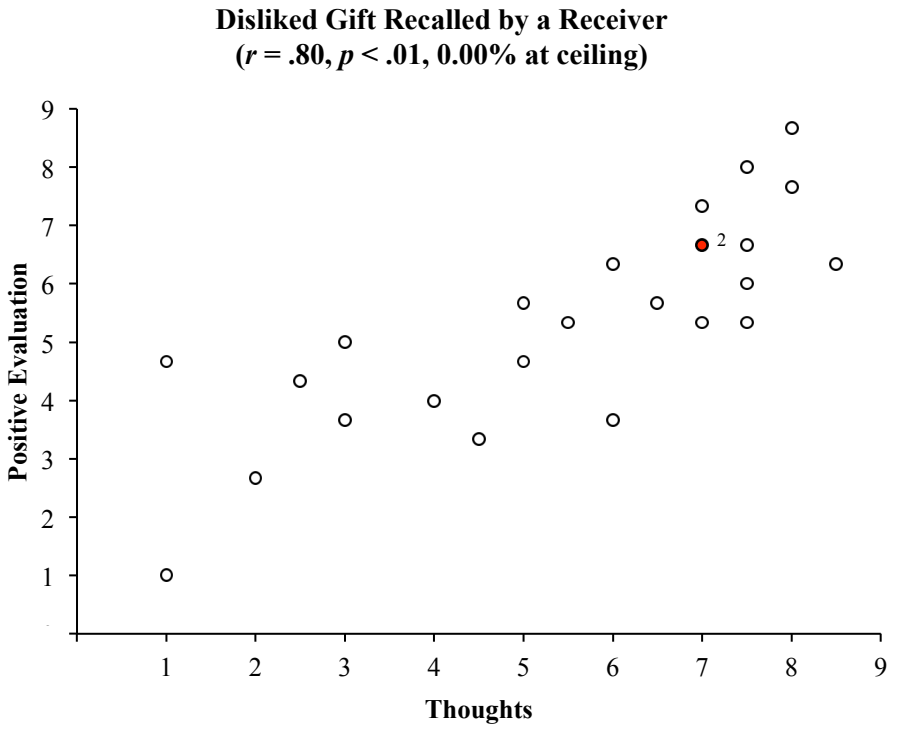


Figure 4.

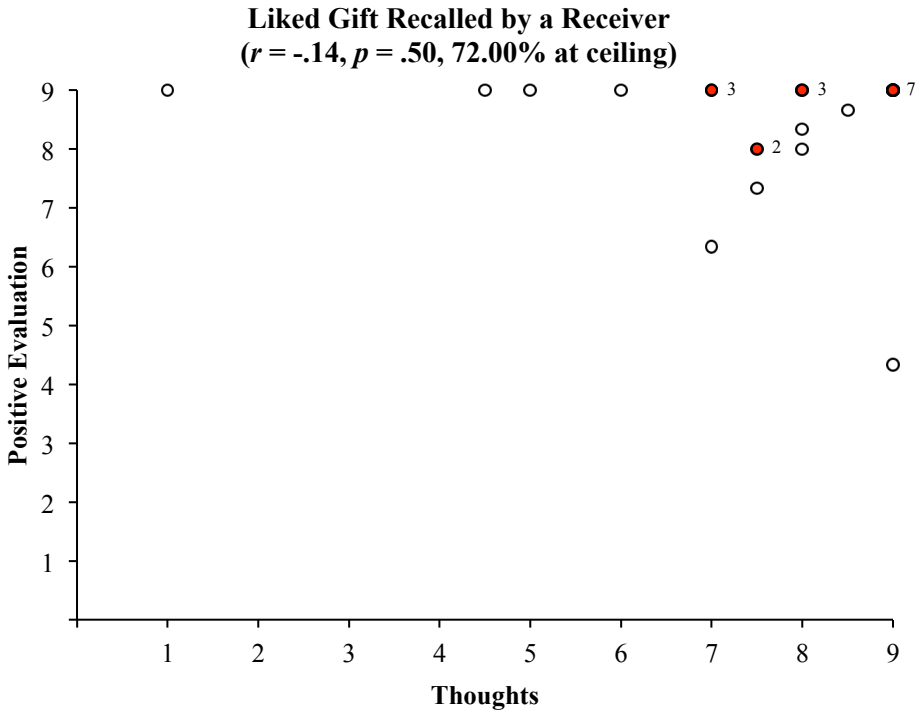


Figure 5.

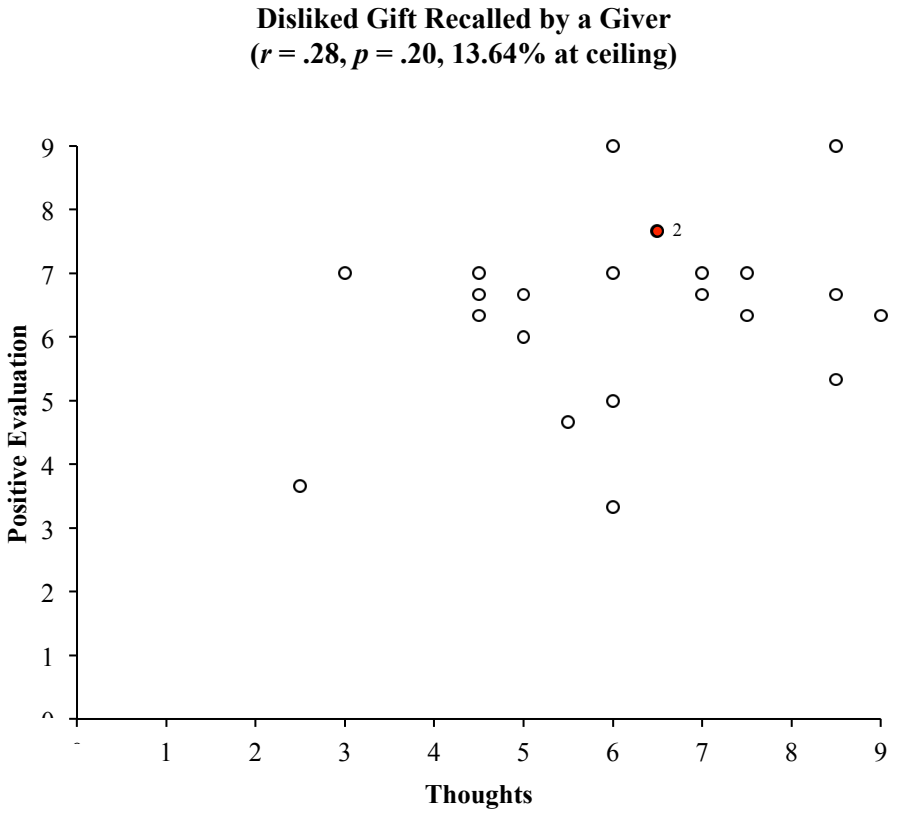
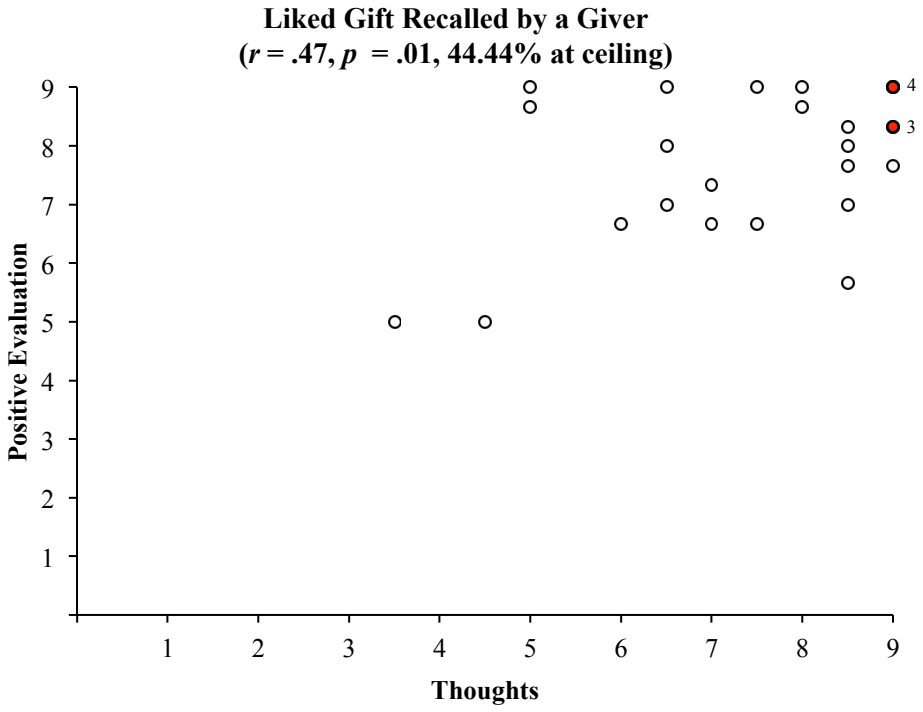


Figure 6.



As can be seen in these scatterplots, the most observations at the ceiling of the positive evaluation and thought measures occurs in the condition where gift receivers are recalling a liked gift. We assess whether these observations at ceiling masked an actual positive correlation, like the positive correlation observed among gift receivers recalling a disliked gift, in three ways.

First, we assessed whether it was even theoretically possible to obtain a significant correlation, given the ranges of observations we actually observed in each of our conditions, or whether the range was so suppressed by a ceiling effect that a correlation was theoretically impossible. To do that, we broke the dependency between thoughts and positive evaluation for each individual participant and simply rank ordered the two dependent measures to identify the maximum positive and negative correlation that could have been observed in each of the four conditions given the responses obtained. The results are presented below in Table 2.

Table 2. **Maximum possible correlation in Experiment 1.**

	Positive correlation		Negative correlation	
	Liked gift	Disliked gift	Liked gift	Disliked gift
Receivers	.95(<.01)	.97(<.01)	-.41(<.05)	-.95(<.01)
Givers	.98(<.01)	.95(<.01)	-.77(<.01)	-.92(<.01)

These results simply show that there is enough variability in all conditions in this experiment for a strong positive correlation to emerge. The restricted ranges of observations alone was not sufficient to produce the nonsignificant correlations we observe among receivers in the disliked gift condition.

Second, we assessed the correlations between thoughts and positive evaluations using Tobit regressions rather than Pearson correlations. A Pearson correlation treats an observation at the ceiling of a given measure as the true value of the observation, whereas a Tobit regression treats an observation at the ceiling of a measure as equivalent to that observation or greater. Tobit regressions are designed specifically for cases in which observations are censored by ceiling (or floor) effects. In our data, a Tobit regression tests the potential relationship between positive evaluations and a gift giver's thoughts if you were to remove the ceiling on the positive evaluation measure. As shown below in Table 3, Tobit regressions produce the same pattern of correlations across experimental conditions as the Pearson correlations.

Table 3. **Tobit regression for Experiment 1.**

	Tobit regression coefficients (p-value in parenthesis)	
	Liked gift	Disliked gift
Receivers	-0.38 (.41)	0.62 (<.0001)
Givers	0.43 (.02)	0.25 (.17)

For gift receivers recalling a liked gift, there is no evidence at all of a suppressed positive correlation. If anything, the relationship between thoughts and positive evaluations becomes more *negative* when the impact of a ceiling effect is reduced.

Another way to raise the ceiling on the thought and positive evaluation measure, aside from the Tobit regression, is to perform a reflected log transformation on all observations to reduce the negative skew. We did so on the two measures for the liked gift condition using this formula: $Measure_{transformed} = -\log(-Measure_{original} + 10) + constant)$. The resulting correlation among gift receivers in the liked gift condition ($r = -.05$, $p = .80$) was nearly identical to the correlation observed in the raw data ($r = -.14$, $p = .5$). There is no evidence that a disproportionate number of observations at the ceiling in this condition are responsible for the absence of a correlation in this condition.

Third, instead of assessing the consequences of lifting the ceiling on our critical dependent variables in the liked gift condition, we assessed the consequences of *creating* a ceiling effect on our dependent variables in the disliked gift condition. The concern about a ceiling effect among gift receivers is that the actual correlation between positive evaluations and presumed thoughts is the same when receivers recall a liked versus disliked gift, but that this correlation can only be observed in the disliked gift condition due to the *absence* of a ceiling effect. If this is correct, and if the distributions for gift receivers are actually the same between liked and disliked gifts, then the strong positive correlation we observe in the disliked gift condition should be eliminated if we create a ceiling effect in the distribution of this condition.

To assess this, we truncated the distribution across the entire range of the positive evaluation and thought measures to create varying degrees of ceiling effects, from modest censoring of observations to severe censoring. The results are presented below in Table 4.

Table 4. **Truncated correlations in the disliked gift condition for receivers in Experiment 1.**

Ceiling*	Number of Obs. at ceiling (percentage in parenthesis)	Correlation (p-value in parenthesis)
No truncation, (9, 9)	0 (0%)	.80 (< .01)
Truncated at (7.5, 6.3)	11 (44%)	.80 (< .01)
Truncated at (7.5, 5)	16 (64%)	.67 (< .01)
Truncated at (6.5, 5)	16 (64%)	.68 (< .01)
Truncated at (6, 5)	17 (68%)	.67 (< .01)
Truncated at (4, 4)	22 (88%)	.64 (< .01)
Truncated at (3, 3)	23 (92%)	.68 (< .01)
Truncated at (2, 2)	24 (96%)	.69 (< .01)

* The first number in parentheses indicates the ceiling for the thoughts measure, and the second number indicates the ceiling for the positive evaluation measure.

The first of these truncated distributions mimics, roughly, the number of observations at the ceiling of both positive evaluation and thoughts measures among gift receivers in the liked gift condition. The other truncation points are arbitrary, simply increasing in the severity of the ceiling effect. At no point do we observe a correlation that even vaguely resembles the correlation observed among gift receivers in the liked gift condition (r = -.14). This provides evidence that the distributions among gift receivers in the liked gift and disliked gift condition are different from each other. There is no evidence in these data that creating a ceiling effect would cause these two distributions to look similar to each other.

The scatterplots for all of these distributions summarized in Table 4 are presented below in Figures 7-14.

All of these analyses for Experiment 1 provide evidence against concerns about a ceiling effect suppressing a genuine positive correlation among gift receivers recalling a liked gift. Instead, they all support the existence of a genuine nonsignificant relationship, consistent with our theoretical account.

Figure 7.

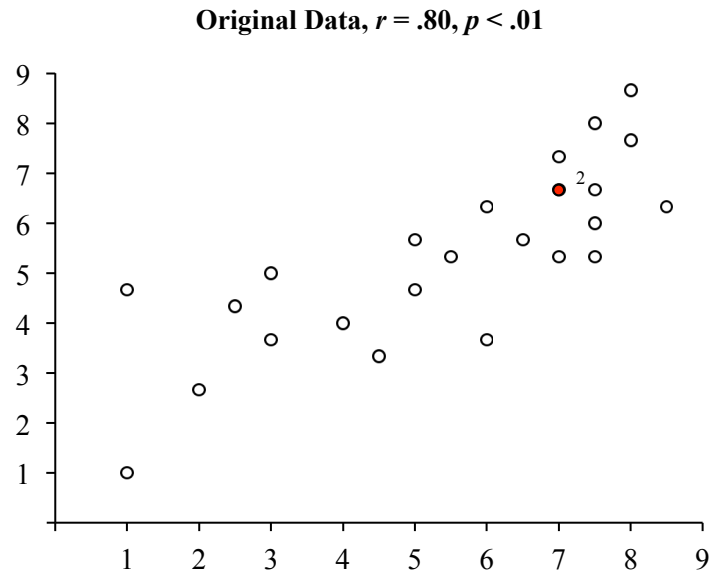


Figure 8.

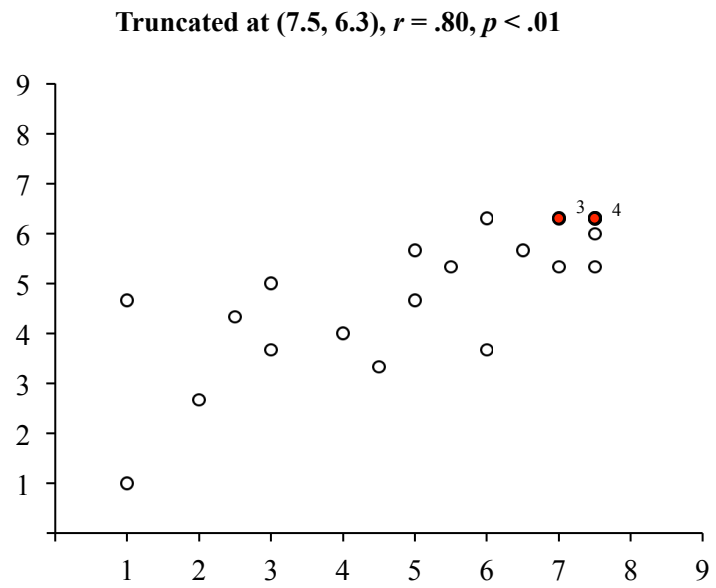


Figure 9.

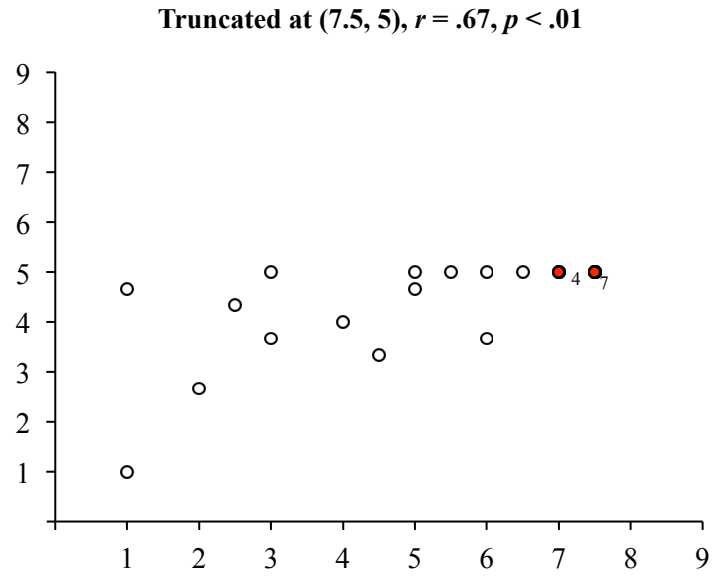


Figure 10.

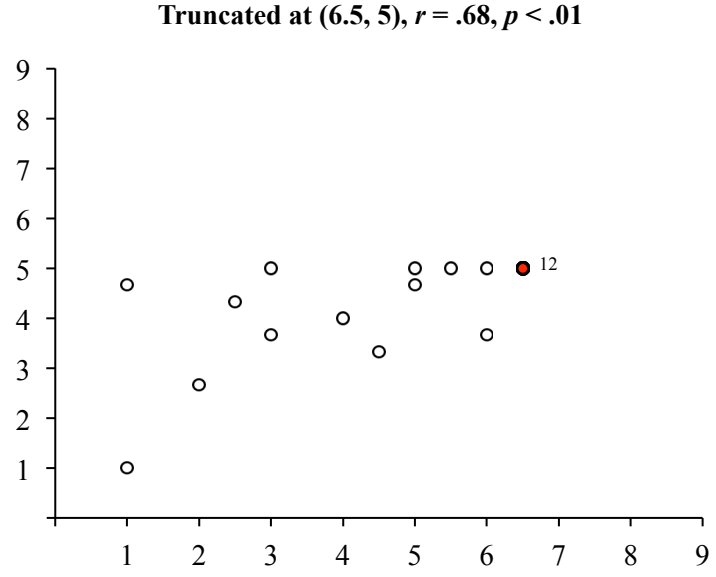


Figure 11.

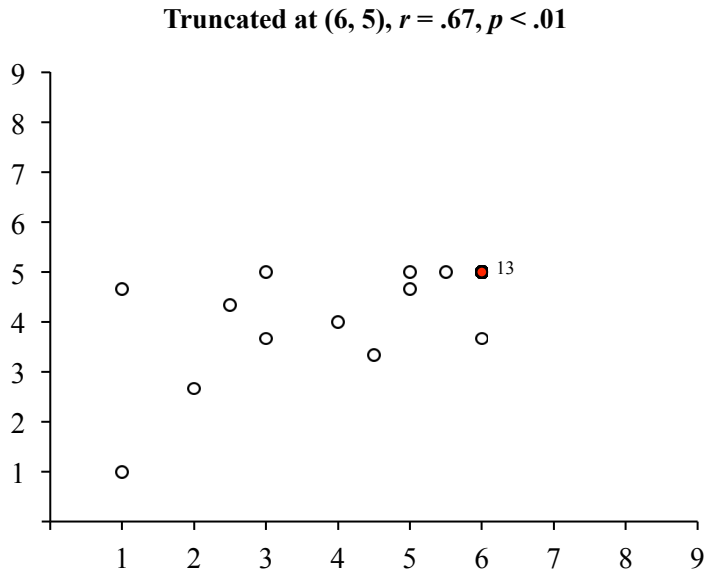


Figure 12.

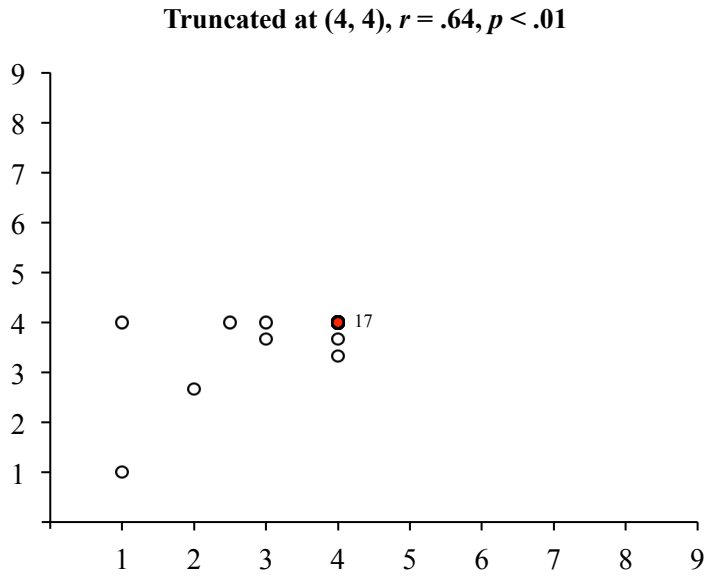


Figure 13.

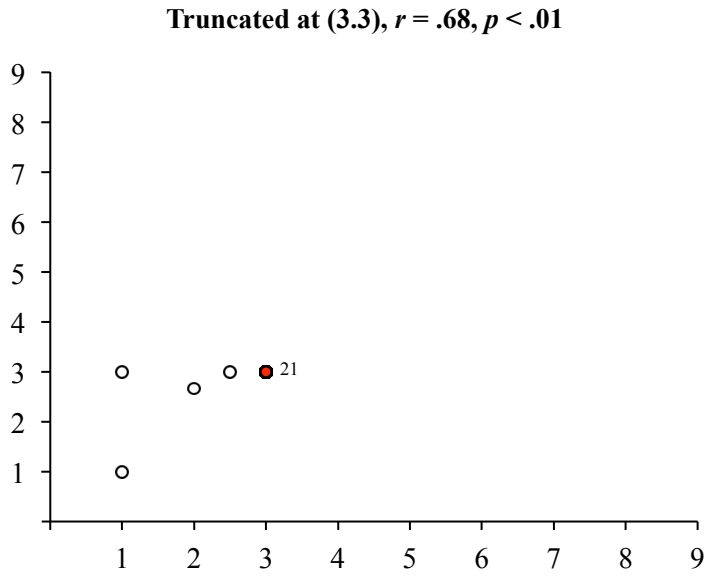
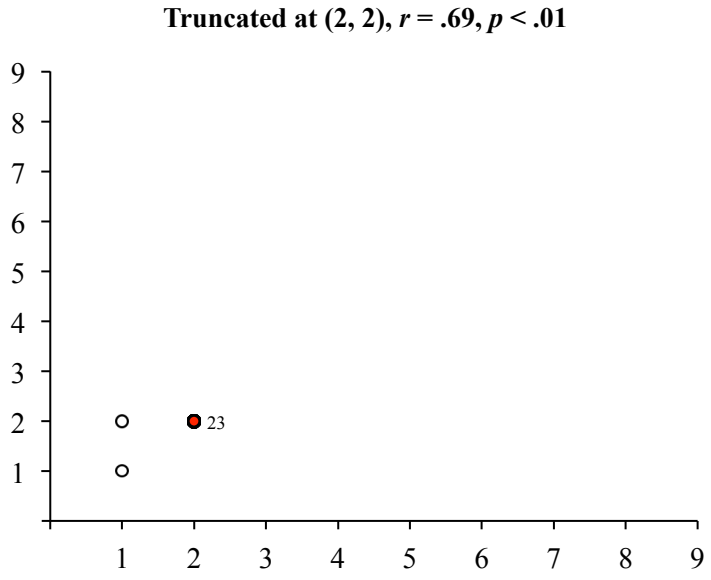


Figure 14.



Experiment 2

Experiment 2 manipulated whether participants were explicitly asked to think about a gift giver's thoughts or not before reporting their positive evaluations (of appreciation and gratitude). We predicted that this explicit trigger would bring gift receivers' positive evaluations in line with the gift giver's presumed thoughtfulness for both disliked as well as liked gifts. That is, even for liked gifts, we predicted that gifts given more thoughtlessly would produce more negative evaluations than gifts chosen thoughtfully. When not explicitly triggered to consider a gift giver's thoughts, we predicted that receivers would show a correlation between positive evaluations of the exchange and a gift givers presumed thoughts only when recalling a disliked gift. As in Experiment 1, we predicted that gift receivers recalling a liked gift would evaluate the gift positively regardless of the amount of thought gift givers presumably put into the gift because they were not triggered to think of a giver's thoughts.

Our results support this prediction. To assess whether a ceiling effect could again be suppressing an actual positive correlation among gift receivers in some conditions (in particular, among gift receivers recalling a liked gift in the thoughts last condition), we conducted the same supplemental analyses as we did for Experiment 1.

As in Experiment 1, there are more observations at the ceiling of our dependent measures among receivers evaluating liked gifts than in any of the other conditions. There is not, however, a marked difference in the percentage of observations at ceiling between gift receivers in the thoughts first versus thoughts last conditions, and yet we observe very different patterns of correlation between these two conditions. The percentage of observations at the ceiling of either the positive evaluation or thoughts measure are shown below in Table 5.

Table 5. **Percentage of responses at ceiling in Experiment 2.**

	Percentage of responses at ceiling	
	Liked gift	Disliked gift
Thoughts First		
Receivers	57.89%	4.55%
Givers	47.37%	10.53%
Thoughts Last		
Receivers	65.00%	4.76%
Givers	30.00%	14.29%

The exact distributions for all eight experimental conditions are presented in the scatterplots below as Figures 15-22. In these scatterplots, red markers indicate duplicate points. The number to the right of each red marker indicates the number of observations at that point.

Figure 15.

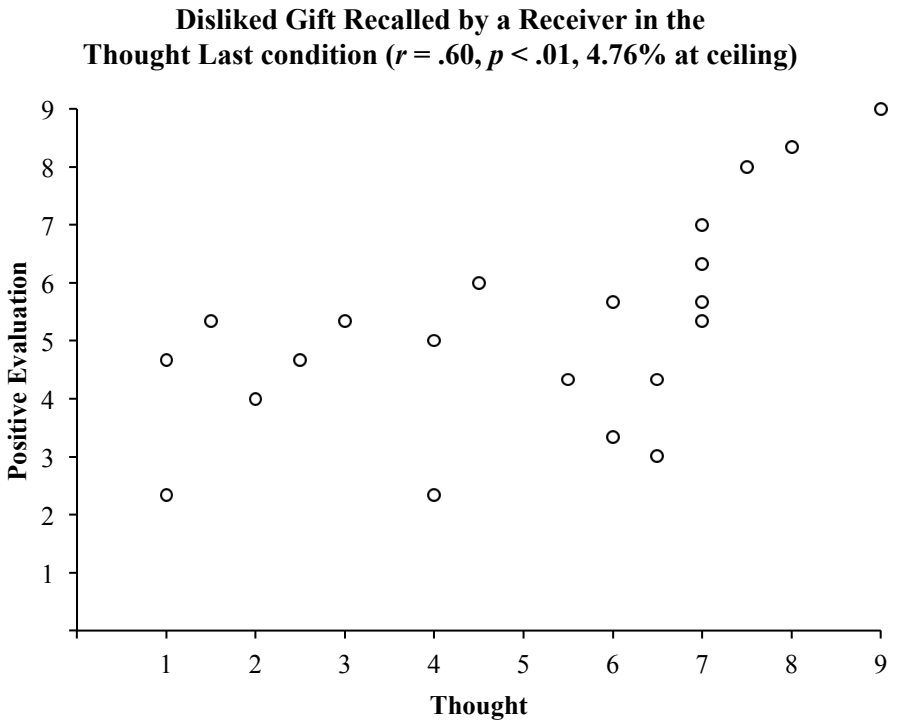


Figure 16.

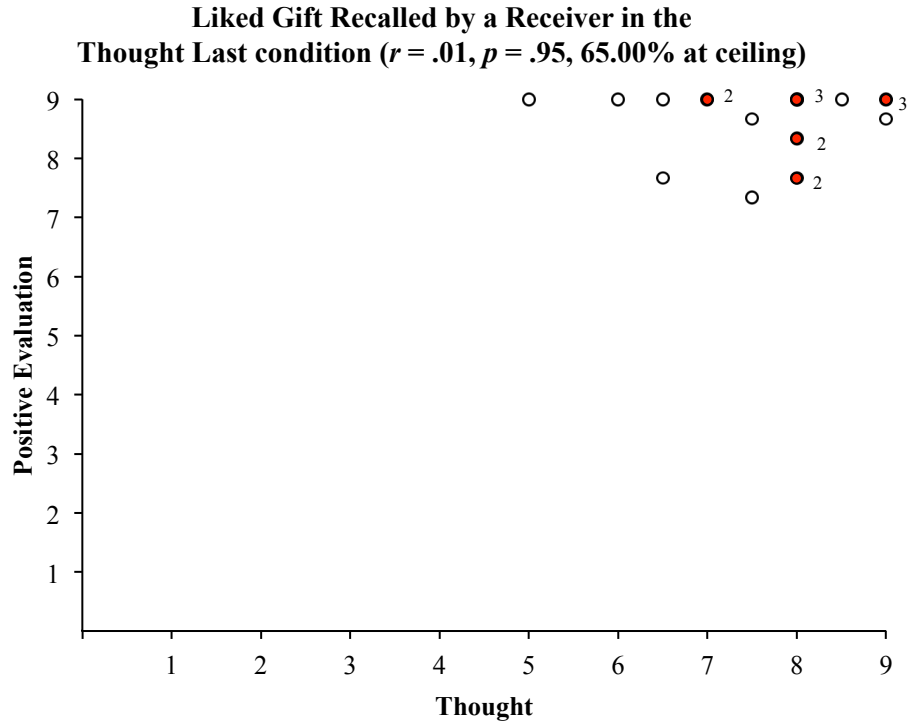


Figure 17.

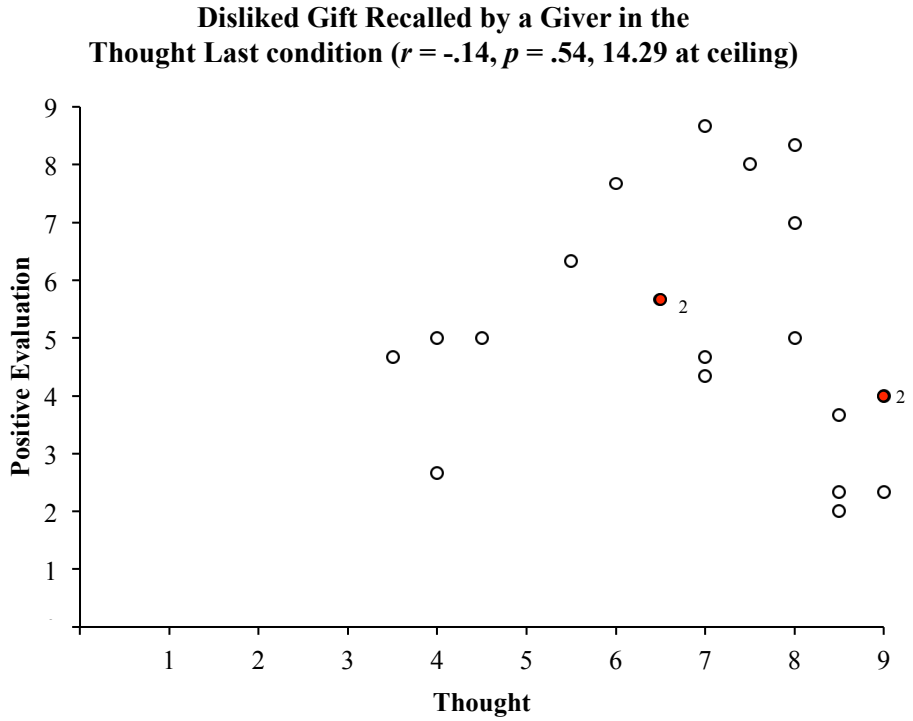


Figure 18.

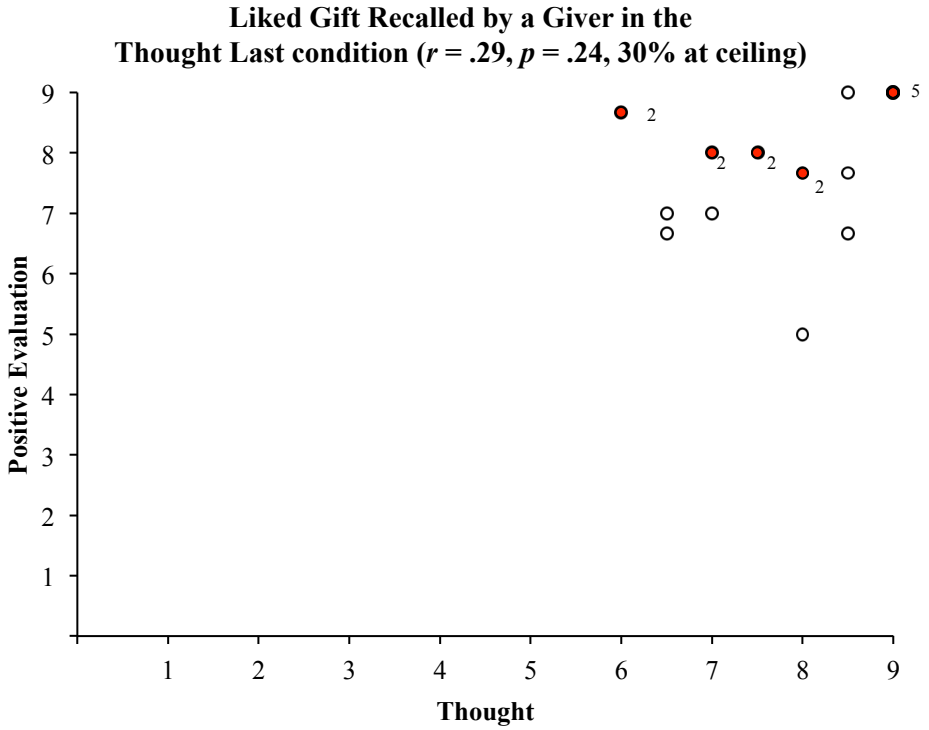


Figure 19.

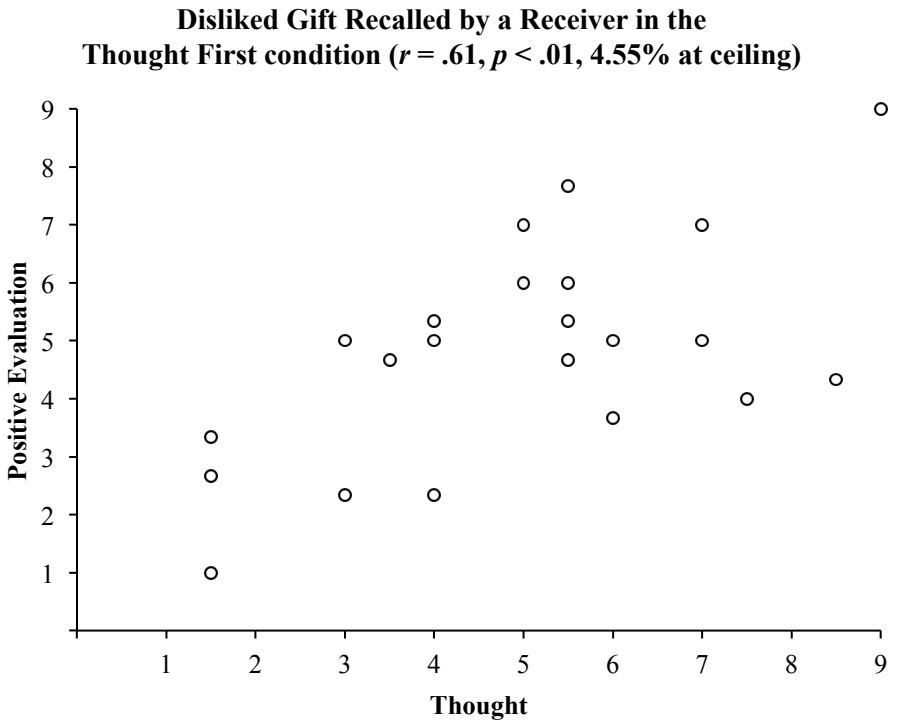


Figure 20.

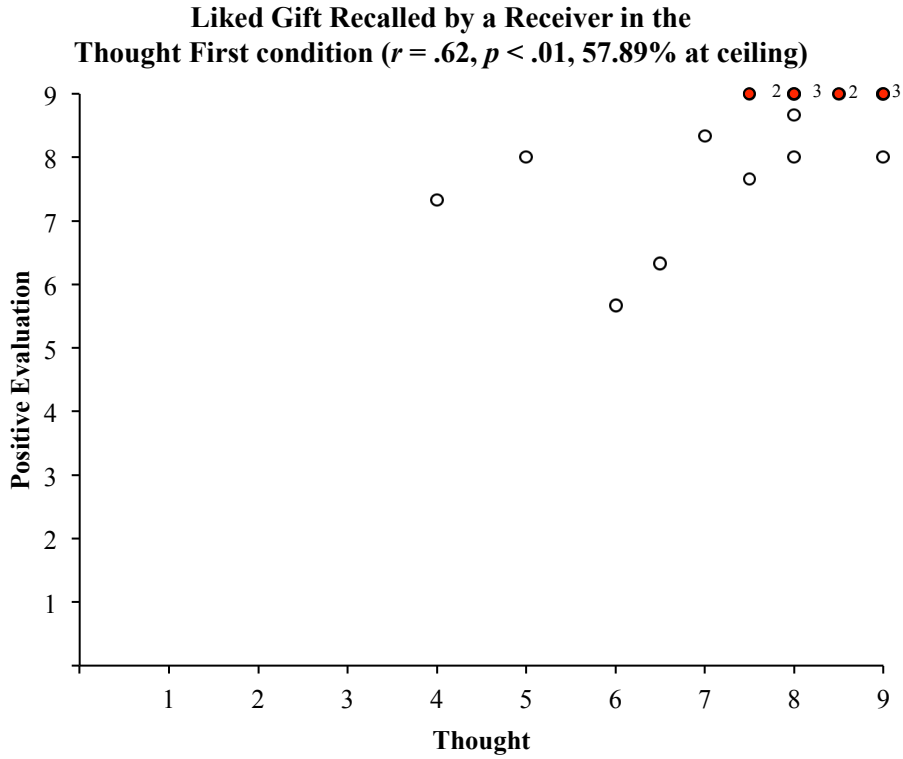


Figure 21.

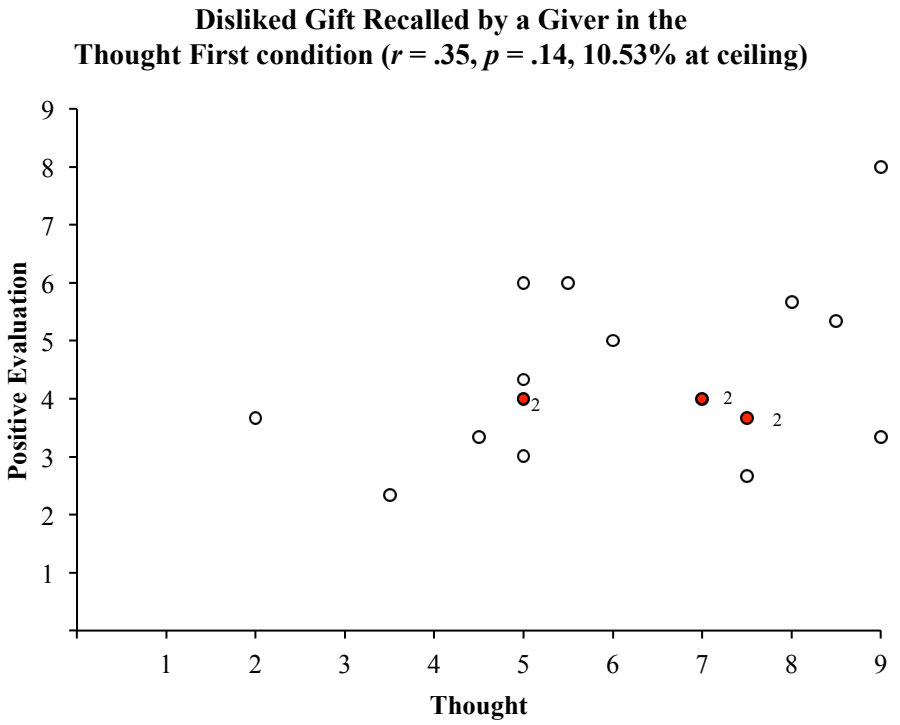
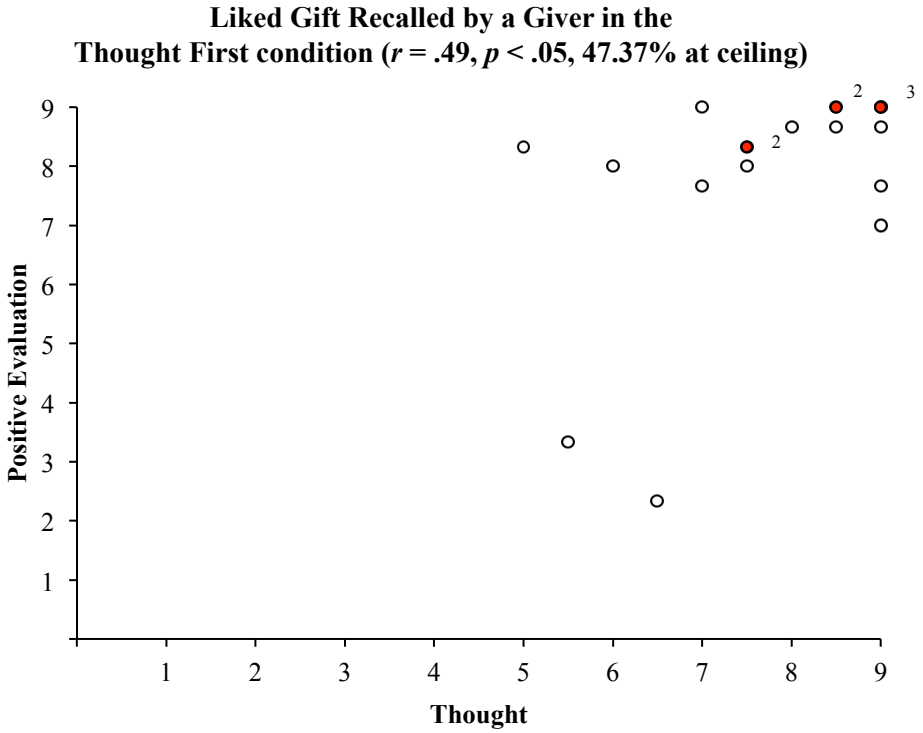


Figure 22.



First, we again assessed the maximum possible correlations in each of our experimental conditions. The results are shown below in Table 6. As in Experiment 1, it is at least theoretically possible to obtain strong positive correlations, given the data we actually observed, in all of our experimental conditions.

Table 6. **Maximum possible correlation in Experiment 2**

	Positive correlation		Negative correlation	
	Liked gift	Disliked gift	Liked gift	Disliked gift
Thoughts First				
Receivers	.96 (<.01)	.97(<.01)	-.63(<.01)	-.97(<.01)
Givers	.87 (<.01)	.90(<.01)	-.57(=.01)	-.93(<.01)
Thoughts Last				
Receivers	.94 (<.01)	.92 (<.01)	-.72(<.01)	-.96(<.01)
Givers	.94 (<.01)	.94 (<.01)	-.87(<.01)	-.98(<.01)

Second, we analyzed our data using Tobit regressions instead of Pearson correlations. As in Experiment 1, doing so does not alter the results meaningfully. In particular, there is still no significant positive relationship observed in among gift receivers considering a liked gift in the thoughts last condition (B = -.07, p = .83). The Tobit regression did show a significant positive relationship between thoughts and positive evaluations for liked gifts in the thought last condition, but it was among givers (B = .65, p < .05), not receivers. If anything, these analyses suggest that a ceiling effect for liked gifts may be suppressing a positive relationship between thoughts and predicted positive evaluations among gift *givers*, but it does not suggest any suppressed correlation among gift receivers. This provides further support for our prediction that gift givers would fail to predict when their thoughts would count for gift receivers and when they would not. The Tobit regression results are presented below in Table 7.

Table 7. Tobit Regression for Experiment 2:

	Tobit regression coefficients (p-value in parenthesis)	
	Liked gift	Disliked gift
Thoughts First		
Receivers	0.74 (< .01)	0.57 (<.01)
Givers	1.00 (.01)	0.25 (.10)
Thoughts Last		
Receivers	-0.07 (.83)	0.46 (< .01)
Givers	0.65 (<.05)	-0.16 (.51)

As with Experiment 1, we also normalized the distribution in the gift receiver/disliked gift/thoughts last condition using a reflected log transformation. Doing so did not produce a significant correlation in this condition, $r(17) = .10$, $p = .68$.

Third, we again truncated the distribution of responses among gift receivers evaluating a disliked gift in the thoughts last condition (the analogous condition in Experiment 1). Truncating the distribution produced more modest correlations overall than what we observed in Experiment 1, presumably because the actual correlation in this experiment was weaker to begin with (.59 instead of .80 in Experiment 1). The correlations observed from the truncated distributions are shown below in Table 8.

Table 8. **Truncated correlations for gift receivers evaluating a disliked gift in the thoughts last condition.**

Ceilings*	Number of Obs. at ceiling (percentage in parenthesis)	Correlation (p-value in parenthesis)
No truncation, (9, 9)	1 (5%)	.59 (< .01)
Truncated at (7, 6)	8 (38%)	.41 (.06)
Truncated at (7, 5)	12 (57%)	.28 (.22)
Truncated at (7, 4)	17 (81%)	.24 (.30)
Truncated at (7, 3)	19 (90%)	.36 (.11)
Truncated at (6, 4)	19 (90%)	.21 (.36)
Truncated at (6, 3)	19 (90%)	.35 (.11)

* The first number in parentheses indicates the ceiling for the thoughts measure, and the second number indicates the ceiling for the positive evaluation measure.

These results suggest that a ceiling on this distribution could reduce a significant correlation of .59 quite considerably. At no point, however, do we observe a correlation as small as what we observed among gift receivers evaluating a *liked* gift in the thoughts last condition (r = .01). This particular analysis suggests that the significant correlation of .59 we observed could be suppressed considerably by ceiling effects, but it does not demonstrate that it would be directionally eliminated altogether. The scatterplots for all of these truncated distributions are presented below as Figures 23-29.

Conclusion

These analyses provide no evidence that ceiling effects are suppressing an otherwise significant positive correlation among gift receivers in either Experiments 1 or 2.

Figure 23.

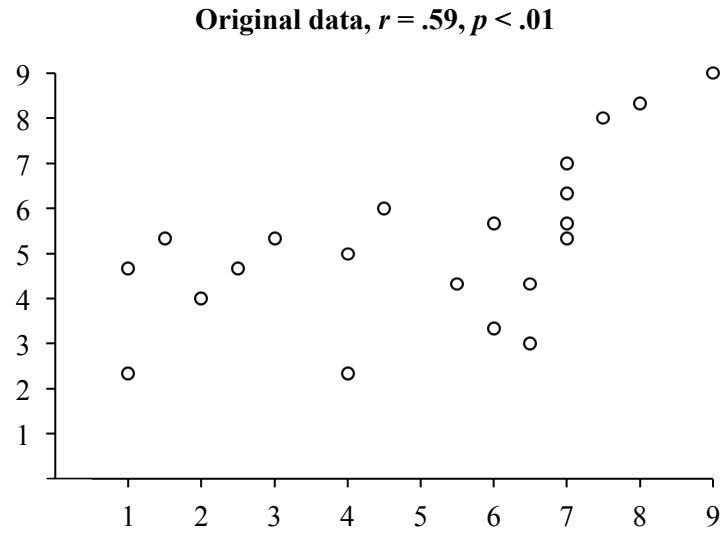


Figure 24.

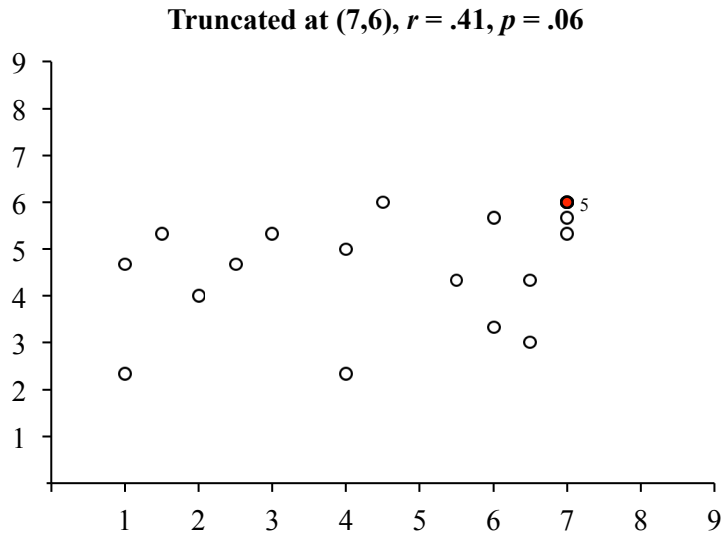


Figure 25.

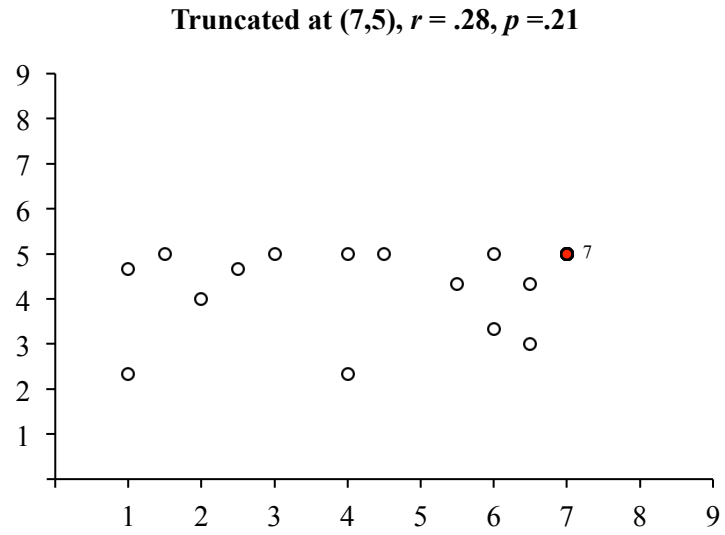


Figure 26.

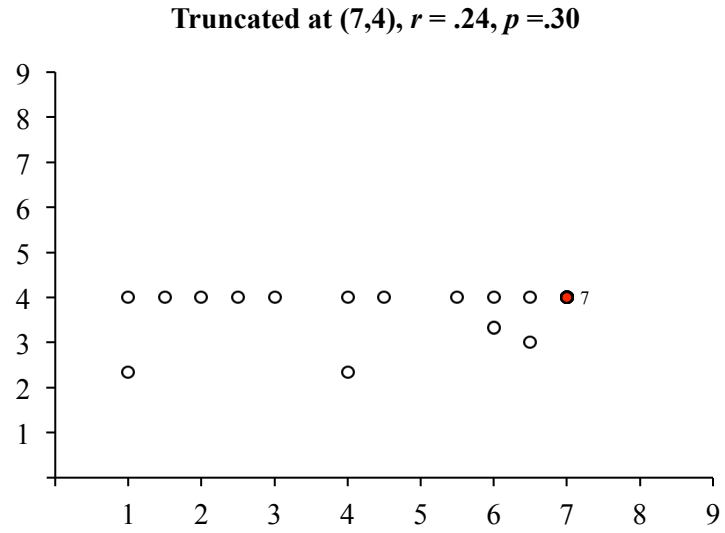


Figure 27.

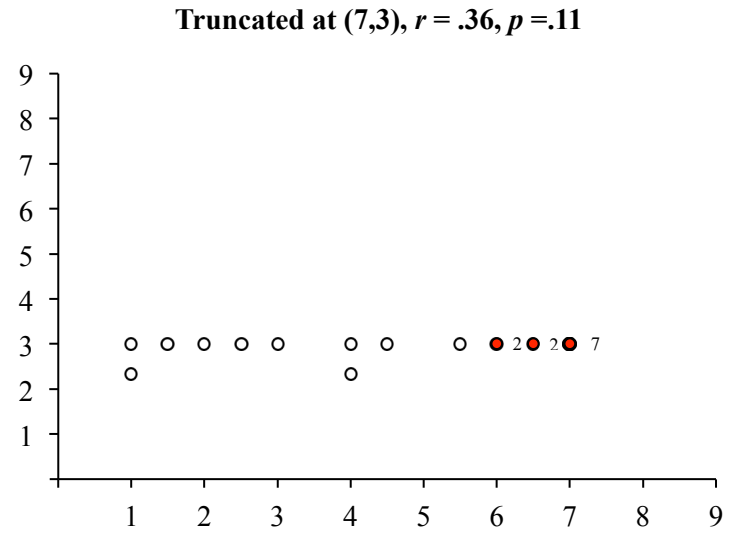


Figure 28.

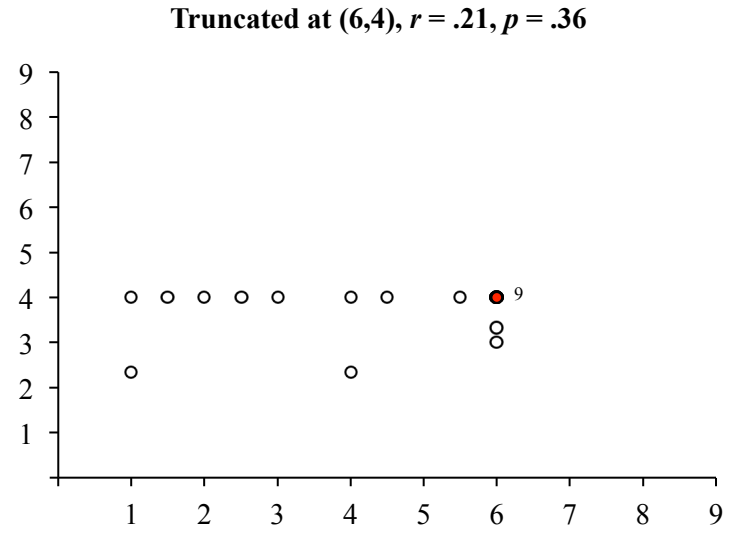


Figure 29.