Mere Gifting: Liking a Gift More Because It Is Shared

Personality and Social Psychology Bulletin
1–13

DOI: 10.1177/0146167217718525
pspb.sagepub.com
SAGE

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Abstract

We investigated a type of mere similarity that describes owning the same item as someone else. Moreover, we examined this mere similarity in a gift-giving context, whereby givers gift something that they also buy for themselves (a behavior we call "companionizing"). Using a Heiderian account of balancing unit-sentiment relations, we tested whether gift recipients like gifts more when gifts are companionized. Akin to mere ownership, which describes people liking their possessions more merely because they own them, we tested a complementary prediction: whether people like their possessions more merely because others own them too. Thus, in a departure from previous work, we examined a type of similarity based on two people sharing the same material item. We find that this type of sharing causes gift recipients to like their gifts more, and feel closer to gift givers.

Keywords

mere similarity, gift-giving, sharing, balance theory, interpersonal closeness

Received February 9, 2017; revision accepted June 12, 2017.

According to a recent report, Americans spent more money on gifts in 2016 than the total yearly GDP of Sweden (Deloitte University Press, 2016). However, people too often give gifts that recipients do not want. For instance, 38% of people admit to *regifting*, or passing unwanted gifts to others (Adams, Flynn, & Norton, 2012). In economic terms, gift-giving represents billions of dollars of deadweight loss annually because recipients value and sell their gifts for less than the prices paid by givers (Waldfogel, 1993). In practical terms, people struggle with choosing gifts for others (Wooten, 2000) to the extent that in lieu of choosing gifts, people are now more than ever likely to choose gift cards (National Retail Federation, 2014)—a discouraging trend considering most people consider gift cards wooden and impersonal, regardless if they are giving or receiving them (Tuten & Kiecker, 2009).

Previous research has examined the economic, social, and emotional challenges that people face when deciding what to give others (for a review, see Galak, Givi, & Williams, 2016). In short, givers often experience ambivalent feelings: Many relish the opportunity to buy gifts because gift-giving offers a means by which to build stronger bonds with others (Aknin & Human, 2015; Dunn, Huntsinger, Lun, & Sinclair, 2008), yet people are often inaccurate in what they think others will like receiving (Baskin, Wakslak, Trope, & Novemsky, 2014; Flynn & Adams, 2009; Polman & Emich, 2011; Steffel & Le Boeuf, 2014). Typically, such deficits are discussed in terms of givers struggling to take the perspective of recipients (Zhang & Epley, 2012)—indeed, much research has found that people are susceptible to egocentrism, social projection, and multiple attribution errors (Ross, 1977).

In this article, we take a different approach to this topic. Rather than examining the effects of givers' egocentrism dampening recipients' satisfaction, we offer a theoretically—and practically—relevant means to enhance gifts. In so doing, we develop links between gift-giving and the heretofore separate literature stream on incidental (mere) similarities, and a special type of sharing that we refer to as "companionizing."

Incidental Similarities and Interpersonal Closeness

A long literature on sharing has shown that possessing or using the same item as others feels intimate, fostering a feeling of connection to others (for a review, see Belk, 2010). From sharing a bed to sharing a birthday, people feel more connected to others when they share something in common—which has been found to accompany some important, generally positive, downstream consequences (Cialdini &

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Goldstein, 2004; Pinel, Long, Landau, Alexander, & Pyszczynski, 2006; Polman, Pollmann, & Poehlman, 2013; Walton, Cohen, Cwir, & Spencer, 2012; Woolley & Fishbach, 2017). For example, participants who were led to believe that they share the same fingerprints as someone else were subsequently more likely to help him or her with a school assignment (Burger, Messian, Patel, del Prado, & Anderson, 2004). In another example, study participants cooperated more in the Prisoner's Dilemma game when they were led to believe they share the same birthday as their partner (Miller, Downs, & Prentice, 1998). As these examples of sharing point out, "sharing is caring"—an aphorism that was further put to the test by Finch and Cialdini (1989), who found that sharing something incidental with someone *un*desirable (e.g., Rasputin) has the effect of lessening the negativity of people's judgments of that undesirable other. In short, when sharing something with others (e.g., values, attitudes, experiences, birthdays), the net effect is that sharing with someone promotes more liking for him or her.

The primary account for the effects of sharing something with others derives from the work of Heider (1958), who proposed that sharing something in common with others creates a "unit relation"—an association that leads, in turn, to treating people (with whom one shares) more favorably. In Heider's research, a unit relation is an ambivalent connection that results from two (or more) people having something in common, such as a birthday or a name. The connection is ambivalent because a unit relation is created whenever people share something in common, regardless of whether people like or dislike the unit relation. In plain terms, any degree of commonality that one has with another person creates a corresponding unit relation, which subsequently causes people in a unit relation to feel socially connected to one another.

The present studies build on past work on sharing by proposing that sharing something material with others similarly creates a unit relation, and we investigate the role that this unit relation plays in gift-giving. Specifically, our research focuses on whether receiving a gift from someone who buys (and thus comes to similarly own) the same gifted item for himself or herself is liked more than an identical gift that is not shared (i.e., not bought by the gift giver for himself or herself). Put simply, we investigate whether sharing an item with someone in this way fosters a unit relation—and relatedly, if an item is liked more because it is shared with someone else.

Companionizing Gifts and Creating Unit Relations With Others

Broadly speaking, we refer to gifts as "companionized" when recipients receive an item that a giver has bought for himself or herself as well. Thus, by companionizing a gift, both the recipient and the giver own (i.e., share) the same item, which could be something even ordinary. It is for this reason that we situate our investigation within the family of "mere" effects (e.g., Walton et al., 2012) because our research tests whether a gift recipient would evaluate a gift more favorably merely because the gift giver has bought and therefore owns the same gifted item for himself or herself. Because sharing something incidental creates a unit relation with others, we tested whether this type of sharing (for something material) has a similar effect on creating a unit relation and the consequences thereof.

Compared to previous research on mere similarities, our research offers two divergent perspectives. First, we consider the sharing of tangible, material items. In past research on mere similarities, the emphasis has been on sharing things like birthdays or names or attitudes with others (Burger et al., 2004; Jiang, Hoegg, Dahl, & Chattopadhyay, 2010; Miller et al., 1998; Polman et al., 2013; Walton et al., 2012). In a departure from these examples, we examined a scenario in which sharing tangible, material items establishes a mere similarity. Thus, our research tests whether the effects of mere similarities will extend to a novel domain that has not yet received experimental attention: A mere similarity that is based on *possessions* that are shared (i.e., separately owned) between people. In other words, past research has examined how people feel about something they share that is experiential or intangible, like when people evaluate a movie when they watch that movie with someone else versus alone (i.e., when attention is shared; Shteynberg, 2015). By contrast, our research is akin to investigating how people feel about something they share that is materialistic, like when they find out they have the same mug as someone in a movie.

Second, past research on mere similarities has focused primarily on the interaction people have with others who share something with them, which is to say that in past research, mere similarities have been found to create a unit relation between two people, and thus change how one person behaves toward the other on account of liking him or her more (Cialdini & Goldstein, 2004). For example, people feel validated when they share the same phenomenological reaction as someone else, like when two people laugh at the same joke (Pinel et al., 2006). Called I-sharing, this type of sharing instantly increases how much people like and feel connected to each other (Pinel, Long, Landau, & Pyszczynski, 2004). In our research, we explore a similar pattern: whether recipients feel closer to givers when the item they receive is companionized (the same as a giver's own item). Furthermore, building on past work, we test whether recipients will like their gift more too. Past research has not tested whether sharing causes a person to more or less like *what* she or he shares with the other person (be it a birthday or name or material item). To wit, in keeping with the example of having the same reaction as someone else to a joke, research has not tested whether a person will like the joke more when the reaction to it is shared. In this vein, our research focuses on whether companionizing gifts leads recipients to like their gifts more.

Once again, Heider's (1958) work on unit relations offers an explanatory mechanism. The research conducted so far on

mere similarities implies that people like the unit relation that results from a shared similarity. Appearing throughout the research, people view a unit relation as an inherently favorable association. However, in Heider's work, unit relations are ambivalent—they are not affective; instead, they simply indicate a connection between two people. For Heider, the increase in liking for another person that typically follows a unit relation with him or her is not a given. In fact, liking forms a separate relation that is independent from unit relations, such that Heider conceptualized both unit relations (the extent of connection people have with others based on sharing something in common) and sentiment relations (the extent to which people like or dislike a unit relation). Both unit and sentiment relations are integrated by Heider's balance theory, which proposes that sentiments (liking) toward entities in a unit relation will align toward a balanced state. By that, Heider means that conditional upon a unit relation existing, people are motivated to view it as a positive (i.e., liked) unit relation; alternatively, people are motivated to dissolve or weaken the unit relation if they dislike it.

The distinction between unit and sentiment relations is often absent from work on mere similarities because it is assumed that a unit relation coincides with a sentiment relation (i.e., that the unit relation is a desirable, liked relation). Generally speaking, this is a reasonable assumption, because people tend to like their birthday, name, and just about anything that reminds them of themselves (Pelham & Mauricio, 2015). Thus, with mere similarities, past research has only examined instances in which a person shares something that she or he *a priori* likes about herself or himself; that is, when the positive sentiment (liking) is already in balance with the unit relation. By contrast, people do not always receive gifts that they like; for instance, givers only correctly identify about half of the gifts that a recipient would want for himself or herself (Pollmann & van Beest, 2013). Therefore, unlike a unit relation that stems from sharing a birthday, name, or something else that people already like about themselves, a change in balance is required when the unit relation is based upon a commonality (such as a companionized gift) that is not initially, nor especially, well-liked. To achieve balance with the unit relation in this case, one strategy a gift recipient can use is to increase the sentiment relation, which means increasing liking for the gift.

Entirely consistent with this account of balancing unit-sentiment relations, some research has shown that the effects of mere similarities are moderated by how much people like themselves. People with lower self-esteem tend to like their names less (compared to people with higher self-esteem), and thus behave no more nor less friendly to others who share their names—as though a weaker sentiment relation begets a weaker unit relation (Kocan & Curtis, 2009). This suggests that unit-sentiment relations can balance according to changes in sentiment relations that are based on *what* people share (i.e., how much they like what they share). In particular, this example demonstrates the effect of sentiment relations (extent of liking one's name) on the strength of the unit relation. Our work examines the reverse: the effect of unit relations on the strength of the sentiment relation (extent of liking one's gift). In line with this focus, research has shown that when people have a unit relation with someone unpleasant, they like him or her more compared to people with no unit relation with him or her; moreover, no difference in liking was observed among people who have a unit relation (vs. no unit relation) with someone who is pleasant (Tyler & Sears, 1977). In other words, people changed the sentiment relation so that the unit-sentiment relations are in balance. However, this research measured the sentiment relation (liking) toward the other person in the unit relation rather than the sentiment relation to *what* is shared with the other person. In this vein, the present studies were designed to better understand whether sharing an item affects how much people like it.

Needless to say, an alternative solution to an imbalance between unit-sentiment relations is dissolving the unit relation. However, research on incidental similarities tends to show evidence in favor of changing the sentiment relation to fall in line with the unit relation, instead of weakening the unit relation (e.g., Arkin & Burger, 1980; Chandler, Griffin, & Sorensen, 2008; Finch & Cialdini, 1989; Nelson & Simmons, 2007 though see Snyder, Lassegard, & Ford, 1986 for an exception). Thus, balance theory suggests that when it comes to establishing a unit relation (by owning the same item as someone else), people will increase their liking of the shared item. However, research to date on mere similarities has only examined sharing positive things, much less products or other material items that people may have in common and vary in how much people like them.

In summary, our studies test for a new type of mere similarity, based on liking a gift more simply because it is shared (owned also by the giver). Holding constant all other variables, we directly compare individuals' evaluations of gifts they receive from others to test whether companionized gifts are favored more than non-companionized gifts. Evidence shows that people like their possessions more merely because they own them (Beggan, 1992). Here, we test a complementary prediction: whether people like their possessions more, merely because others own them too.

Note, we focus on gifts because unlike other material items that are shared between two people (i.e., items that are the same but not gifted from one person to another), receiving a gift does not always imply having similar preferences or tastes with the other person (the giver) in the way that sharing other non-gifted material items does. Should someone have acquired the same mug as someone else, it would be reasonable to infer that those mug-owners have similar preferences, and as a result feel more similar to one another. However, should a person receive a companionized mug as a gift rather than independently choose to acquire one that, by chance, someone they know or meet owns as well, then this could be seen as a case of sharing a material item that occurs

despite people's preferences—a more conservative test case than solely sharing similar things because, as evidenced by the opening statistic on regifting and research by Pollmann and van Beest (2013), people do not often receive gifts that are consistent with their preferences.

Overview of Studies

The major goal of the present studies is to assess people's evaluation of companionized gifts, and investigate the role of balancing unit-sentiment relations in boosting people's evaluation of companionized gifts. In the first study, we employed a relatively straightforward approach by asking ostensible recipients to imagine that givers had bought the same gifted item for themselves. We subsequently measured recipient-rated likability, thoughtfulness, and considerateness of the gift to determine how much people favor companionized gifts relative to non-companionized gifts. The next three studies ruled out alternative explanations to our hypothesized balancing account. In each of these studies, we found evidence of the companionizing effect, but we found no evidence that the boost in evaluation is owing to recipients' intuiting that (a) companionized gifts signal higher quality, (b) givers put more effort into a companionized gift, or (c) companionized gifts are more likely to come from closer (vs. relatively more distant) others. Conceivably, recipients might favor gifts that appear higher in quality, or require considerable effort, or come from closer versus distant others. However, we do not find that these reasons explain why people favor companionized gifts. In this vein, Study 5 tested whether balancing unit-sentiment relations explains the effects of companionizing. In this study, we manipulated the unit relation between giver and recipient to examine the effects of companionizing a gift in a context in which there is virtually no unit relation between giver and recipient. In keeping with our balancing account, we found recipients to be more ambivalent about gifts that contain little to no unit relation between themselves and givers. Finally, in a more direct test of our balancing account, Study 6 manipulated companionizing and subsequently measured not only recipients' evaluation of their gifts but also their unit relation with givers.

We decided ahead of time on the minimum sample size per cell in each of our studies (further justification is provided in each study's respective method section). Furthermore, we report all manipulations and measures that we conducted, and we declare that all data that were collected are included in our analyses.

Note, throughout our studies, we investigated a variety of gifts (31 in all), and therefore always included a "gift variable" in our analyses as a random factor. Accordingly, our tests are particularly conservative because they account for the random stimulus variation that is typical yet often ignored in experimental data (for a discussion, see Judd, Westfall, & Kenny, 2012). A drawback, however, of a mixed design containing fixed and random factors is that there is no effect size statistic to report. Usually, researchers report Cohen's *d* or partial eta-squared, but these statistics are only useful in designs with fixed factors (Lakens, 2013). To our knowledge, there is no effect size statistic that can accommodate a mixed design with fixed and random factors. Nevertheless, in Table 1, we provide the effect size for each individual gift that we tested, by conducting a separate individual *t* test between the companionized and control conditions. We subsequently conducted a mini meta-analysis of all of the gifts, across all studies, which we discuss in the "General Discussion."

Study 1

In Study 1, we asked participants to rate one of 10 gifts that were given by someone who companionized the gift. We measured participants' evaluations of companionized gifts and compared those evaluations to a control condition. We predict that, across the range of gifts, participants would indicate more favorability of companionized than non-companionized gifts.

Method

In this study design, we included 10 gifts as a random factor being that our 10 gifts are just a subset of possible gifts (and we wanted to investigate a range of gifts; see Table 1 for list of gifts); thus our study is a 2(condition) × 10(gift) between-subjects design. Because our design has 20 cells (participants rated only one gift, not all 10 gifts), we recruited participants on Amazon's mTurk platform to accommodate sufficient power for all of the cells. We recruited 600 participants from Amazon mTurk to participate in exchange for US$0.05 and received 616 participants.

We decided on our sample size ahead of time based on a goal of at least 300 participants in each of the control and companionized conditions, which is sufficient to detect even a small effect. Plus, it meant that our statistical power would likely exceed the recommended 80% provided that (a) we observe a difference in conditions that is at least 0.30 and (b) our study's standard deviations are similar to past research that has measured gift recipients' satisfaction (e.g., *SD*s = 0.59-1.73 in Gino & Flynn, 2011).

We randomly presented participants with one of two conditions. The companionized condition accompanied this message: *Imagine you receive a gift for a special occasion. You open the present, and inside is a new [gift], with a card that reads, "I hope you like the [gift]—I got myself the same [gift] too!"* The control condition accompanied this message: *Imagine you receive a gift for a special occasion. You open the present, and inside is a new [gift], with a card that reads, "I hope you like the [gift]!"* Participants rated how likable, thoughtful, and considerate the gift was on a scale ranging from 1 (*not at all*) to 7 (*extremely*).

Table 1. Evaluation Ratings of Gifts/Condition.

	Control		Companionized			Quality		Un-relational		Un-temporal	
	M	*SD*	*M*	*SD*	Cohen's *d*[a]	*M*	*SD*	*M*	*SD*	*M*	*SD*
Study 1											
Scarf	5.30	1.03	5.27	1.04	(0.028)						
Notebook	4.49	1.79	5.20	1.44	0.437						
Stapler	3.49	1.82	3.58	1.57	0.053						
Umbrella	4.80	1.18	4.97	1.16	0.145						
USB key	4.44	1.40	4.78	1.30	0.251						
Blanket	5.32	1.22	5.31	1.03	(0.008)						
Calendar	4.07	1.34	4.77	1.38	0.515						
Flashlight	4.22	1.65	4.36	1.32	0.094						
Kettle	4.69	1.75	5.14	1.20	0.300						
Cell phone case	4.56	1.56	5.29	1.42	0.181						
Study 2											
Bowl	4.61	1.32	4.91	1.31	0.228	4.28	1.49				
Water bottle	4.17	1.60	4.56	1.33	0.265	3.93	1.50				
Bottle opener	3.75	1.51	4.07	1.55	0.209	3.85	1.48				
Study 3											
Blender	5.15	1.07	5.79	1.08	0.590						
Tempurpedic pillow	5.33	1.28	6.03	0.90	0.630						
Cookbook	5.00	1.25	5.59	1.12	0.470						
Study 4[b]											
Air purifier	4.84	1.49	5.28	1.44	0.300						
Headphones	5.64	1.16	5.96	1.02	0.293						
Pen cup	4.53	1.59	5.03	1.38	0.336						
Study 5											
Chef's knife	4.94	1.41	5.54	1.04	0.484			4.88	1.31	5.33	0.96
Thick wool socks	4.04	1.56	4.90	1.14	0.629			4.39	1.22	4.66	1.38
Totebag	4.07	1.77	4.27	1.62	0.118			3.55	1.55	3.84	1.37
Gourmet chocolates	5.18	0.98	5.28	1.10	0.095			4.78	1.49		
White noise machine	4.87	1.26	5.06	0.88	0.174			4.66	1.54		
Candle	4.02	1.58	4.43	1.42	0.273			4.03	1.71		
Lamp	4.76	1.36	5.08	1.24	0.246					4.69	1.54
Watch	5.31	0.99	5.50	1.23	0.170					5.11	1.48
Bourbon	5.16	1.29	5.08	1.47	(0.057)					4.54	1.31
Study 6											
Bonsai plant	4.32	1.25	4.66	1.32	0.264						
Mug	3.96	1.21	4.30	1.37	0.263						
Book	4.82	1.63	5.04	1.29	0.150						
Mini meta-analysis											
Mean Cohen's *d*					0.279						

Note. Evaluation ratings consist of likability, thoughtfulness, and considerateness scores; for each study, $\alpha > .85$.
[a]Values represent results to independent *t* tests between control and companionizing conditions (values from negative *t* tests are in parentheses).
[b]Values represent results to independent *t* tests between control and companionizing conditions (across close and distant conditions).

Results and Discussion

We averaged participants' ratings of likable, thoughtful, and considerate into one index, favorability ($\alpha = .89$). Table 1 presents the descriptive statistics for each gift per condition. We conducted a 2(condition) × 10(gift) ANOVA with gift as a random factor. Participants in the companionized condition favored their gift more ($M = 4.87$, $SD = 1.38$) than participants in the control condition ($M = 4.56$, $SD = 1.56$), $F(1, 9.87) = 10.60$, $p = .009$. Thus, this study provided support for our prediction that companionized gifts are favored more than noncompanionized gifts. In addition, the effect of gift was also significant, $p < .001$ (a point to which we return in the "General Discussion," because it implies that we examined gifts that varied in their baseline and companionized evaluation, which is to be expected since we investigated 10 gifts, and it is likely that there would be between-gift

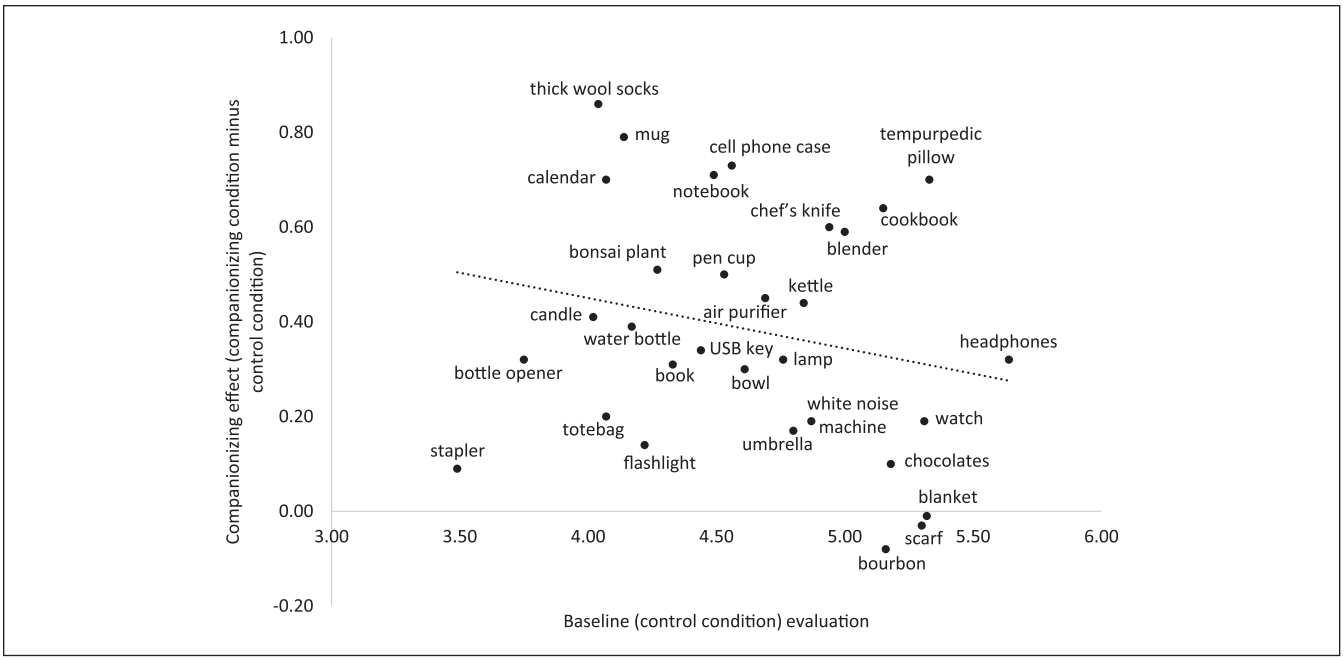

Figure 1. Downward linear relation between baseline evaluation and companionizing effect.

differences in how much people evaluate these gifts). Importantly, the interaction was not significant, $p = .801$, which indicates that our main effect of condition is not due to some gifts in our set of 10 being especially more susceptible to the companionizing effect than others (even though this is indeed the case; as we explain in our mini meta-analysis, some gifts are more companionize-able than others [see Figure 1]; however because the interaction term is not significant, the companionizing effect is occurring despite that some gifts are more susceptive to companionizing than others).

Study 2

Quite possibly, the results of Study 1 could have arisen because participants inferred that companionized gifts are higher in quality. Because givers bought themselves the same gift, receivers might infer that the giver is advocating the quality of the gift. Study 2 aimed to investigate this possibility, by comparing whether a gift that accompanies a message that communicates the high quality of the gift is more or less favored than a companionized gift.

Method

We recruited 452 participants who participated in exchange for extra-credit in a course, which would furnish roughly 150 participants in each of our three conditions. Moreover, because we tested three gifts in Study 2 (instead of 10 as in Study 1), we would exceed the roughly 30 participants/condition/gift that we had in Study 1 (which we did, with roughly 50 participants/condition/gift in Study 2).

The procedure was the same as Study 1, except that we used different gifts (see Table 1) and added a quality condition that accompanied this message, *Imagine you receive a gift for a special occasion from a friend. You open the present, and inside is a [gift], with a card that reads, "I hope you like the [gift]—it got great reviews online!"* We manipulated quality with a message about online reviews because people infer product quality from online reviews (Duan, Gu, & Whinston, 2008). Finally, we measured gift evaluation with the same three items from Study 1 ($\alpha = .85$).

Results and Discussion

As in Study 1, gift was a random factor, and we conducted a 3(condition) × 3(gift) ANOVA, which revealed a significant difference between conditions, $F(2, 4.56) = 11.95$, $p = .015$ (see Table 1 for descriptive statistics for each gift per condition). Post hoc analyses (least significant difference [LSD]) revealed that participants in the companionized condition favored their gift more ($M = 4.55$, $SD = 1.42$) than participants in both the control ($M = 4.22$, $SD = 1.52$) and quality conditions ($M = 4.05$, $SD = 1.49$), ps $< .054$. Because ratings among participants in the companionized condition differed from the quality condition, we can conclude that companionized gifts are not favored more on account of recipients inferring that such gifts are particularly high quality. Like Study 1, the effect of gift was also significant, $p = .006$, and the interaction was not significant, $p = .840$.

It is worth pointing out that we observed no difference in gift evaluation between the control and quality conditions. At first glance, this result might cast doubt on whether the

quality condition had the intended effect of increasing the inferred quality of the gifts. In support of our manipulation, however, we conducted a post-test in which we recruited a separate sample of 225 participants on mTurk (and received 229) and asked them to rate the quality of the gifts from Study 2, where participants from the three conditions rated one gift (assigned randomly) from 1 (*low quality*) to 7 (*high quality*). The post-test revealed a significant difference between conditions, $F(2, 226) = 3.24$, $p = .041$, as well as a linear trend illustrating that the quality ratings across the three conditions increase in order of control ($M = 4.45$, $SD = 1.08$), companionizing ($M = 4.59$, $SD = 1.22$), and quality ($M = 4.92$, $SD = 1.19$), $F(1, 226) = 5.94$, $p = .016$. Specifically, post hoc analyses (LSD) revealed that participants in the quality condition rated the quality of the gifts higher than participants in both the companionizing ($p = .082$) and control conditions ($p = .014$), whereas participants in the companionizing and control conditions rated the quality of the gifts almost the same ($p = .451$). In all, Study 2 finds no evidence that inferences of quality explain our results.

Study 3

Possibly, instead of the unit relation that a companionized gift generates, the boost in gift evaluation could be due to the effort that recipients infer upon receiving a companionized gift, intuiting that givers spend more time or effort choosing a companionized gift. In line with the adage "it's the thought that counts," conventional wisdom and supporting research suggest that the more effort or care that goes into choosing a gift, the more recipients will like it (Zhang & Epley, 2012). Study 3 aimed to investigate this possibility by measuring the amount of effort that participant-recipients believe givers spent in choosing the gift.

Method

We recruited 300 participants from Amazon mTurk to participate in exchange for US$0.20, and received 302 participants, yielding 151 participants/condition (similar to Study 2). The procedure was the same as Study 1, except that we used different gifts (see Table 1) and added a perceived effort measure ("How much effort do you think went into choosing the gift?"). Participants responded on a scale ranging from 1 (*not a lot of effort*) to 7 (*a lot of effort*), which we counterbalanced with the main dependent variable (there was no effect of order). As in Studies 1 to 2, the dependent variable (gift evaluation) was measured with the same three items ($\alpha = .89$).

Results and Discussion

Consistent with Studies 1 to 2, gift was a random factor, and we conducted a 2(condition) × 3(gift) ANOVA on gift evaluation. Participants in the companionized condition favored their gift more ($M = 5.80$, $SD = 1.09$) than participants in the control condition ($M = 5.13$, $SD = 1.20$), $F(1, 30.51) = 372.39$, $p < .001$. As in Studies 1 to 2, the effect of gift was also significant, $p = .020$, and the interaction was not significant, $p = .944$.

We next conducted the same analysis on gift evaluation with perceived effort as a covariate, and the above significant difference between the companionized and control conditions on gift evaluation was maintained, $F(1, 26.36) = 21.78$, $p = .043$. In further support of the inoperative role of perceived effort on the companionizing effect, we conducted an ANOVA on perceived effort, which did not yield a significant difference between participants in the companionized condition ($M = 4.56$, $SD = 1.29$) and participants in the control condition ($M = 4.39$, $SD = 1.44$), $F < 0.25$, $p = .730$. Altogether, this shows that the perception of giver-effort is not a likely explanation for the companionizing effect. As with any null result, it is possible that our design simply lacked statistical power to detect an effect on the effort measure. However, we note that with cell sizes of 151, Study 3 could detect even small effects with high power. Besides that, our prediction was based on finding both a significant result (the companionizing effect) and a non-significant result (a non-significant difference in perceived effort in the companionized and control conditions)—and we found both. All in all, inferred effort does not appear to explain the companionizing effect.

Study 4

Besides inferences of quality and effort, another competing explanation for companionizing is that participants may assume that close others are more likely to companionize their gifts than distant others—and by virtue of liking close others' gifts more than distant others' gifts (Waldfogel, 1993), participants might therefore favor companionized gifts over non-companionized gifts. For example, Waldfogel (1993) found that people valued gifts more from close others than distant others; moreover, he suggested this is because close others tend to select better gifts than distant others (in some support, research has found that close others spend more money on gifts than distant others; Saad & Gill, 2003). Thus, in this study, we tested whether people favor companionized gifts because they assume that such gifts originate from closer others.

Method

We recruited 1,150 participants from Amazon mTurk to participate in exchange for US$0.20 (and received 1,193 participants). As in Study 1, we sought participants on Amazon mTurk to accommodate a design that requires many participants, because in this study, we manipulated a third factor: closeness of the giver. Thus, Study 4 is a 2(condition) × 2(closeness) × 3(gift) between-subjects design.

We manipulated closeness in this study because recipient-participants in our previous studies could have assumed that companionized gifts come from givers who are *a priori* socially close—that is, givers who are already close to recipients. If so, recipients could be favoring companionized gifts because, perhaps, they might believe that close others give better gifts than distant others. However, if people respond to companionized gifts that are received from close and distant others in a similar way, then it would appear as though this particular belief is not causing the companionizing effect. Because ruling out this explanation entails finding a null effect of closeness, we collected twice as many participants/condition than we collected in Studies 2 to 3, in keeping with the guidelines for testing designs where one effect is expected, and another is not (e.g., testing an attenuated interaction; Simonsohn, 2014).

The procedure was the same as Study 1, except that we used different gifts (see Table 1) and included a manipulation of the giver's closeness. We changed the beginning of the messages used in the previous studies' control and companionized conditions to indicate that the gift was received *from a close friend* or *from a distant friend*. This manipulation is a common way to vary social closeness in research on gift giving (e.g., Cavanaugh, Gino, & Fitzsimons, 2015). Finally, we measured gift evaluation with the same three items from the previous studies ($\alpha = .91$).

Results and Discussion

Consistent with Studies 1 to 3, gift was a random factor, and we conducted a 2(condition) × 2(closeness) × 3(gift) ANOVA on gift evaluation. As in Studies 1 to 3, the main effect of gift was significant, $p = .047$, and the interaction between condition and gift was not significant, $p = .518$. The only other significant effect was the main effect of condition: Participants in the companionized condition favored their gift more ($M = 5.43$, $SD = 1.35$) than participants in the control condition ($M = 4.98$, $SD = 1.51$), $F(1, 2.00) = 73.94$, $p = .013$. Importantly, the main effect of closeness was not significant, $p = .415$, nor was the interaction between condition and closeness significant, $p = .149$ (the three-way interaction was also not significant, $p = .664$). Taken together, this shows that participants favored the companionized gift regardless of whether it came from a close or distant other. That is, instead of believing that companionized gifts are more favorable because they come from closer others, we found that companionized gifts are just as favored when they come from distant others—moreover, we found this pattern in a high-powered, $\pi = .99$, sample that could have identified even a small effect of closeness. In all, these findings weaken the possibility that people favor companionized gifts because they believe such gifts come from close others, whom they might presume to choose relatively good gifts.

Study 5

In Study 5, we aimed to rule in an explanation based on balancing the unit relation that results from a mere similarity by manipulating the strength of the unit relation between giver and receiver. At first glance, manipulating closeness (as done in Study 4) might be viewed as a manipulation of unit relations. However, closeness is orthogonal to unit relations: People can have a unit relation with someone who is socially close to or distant from them. In fact, there are many ways of manipulating unit relations, and throughout the existing literature, unit relations are induced among people who are not originally close (e.g., among people who have never met). To be sure, a unit relation changes a person's *subsequent* closeness to his or her unit relation partner, but *a priori* closeness alone does not imply a unit relation, because a unit relation is based upon something that is specifically shared (be it a birthday or a material item) with someone else. Thus, we manipulated the extent that recipients share the gift with their giver.

We did this in two separate ways. In one manipulation, we removed the unit relation between the giver and recipient outright, by asking participant-recipients to imagine that someone besides the giver owns the same gifted item. This is akin to Burger et al.'s (2004) manipulation that induced whether subjects shared a name with a requester-confederate who asked for subjects' help (thus establishing a unit relation between subject and requester-confederate) or shared a name with someone depicted on a poster that a requester-confederate was carrying (thus abating a unit relation between subject and requester-confederate). In a second, separate manipulation, we weakened the unit relation by manipulating the temporal contiguity of sharing between giver and recipient. Past work has found that temporal contiguity is related to unit relations (Asch, Ceraso, & Heimer, 1960). For example, two people who graduated from the same university would have a unit relation; however, the extent to which they overlapped, from 100% to graduating several years/decades apart, weakens their unit relation. Thus, we manipulated unit relations in a second way by changing the time at which the giver bought the same item for herself.

Taken together, we expected participants in these cases/conditions to rate their gifts as less favorable than participants in the companionized condition, because such gifts, respectively, imply no unit relation or a weaker unit relation between giver and receiver.

Method

We recruited 1,450 participants from Amazon mTurk to participate in exchange for US$0.10 (and received 1,461 participants). As in Study 1, we sought participants on Amazon mTurk to accommodate a design that includes many gifts.

The procedure was the same as Study 1, except that we added two separate conditions: one that describes that the

gift is shared with, and owned by, someone who is not the gift giver (un-relational condition) and another that describes that the gift is shared at a different time—that the giver bought himself or herself the same (gifted) item last year (un-temporal condition). Specifically, these conditions' messages were the same as in the control condition except that after the control condition message, the vignettes indicated, respectively, "*I got my sister the same [gift] too!*" (un-relational condition) and "*I got myself the same [gift] last year!*" (un-temporal condition). Of note, despite the "sister" in the un-relational condition, none of our effects were influenced by gender.

As in our other studies, we used different gifts in this study, as well as both non-identical and identical gifts between our conditions. By this, we mean that the un-relational and un-temporal conditions each included six gifts (three different gifts and three of the same gifts)—for a total of nine gifts in all: three unique gifts in the un-relational condition and three unique gifts in the un-temporal condition (equaling six gifts), plus three other gifts that were the same in both conditions (see Table 1). We did this because our results so far could be accounted for by the specific gifts we have used to test our hypothesis. Although we have tried to minimize any idiosyncrasies a gift might have on our results by testing and analyzing gift as a random factor, in this study, we used both different and identical gifts to test whether our expected pattern would emerge across identical gifts. Finally, we measured gift evaluation with the same three items from the previous studies ($\alpha = .89$).

Results and Discussion

As in Studies 1 to 4, gift was a random factor, and we conducted a 4(condition) × 9(gift) ANOVA, which revealed a significant difference between conditions, $F(3, 20.98) = 8.45$, $p = .001$ (see Table 1 for descriptive statistics for each gift per condition). Post hoc analyses (LSD) revealed that participants in the companionized condition favored their gift more ($M = 5.01$, $SD = 1.32$) than participants in the control condition ($M = 4.68$, $SD = 1.48$), un-relational condition ($M = 4.42$, $SD = 1.53$), and un-temporal condition ($M = 4.72$, $SD = 1.41$), $ps < .009$. Moreover, participants in the un-temporal condition favored their gift more than participants in the un-relational condition, $p = .030$, which is consistent with our theorizing in which the un-temporal condition contains some semblance of a unit relation (albeit weakened), whereas the un-relational condition contains no unit relation. Overall, the evidence is encouraging: Because ratings among participants in the companionized condition exceeded the ratings among participants in the un-relational and un-temporal conditions, it is likely that the evaluation associated with companionized gifts results from a unit relation between giver and receiver. Finally, like Studies 1 to 4, the effect of gift was significant, $p < .001$, and the interaction was not significant, $p = .385$.

Study 6

To provide convergent evidence for our balancing unit-sentiment relations account, in Study 6, we precisely measured the unit relation by measuring feelings of interpersonal connectedness that recipients have toward givers who companionize their gifts, and tested if these feelings mediate the relation between companionizing and the evaluation receivers ascribe to their gifts.

Method

We invited 223 undergraduate students from a pool of undergraduate students who participated in exchange for extra-credit in a course. In all, 205 students agreed, which furnished roughly 100 participants in each of our two conditions. The procedure was the same as Study 1, except that we used different gifts (see Table 1) and added two items intended to measure a causal chain in support of our balancing account. These two items measure similarity ("How similar do you feel to the gift giver?") and closeness ("How close do you feel to the gift giver?"), respectively, from 1 (*not at all similar*) to 7 (*extremely similar*), and 1 (*not at all close*) to 7 (*extremely close*). This assessment enables us to test whether companionizing leads to feelings of greater similarity with the recipient (thus evidencing a unit relation), which in turn leads to feelings of greater closeness with the recipient, which finally leads to favoring the gift more. As in the other studies, the dependent variable (gift evaluation) was measured with the same three items ($\alpha = .86$).

Results

As in Studies 1 to 5, gift was a random factor, and we conducted a 2(condition) × 3(gift) ANOVA on all three dependent measures (gift evaluation, similarity, closeness). Participants in the companionized condition favored their gift more ($M = 4.66$, $SD = 1.35$) than participants in the control condition ($M = 4.38$, $SD = 1.42$), $F(1, 2.02) = 51.61$, $p = .018$. In addition, participants in the companionized condition felt more similar to gift givers ($M = 4.50$, $SD = 1.40$) than participants in the control condition ($M = 3.69$, $SD = 1.49$), $F(1, 2.00) = 25.17$, $p = .037$, and marginally closer to gift givers ($M = 4.36$, $SD = 1.54$) than participants in the control condition ($M = 3.74$, $SD = 1.59$), $F(1, 2.55) = 13.00$, $p = .069$. As in the previous studies, the effect of gift on evaluation was significant, $p = .008$, and the interaction was not significant, $p = .954$.

In a mediation analysis, we tested whether the similarity, and subsequently the closeness, created by a companionized gift accounted for the increase in gift evaluation. We tested this particular chain-order of variables because, consistent with the work on unit relations and mere similarities, feelings of similarity tend to precede feelings of closeness—that is, the feeling of closeness that is documented in the literature is dependent

upon first sharing something in common with others and subsequently feeling closer. We tested this chain using a bootstrapping procedure with two mediators (in serial order of similarity then closeness) while controlling for gift (Hayes, 2013; Model 6). One thousand resamples were taken from the data to compute the size of the indirect effect ($\beta = .33$, $SE = .11$). This indirect effect was estimated to lie within a non-zero-containing 95% confidence interval, 0.16 and 0.58, suggesting that companionizing a gift increased the felt similarity between recipient and giver, which in turn led recipients to feel closer to the recipient and subsequently like their gift more.

As a robustness check, we conducted an additional analysis that replaced the order of similarity and closeness. Although the indirect effect of companionizing was significant in this analysis as well ($\beta = .10$, $SE = .05$), the effect shrinks significantly, $p < .05$. This suggests that companionizing is more proximal to feelings of similarity, which subsequently lead to greater feelings of closeness. Overall, our predicted chain-order is consistent with a unit relations account, whereby feelings of closeness stem from feelings of similarity. Said differently, closeness alone is not sufficient to account for the companionizing effect (consistent with the *a priori* closeness manipulation in Study 4); rather, closeness by dint of similarity explains the companionizing effect. This may be due to the fact that closeness may affect behavior in both unit relations and non-unit relations, but it does not transform the latter into the former; instead, our results show that when closeness affects sentiment relations, it is an *a posteriori* type of closeness that derives from a unit relation and hence a shared similarity. This is akin to a feeling of interpersonal connectedness (in support, feelings of interpersonal connectedness in the sharing literature are sometimes measured via closeness; for example, Pinel et al., 2006).

General Discussion

Between birthdays, holidays, and anniversaries, giving gifts has become commonplace, but no gift giver wants to give a commonplace gift. Instead, givers tend to want to find the right gift that will maximize the enjoyment experienced by the recipient (Belk, 1979). Our studies offer a practical solution to this challenge: companionizing. We offered a theoretical account for companionizing, specified two boundary conditions, and enriched existing work on mere similarities, sharing, gift-giving, and Heiderian unit-sentiment relations. We have also demonstrated the applicability of the companionizing effect, which offers people an easy-to-use way to improve their gifts. Moreover, we have shown our companionizing solution boosts recipients' gift evaluation not via an inference of higher quality, effort, or *a priori* giver-closeness (Studies 2-4), but rather by generating a unit relation that is based on sharing something specifically with the giver (Studies 5-6).

Along these lines regarding valuation, a prominent prescription to emerge from the happiness literature in recent years points to the superiority of experiences over material items in enhancing satisfaction (Van Boven & Gilovich, 2003). One account for this distinction is that experiential items tend to better connect people with others (Chan & Mogilner, 2016). Although previous researchers have identified the role of sharing with others in experiences (e.g., going on a vacation), the present studies are the first to examine sharing everyday *material* items with others. In the same way that sharing helps boost happiness while consuming experiences, sharing material items could create increased evaluation of those items too, insofar as people establish a unit relation with others who share their gifted, material items. This general phenomenon has proven remarkably easy to evoke. For example, people feel stronger bonds to strangers who simply share their birthday (Miller et al., 1998). In this vein, we are reminded of Sherif's "Robbers Cave" experiment where boys were randomly divided into groups and bonded solely based on their random, superficial membership (Sherif, 1961). Given the wealth of other avenues by which people can bond with others and the powerful consequences that sharing can have (e.g., Pinel & Long, 2012; Polman & Kim, 2013), we have attempted to consider how this mediating process operates in improving gift-giving. Of course, with balancing, an alternative to changing the sentiment relation is changing the unit relation (e.g., by discarding a shared item); however, we find that people like their gifts more when companionized; thus, it is less likely that when recipients receive such gifts, they will weaken the unit relation.

We suspect that some gifts might be more or less companionize-able. In fact, some items may make better or worse gifts, but no research to date has tested the ilk of material gifts on evaluation. Besides demonstrating when companionizing occurs and when it does not, we observed significant effects of the random gift factor in all our studies, and because our gifts varied in their baseline (control condition) level of evaluation (as evidenced by Table 1), we are able to identify whether some gifts are more companionize-able than others by plotting the baseline level of evaluation against the companionizing effect (the difference in evaluation between the control and companionized conditions). We observed a linear relation that shows that the least-favored gifts show the greatest companionizing effect (see Figure 1). In all, this post hoc analysis demonstrates which kinds of gifts are more susceptible to companionizing and, consistent with balancing theory, we find that the less good gifts profit the most from companionizing. Per balance theory, when a unit relation emerges, a sentiment relation emerges in tow. In the case of receiving a relatively well-liked gift, the positive sentiment is already in balance with the unit relation, and thus, there is no need to change how much the gift is liked. However, in the case of receiving a poorly liked gift (such as a stapler), the recipient has to increase his or her liking of it in order to maintain balance. Thus, the downward linear relation is consistent with balancing theory, which predicts that unit relations will increase

liking for gifts that are initially regarded ambivalently or negatively but not for well-liked gifts.

In light of this finding, we conducted a mini meta-analysis for the primary comparison (control vs. companionizing) for each of our gifts in Studies 1 to 6. In all, we tested 31 gifts across 3,703 participants. By using the random-effects model procedure suggested by Field and Gillett (2010), we calculated the mean effect size (Cohen's *d*) of the companionizing effect (with $k = 31$ and $N = 3{,}703$). As Table 1 shows, we can be quite certain that companionizing gifts leads to higher evaluations—the mean effect size for the 31 gifts is $d = 0.279$, $p < .001$. Generally, we found that gifts get a small boost when they are companionized, though it is worth noting that by treating our gift stimuli as a random factor, our tests are particularly conservative because they account for the random stimulus variation that is typical in experimental data but usually overlooked (for a discussion, see Judd et al., 2012). Analyzing our gifts as a fixed factor, the *p* values for the differences between the companionizing and control conditions on gift evaluation become, in some cases, 10 times smaller, consistent with the notion that when researchers analyze factors as fixed (vs. random), type 1 error increases (Judd et al., 2012). Thus, even though our effect may be small, it is reliable: Of the 31 gifts that we tested in our studies, all but three gifts showed a companionizing effect.

Although the evidence for companionizing is encouraging, we do not doubt that companionizing has its limits, and more research is needed in order to fully identify the distinct precursors to and consequences (positive and negative) of companionizing. For example, in a related investigation, Aknin and Human (2015) examined a form of gifting similar to companionizing. They compared giver-centric gifts (reflecting givers' preferences) with recipient-centric gifts (reflecting recipients' preferences), finding that both givers and recipients felt closer to one another when the gift was giver-centric. Although different from companionizing (there is no essential sharing dimension in giver-centric gifts), both giver-centric and companionized gifts could be seen as gifts that remind recipients of the giver. Importantly, Aknin and Human's work shows that although giver-centric gifts cause both givers and recipients to feel closer to each other, givers prefer giving recipient-centric gifts, which implies givers might be relatively hesitant to choose companionized gifts. Similarly, perhaps givers misintuit the companionizing effect, believing that companionized gifts will be liked less and less relationship-enhancing, despite evidence to the contrary.

Recipients' attitudes toward the gift giver will also likely moderate whether a companionized gift comes to be favored more or less. Per balance theory, this broad conclusion is probably quite nuanced. Imagine a rude and racist uncle companionizing a gift for his nephew or niece. If the gift is relatively undesirable or poor, then consistent with unit-sentiment relations and balance theory, recipients are not likely to favor the gift more in this case—because relative dislike for the giver is in harmony with relative dislike for the gift. However, if the gift is exceptionally good and desirable, then recipients may positively re-balance how much they dislike their uncle. This sort of reappraisal is consistent with balance theory, as well as with our research, except instead of recipients positively reappraising a gift that is shared (as we have shown), recipients could favorably reappraise a (less-than-good) person with whom one shares, as though the uncle in this example has earned credentials by giving a desired, companionized gift.

Finally, perhaps the main limitation of the current research is the hypothetical nature of the studies. If participants were to receive gifts that had been companionized by givers, the results would be more compelling. That said, hypothetical scenarios are widely used in research on gift-giving, and people's responses to hypothetical scenarios often strongly predict actual behavior (Kühberger, Schulte-Mecklenbeck, & Perner, 2002). Moreover, by using hypothetical scenarios, we were able to test many different gifts, and the range of gifts (stimuli) used throughout our studies increases the external validity of our findings. It is rare to test this many different stimuli in one paper. Although conducting field studies is the usual way to increase external validity, it can also be increased by conducting studies containing many stimuli (Fontenelle, Phillips, & Lane, 1985). That is, investigating many gifts serves the same purpose as conducting a field study containing actual gifting behavior—both strategies increase external validity. So while our studies contained hypothetical choices, external validity is not *de facto* absent from our research.

Conclusion

In summary, our investigation suggests a rather easy way to improve the reaction and feeling one has when receiving a gift. Although the relationship between the self and possessions has been recognized for a long time (e.g., James, 1890), relatively little experimental attention has been devoted to this topic. In theoretical terms, we described why people like material items more when they are shared, and developed links between different literatures (sharing, gift-giving, unit-sentiment relations, balance theory). By drawing from these different literature streams, our research helps broaden the current understanding of mere similarities. By communicating, "I bought the same thing for myself," one can immediately turn an ordinary gift into something a little more special. Even if the gift is fairly undesirable, such as a stapler, the recipient will think that gift is more thoughtful and considerate merely because it is shared.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Supplemental Material

The online supplemental material is available at http://pspb.sagepub.com/supplemental.

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