INCREASING HELP-SEEKING BEHAVIOR AND OPPORTUNITIES

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This dissertation is dedicated to Cait Lamberton and Patti Williams, without whose brilliant ideas and constant support I would not be here today.

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ABSTRACT

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Andrew Morningstar

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Consumers are often in positions of need. These needs may be as small as help picking a product to consume, or as large as finding food and shelter. Fulfilling those needs through the assistance of capable others is help-seeking. A substantial literature exists in certain arenas of help-seeking like advice taking, but there are several places in which little is known. This proposal highlights two areas of help-seeking which have been under-examined by consumer researchers: help seeking from non-profits, and consumer decision supports.

In the first essay of this dissertation (Morningstar and Lamberton), I consider help-seeking behavior from non-profits. Almost all consumer research related to non-profits focuses on how to help non-profits raise funds. It is unknown how people in need respond to the fundraising tactics advocated by academics, especially when many are easily encountered by people in need. This essay considers one of those tactics: making non-profit clients look sad. Five experiments show that people in need interpret the use of sad people in fundraising ads as disrespectful, and are less likely to seek help when non-profits use this tactic. This occurs in five different areas of help, both when salient alternatives are and are not available. This research carries an impetus for both academics and fundraisers to re-evaluate the tactics they advocate.

The second essay of this dissertation (Morningstar, Lamberton, and Williams) considers the worth of delegation services to firms. While the ability to delegate offers many benefits to people in need of decision support, previous research suggests it would not be used often, and may be used to scapegoat. Across five experiments, I show that delegation services may indeed have worth to firms, because delegation services can shield firms from blame—when they are not used. This occurs because the presence of a delegation service is a signal of competence, and competent others are not blamed for negative experiences. Delegating, seeing firms make the negative choice, is a more diagnostic indicator of competence, so this competence signal appears only among people who have the opportunity to delegate, and do not use it.

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CHAPTER 1: SADNESS AS PROSOCIAL POISON: TACTICS THAT INCREASE HELP-GIVING MAY DECREASE HELP-SEEKING

Andrew Morningstar and Cait Lamberton

Abstract

A non-profit's mission is not only to raise money, but also disperse it to those in need. Given the general aversion to seeking help observed in past research, there is reason to believe dispersing aid is a non-trivial challenge. This research considers whether one widely used and well-researched tactic to encourage help-giving—the depiction of victims with sad faces in donation appeals—may be making this challenge even more difficult by discouraging people from seeking help. This negative effect is most robustly mediated by potential help-seekers' perception that non-profits using sad depictions respect them less than non-profits not using such depictions. Consistent with this respect mechanism, if sad expressions are framed as socially desirable (e.g., by telling people that others view displays of sadness as a mark of courage and self-love, rather than weakness), this tactic's negative impact on help-seeking is reduced. These findings enrich our theoretical understanding of responses to sadness in a charitable context, and provide practical guidance for non-profits hoping to use marketing knowledge to elicit and deliver donations. In addition, findings suggest that if researchers desire to support non-profits' whole mission rather than merely fundraising, it is critical to consider carefully their research's potential effects on help-seekers.

Introduction

Imagine that a global pandemic and job loss have led you to need help finding food. You type "help getting food in [my city]" into Google and find two non-profits, all within easy driving distance. As you browse their websites, you see prominent appeals to donate on their homepages. An appeal from one website makes you feel disrespected, while another makes you feel valued. Which non-profit would you choose?

The donation appeals displayed on such webpages may be informed by the wealth of research offering practical insights for the fundraising portion of a non-profit's mission. A non-profit's whole mission, however, is difficult to fulfill if people do not seek the help they offer. Encouraging people to seek help is a pressing problem for both public and private aid organizations. For example, almost one in five people who qualify for the U.S. Supplemental Nutritional Assistance Program (food stamps) do not apply, failing to access funding that could improve well-being and raising concerns about the program's return on investment (Cunnyngham 2021). In the private sector, the Healthwell Foundation, a medical assistance non-profit, distributed only 78% of the $510M it raised in 2020 (Forbes 2021), as 49% of people over 50 who consider their prescriptions a financial burden do not ask for help (Malani et al. 2017). In fact, despite the availability of funds that might help them, 8% of Americans ration medication rather than seeking help (LeWine 2015).

Though many factors likely contribute to the failure to access help, in this research I focus on the contributions from a common marketing tactic: depicting presumed recipients in donation appeals as sad. I find that such depictions consistently decrease peoples' likelihood of seeking help from a given non-profit. Further, I suggest the aversion to non-profit using sad faces

can be most robustly explained by inferences that non-profits using such "poverty porn" do not respect their recipients. Managers can, however, offset this effect in a way consistent with this theory: reframing the expression of sadness as socially desirable decreases its aversive power.

The present research offers theoretical and practical insights. First, fundraising appeals are often placed in areas (website home pages, billboards, grocery stores) where they can be viewed by any of their many constituencies: help-seekers, government regulators, corporate sponsors, watch groups, and employees. Despite this, research on fundraising tactics has only examined those messages' effects on donations and, to a far lesser extent, volunteering. Thus, our theoretical understanding of the effect of marketing tactics on non-profits has remained one-sided. The present research begins to augment that theoretical framework by focusing on help-seeking behavior. Importantly, I suggest that a tactic which has been shown to be optimal for one audience or purpose may be sub-optimally perceived by—even counterproductive for—another.

Second, our findings build on research identifying factors influencing help-seeking behavior (Fisher, Nadler, and Witcher-Alagna 1982; Nadler 1987; de la Rosa 2021), as well as non-experimental or domain-specific marketing research that focuses on health treatment (Jones and Murray 1996; Jones 2001; Krisjanous et al. 2013; DeVos et al. 2017; Achar, Agrawal, and Hsieh 2020). Specifically, I provide experimental tests of the effects of sad depictions among many charitable topics, in both within- and between-subjects tests. In so doing, I identify a novel and robust effect of emotional depictions on help-seeking choices, augmenting general help-seeking models in a way specific to marketing contexts.

Third, our process evidence suggests a boundary condition of past findings. Past work has emphasized the "halo effect" that protects non-profits from negative inferences (Hong and Liskevich 2015; Chernev and Blair 2021). Our results suggest that this halo is not absolute. Just

as tactics used by for-profit companies can backfire due to perceived greed (Grégoire, Laufer, and Tripp 2010), non-profits' marketing tactics can also backfire. Inferences of disrespect not only push away help-seekers, they can spark widespread negative public sentiment (e.g., Barnes 2021; Bumbaca 2020), creating long-term challenges to a given non-profit's mission.

Finally, this research may be critical in helping non-profits sustain support. Non-profits exist to help people. Low help-seeking, then, may undermine confidence in the non-profit, and thus reduce donations. Our findings may support non-profits which are attempting to increase the number of people who choose them to seek help. Further, the knowledge that tactics they use can make people in need feel disrespected may catalyze broader changes in the ways non-profits think about and interact with people in need.

I next provide a brief discussion of relevant past work in help-seeking, which allows me to offer an analysis of prior research relevant to my focal tactic—manipulating facial expressions to make potential recipients appear sad. Taking the perspective of potential help-seekers, I argue such manipulations constitute negative portrayals, and as such, their use indicates a lack of respect by the non-profit. This, in turn, reduces the likelihood that a person in need will seek their assistance.

Theoretical Background

Help-Seeking

Psychological research in help-seeking generally falls into two categories: motivations for seeking help and mechanisms to increase help-seeking. An example of the former category, reactance theory (Worchel, Andreoli, and Archer 1976) postulates that people seek help only if

they feel doing so does not constrict their freedom, either explicitly (e.g., stipulations for aid) or implicitly (e.g., pervasive social norms that one must respond to aid with gratitude). Work in the second category has identified other specific mechanisms that increase help seeking, including psychological ownership of publicly funded benefits (de la Rosa et al. 2021), similarity to the help-giver (Nadler 1987), and the induction of nostalgia (Juhl et al. 2020).

This work has usually analyzed interpersonal help-seeking (e.g., when one person asks for help on a difficult math problem from another person). Work exploring peoples' tendency to seek help from organizations tends to focus on the access of medical services, where antecedents are specific to the context studies. For example, recent research analyzes the way that construal level affects consumers' likelihood of engaging in health procedures that either detect or prevent illness (Achar, Agrawal, and Hsieh 2020). As another example, researchers have explored the effect of communication apprehension on the likelihood to engage in communication-intensive counseling programs (Jones and Murray 1996), and considered the effect of trait anxiety on responses to ads for anxiety-reducing therapy (Krisjanous et al. 2013).

Part of the reason that little empirical work has explored the effects of marketing stimuli in the context of more general help-seeking may be that few ads appeal specifically to potential help-seekers. Indeed, in a survey conducted by the authors (n = 103, Pilot 1, Web Appendix) among people who reported seeing any non-profit-related ads in the prior 30 days, 80% indicated more often seeing fundraising ads than ads inviting help-seekers. As donation elicitation ads are often placed where all —including people in need—can see them (website home pages, billboards, TV ads, etc.) our lack of a framework within which to analyze their likely effect on uptake of non-profits' services makes it difficult for us to help non-profits as they appeal to those in need. I begin the development of one part of such a framework by focusing on a characteristic

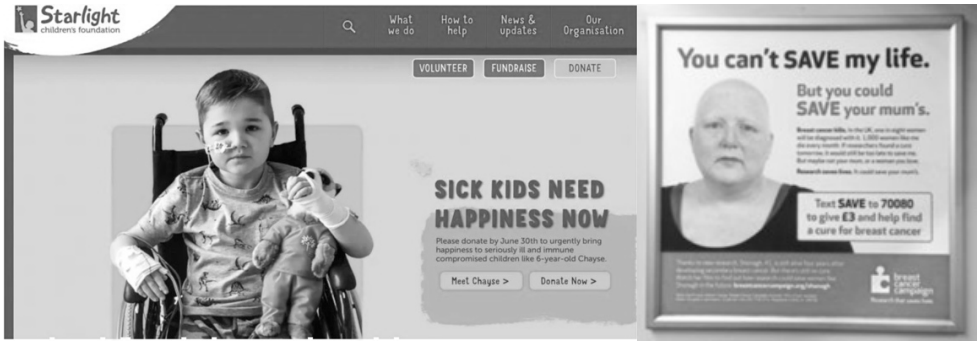
of donation elicitation ads that is ubiquitous, generalizes across contexts, and falls within marketers' control: the way in which ostensible help-recipients are portrayed in their donation solicitations.

Using Sadness to Ask for Help

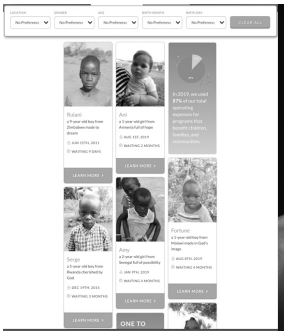
Both original research and more than one thousand subsequent citing works spanning over thirty years suggest that depictions involving sad facial expressions increase help-giving more than do depictions involving happy expressions (Eisenberg et al. 1989; Small and Verrochi 2009; Baberini et al. 2015). First, people with sad expressions—relative to happy expressions—may make the viewer feel more sympathy, thereby increasing help-giving (Eisenberg et al. 1989; Small and Verrochi 2009; Baberini et al. 2015). Second, sadness is associated with helplessness and dependency (Frijda, Kuipers, and ter Schure 1989; Lazarus 1991; Garg and Lerner 2013). If helpless and dependent people, in turn, have greater need, sadness would be expected to elicit more donations, at least under deliberative processing (Bekkers and Wiepking 2011; Cryder, Botti, and Simonyan 2017).

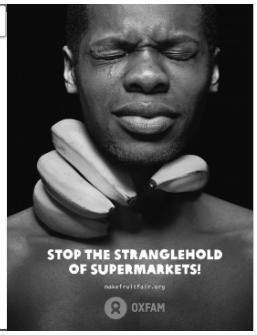
Consistent with this thinking, sad depictions of aid recipients are widely used in ads for charitable organizations. Children's non-profits provided stimuli used by Small and Verrochi (2009), while the real non-profit featured in Tucker (2014) used sad faces to elicit support for women's education in Africa. Figure 1.1 contains further examples of advertisements using this tactic for medical non-profits (Panel A); poverty-alleviation non-profits (Panel B); and environmental activism non-profits, as reflected in the famous "crying Native American" ad (Panel C). Indeed, WorldVision (the child sponsorship non-profit in Panel B) itself has acknowledged the importance of sadness in eliciting donations (WorldVision 2016). As a whole,

the tendency to visually depict the misery of the needy is so rampant it is commonly referred to as “poverty porn” (Collin 2009).

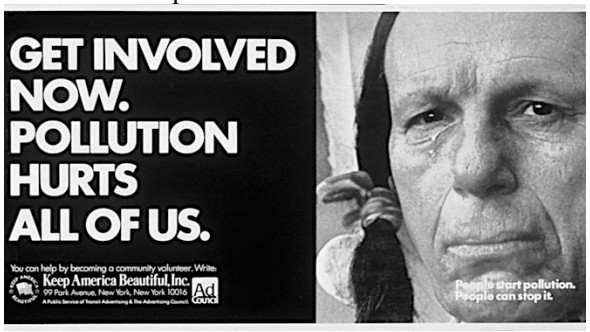


Panel A: Medical non-profits





Panel B: Poverty alleviation non-profits



Panel C: Environmental activism non-profit

Figure 1.1: Depictions of sadness in advertisements

Respect

Researchers define respect as an individual’s perception of their status within a group of people (Emler and Hopkins 1990; Tyler and Smith 1999; Rank-Christman, Morrin, and Ringler 2017). Status in this context refers to the worth or value that group members place on them (Tyler and Blader 2002; Chen et al. 2012). It does not refer to the more colloquial definition of one’s relative standing in the group hierarchy, which researchers term comparative status. Comparative status is in general not determinant of perceived respect or well-being, which is affected by respect (Tyler and Smith 1999; Tyler and Blader 2002).

There are multiple constructs that are related to, but distinct from, respect. Respect, among laymen, refers to deferential perceptions of and behavior toward people in positions of power. Power, however, is determined by comparative status, which does not determine respect in this literature (Tyler and Smith 1999). Self-esteem, defined as the global attitude one has of oneself (Rosenberg 1979), is influenced by respect: perceived respect is the mediator for the effect positive treatment by others has on self-esteem (Smith et al. 1998). Shame, an emotion that relates to one's relationships with others, may occur when people perceive others have low value of them. However, shame occurs only when people perceive themselves to be responsible for the threat to their social standing (Smith and Ellsworth 1985). It is also usually a response specifically to an act the group finds improper or taboo, and occurs not just when social status is low, but when membership to the group itself is threatened (Tangney et al. 1996). Finally, stigma can be considered an extreme form of disrespect. Loss of status is a component of stigma, but stigma also contains more egregious forms of low social status like rejection and discrimination (Link and Phelan 2001). Low respect is always present when people are stigmatized, but they do not have to be stigmatized to feel disrespected.

Emotional depictions, inferences, and help-seeking

Sadness and disrespect. While depictions of sadness may be good for non-profits' fundraising, I propose that they may not be good for the persons in need so ostensibly portrayed. This occurs at least in part because portraying people as sad is seen as disrespectful. People more readily infer negative characteristics such as incompetence and submissiveness on people who display sadness (Tiedens 2001). As a result, people are less likely to vote for sad politicians, promote sad coworkers, or hire sad interviewees than those who do not show sadness (Tiedens

2001). Further, lay beliefs suggest that sadness is a sign of weakness (Beauboeuf-Lafontant 2007; Oakley et al. 2012). Given that the way in which people are portrayed in an ad reflects how an organization wants donors to perceive those people, non-profits are promoting a negative view of people in need when they portray them as sad.

Past research suggests that actively promoting a negative portrayal would constitute a strong signal of disrespect. Previous research indicates that deliberate negative portrayals are seen as a sign that the portrayer fails to value their dignity or respect their self-presentation needs (Roy, Weibust, and Miller 2007). Further, focusing on less desirable attributes to the exclusion of a person's full range of achievements or personality leads people to feel devalued (Franklin 1999). I propose, therefore, that potential help-seekers will infer that non-profits that use sad facial expressions to depict recipients do not respect them as much as non-profits that use happy expressions. I provide further evidence that sad portrayals are perceived as disrespectful in Pilots 4 and 5, in Appendix A.

Disrespect and help-seeking. Even in the face of salient need, disrespect is likely to deter potential help-seekers. Disrespect is counterproductive to the basic motivational need to avoid harm (Maslow 1943): the experience of disrespect lowers subjective well-being (Anderson et al. 2012), negatively affects self-perceptions (Smith et al. 1998), creates cynicism and hopelessness (Stavrova, Ehlebracht, and Vohs 2020), and causes psychological trauma (Trumbull 2008). Research shows respect is crucial in encouraging engagement (Lind and Tyler 1988), such that disrespect prompts people to discontinue even relationships with substantial short and long-term benefits (e.g., education and work; Marinell and Coca 2013; Zea et al. 1997). Qualitative work suggests that aid recipients may reject help if they sense that a non-profit's staff looks down on them for being in need (Kissane 2003), which may be interpreted as disrespect. Thus, negative

emotional depictions will signal disrespect, in turn pushing help-seekers away. Formally, I will therefore test whether:

H1: People will be less likely to seek help from non-profits that depict potential aid recipients as sad than as happy

H2: The relationship between emotional depictions in ads and propensity to seek help is mediated by inferences about the non-profit's respect for help-seekers.

Attenuation: Reframing sadness' social acceptability. Emotions that may be seen as negative, however, can be reframed positively. For example, distress is generally considered a socially inappropriate emotional expression. However, work in management shows that distress can be reframed as passion, a more socially acceptable emotional expression (Wolf et al. 2016). People who reframe distress in this way, in turn, are more likely to be judged as competent, hired, or chosen as collaborators than those who do not. Building on this work, I suggest that it may be possible to reframe sadness in ways that decrease its negative connotations. Indeed, writers (Dalia 2019), business organizations (Whitener 2018), and pop singers (Jessie J. 2011; Desantis 2019) present sadness as a sign of strength rather than weakness. Based on previously mentioned research (Franklin 1999; Roy et al. 2007), providing cues that that sadness can be a socially desirable emotional expression would be a sign that non-profits value, and thereby respect, people in need. As a result, sadness may no longer push help-seekers away. Thus, marketers who can reframe sadness as a socially desirable state may reduce the likelihood that people in need will avoid the non-profit.

H3: The negative effect of sad (as opposed to happy) depictions of potential charitable recipients on the likelihood to seek help is mitigated when sadness is framed as socially desirable, as opposed to when it is not explicitly framed.

Potential contributing mechanisms. While I propose that because of its direct relationship with self-worth, inferred disrespect is a particularly powerful source of help-seeking aversion, it is also possible that the use of sad depictions triggers additional inferences and responses that contribute to effects consistent with H_1.

A first set of mechanisms relates to other inferences made about the non-profit. First, sad facial expressions may lead consumers to infer that a non-profit's aid is of poor quality, as sad depictions suggest the pictured individuals remain helpless (Frijda, Kuipers, and ter Schure 1989; Lazarus 1991; Garg and Lerner 2013). Second, as sad people appear needier than do happy people, respondents might engage in downward social comparison toward the depicted individuals, inferring that they are not as badly-off as the non-profit's typical aid recipient. People in need may avoid seeking help from non-profits that they feel are intended to serve a target market in which they do not see themselves. Third, people may infer that, as sad people appear needier, only deeply needy people will qualify for aid, and therefore that they will not be likely to be approved for aid. Thinking approval for aid is unlikely, they may be slow to apply.

A second set of mechanisms relates to a person in need's emotions. People in need may simply feel that the sad ad is not a reflection of their usual emotional state, which leads them—out of disliking for dissimilar others (Byrne 1969)—to distance themselves from the ad featuring the sad person. Second, people in need might feel ashamed if they think the sad person in the ad represents them, as shame is a self-conscious emotion aroused by salient negative social

comparisons and portrayals (Oakley et al. 2012). If the ad arouses shame, people in need will dissociate themselves from any connection to the source of shame (i.e., the sad ad) (Gross 2002).

No prior research has tested the simultaneous effects of these potential inferences alongside respect. I empirically capture all of these in experiment 2, such that I can test for their relative contribution to the negative effect of sadness on help-seeking.

Overview Of Studies

Five experiments demonstrate that sad depictions in charitable appeals decrease help-seeking from the non-profits who use them. Further, our data supports a respect-based explanation. Further, five pilot tests, located in Appendix A, establish that people in need are likely to encounter donation appeals (Pilots 1-3), and confirm that sadness is associated with disrespect (Pilots 4-5). In these studies, I follow the method and comparisons used in prior help-giving and prosocial behavior research on this effect (e.g., Eisenberg et al. 1989; Small and Verrochi 2009; Baberini et al. 2015; Zhou et al. 2021), comparing sad faces to happy faces. Experiment 1A is a within-subject field study conducted among people experiencing homelessness, showing relative aversion to homeless-supportive non-profits that use sad as opposed to happy depictions in their donation elicitation ads. Experiment 1B surveys low-income participants, who chose between free clinics that use happy, sad, or neutral depictions. The addition of this neutral condition allows us to see that the difference in help-seeking is not driven solely by a positive effect of happy depictions, but due to a negative effect of sad depictions. Experiment 2 replicates these effects in the context of food bank choices made by people who report food insecurity. In addition, results provide insight into the relative robustness

of our inferred respect-based mechanisms: I find only inconsistent evidence for the quality of help and likelihood of approval explanations, either in the version of Experiment 2 in this manuscript or in five earlier iterations of Experiment 2, provided in Appendix C. In Experiment 3, I test my predictions in a between-subjects experiment. Here, I replicate the mediation by inferred respect identified in Experiment 2, here with regard to a consequential dependent measure, capturing participants' actual opt-in to become a given non-profit's clients. Experiment 4, which follows a between-subjects design, demonstrates that framing sadness as a socially desirable expression mitigates its negative effect on help-seeking, providing support for H_3. I close with a discussion of the theoretical and practical implications of this work, as well as identification of future research that may continue to build my overall theory of help-seeking behavior, a critical complement to our understanding of non-profit-related marketing.

Experiment 1A

Methods

Participants and design.

Experiment 1A followed a two (Emotional Depiction: Sad vs. Happy) group within-subjects design (N=145). Researchers collected data in a US city where organizations provide meals to people experiencing homelessness, visiting four different sites across the city. Participants answered a one question survey in exchange for a $5 convenience store gift card.

Procedure.

Participants saw real ads for two homeless care non-profits. Participants saw their assigned pair of ads side by side, with the order of non-profit and emotional depiction randomized. All stimuli created using this paradigm are shown in Figure 1.2.

Both ads originally depicted a sad person experiencing homelessness. To keep emotional depiction and non-profit orthogonal, Photoshop was used to create a happy version of each ad, so that emotional expression and ad execution combinations could be rotated and randomized across participants. The expression manipulations were pretested among 300 participants using the same pretesting paradigm used in prior work on this topic (Small and Verrochi 2009): participants saw one of the four ads and were asked to rate how happy and sad each ad was. Results confirm that the happy version of each ad appeared happier than the sad version regardless of execution ("Give Homeless People Change" ad: M_{Happy}=4.24, M_{Sad}=.76, t=24.47, p<.0001; "John Lives Here" ad: M_{Happy}=4.42, M_{Sad}=.99, t=22.91, p<.0001) and the sad version appeared sadder than the happy version ("Give Homeless People Change" ad: M_{Happy}=1.32, M_{Sad}=4.82, t=-24.21, p<.0001; "John Lives Here" ad: M_{Happy}=1.27, M_{Sad}=4.75, t=-20.39, p<.0001).

Measures.

After viewing both ads, participants chose the non-profit from which they would prefer to seek help.[1]

Set 1 *Set 2*

Set 3 *Set 4*

Figure 1.2: Full set of stimuli pairs in experiment 1A

Results

Sample and respondent screening.

Our target sample size was 150 participants. From the four sites visited I was able to collect only 149 responses, of which four had to be screened, leaving 145 responses for analysis. In this set, one participant's response was omitted prior to analysis for non-cooperation with the survey personnel; two participants' responses were omitted prior to analysis due to interference from others; and one participant made a second response, which was omitted as a duplicate.

Help-seeking.

A chi-square model was estimated comparing the distribution of people who chose the sad or happy stimulus to a 50/50 distribution (which would occur if emotion had no impact on choice of non-profit). Consistent with H_1, participants' choice of non-profit was contingent on the emotion shown in the ad ($Choice_{Sad}$=34% vs. $Choice_{Happy}$=66%, χ^2=13.97, p=.0002).[2] Findings were robust to the inclusion of screened participants (see endnotes).

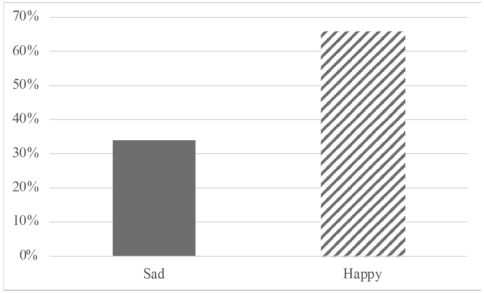


Figure 1.3: Choice of Ad in Experiment 1A

Discussion

I find that people are less likely to seek help from non-profits that depict sad (vs. happy) people in donation ads (H_1). This paradigm offers a realistic test of the effect, in a population that is in need and for whom assistance is meaningful, using real ads for non-profits that might compete for help-seekers' interest. The magnitude of this effect should be noted: among this population, participants were half as likely to elect to seek help from a non-profit when it featured a sad person than when it featured a happy person in its donation elicitation ads.

However, the focus on realism in experiment 1A may raise questions. First, while a pretest indicated that the facial expressions in the ads differed as expected, they are not standardized emotional expressions used in academic literature. To offer results more alignable with past research, I designed experiment 1B using validated sets of emotional facial expressions

(Olszanowski et al. 2015). In addition, Experiment 1A included only happy and sad faces, consistent with the vast majority of past work (Eisenberg et al. 1989; Baberini et al. 2015; Ketron and Naletelich 2019; Zhou et al. 2021). Only one paper (Small and Verrochi 2009) included an additional neutral condition, in a minority of their studies (the first two out of five). To ensure that our effect is due to aversion to sad depictions (as opposed to the strong appeal of happy depictions), I add a similarly-designed neutral comparison in experiment 1B.

Further, the situation in which ads were viewed in experiment 1A may be unlike the context wherein consumers in less extreme states of need encounter appeals for charitable donations. In a survey conducted by the authors (Pilot 2, Appendix A), participants rated various sources of information in terms of their likelihood to use when determining where to seek help. Respondents stated that they were more likely to visit non-profits' websites than to access any other source (M=5.50 on a 7 point scale, anchored at 1 ("Not at all") and 7 ("Extremely"), higher than any other source $t \geq 2.42$, $p \leq .0174$). Interestingly, while 87% of respondents said they would visit the section of the non-profit's website that was designated explicitly for help seekers, only 13% would visit that page exclusively: 71% would also visit the home page and 25% would also visit the parts of the website specifically for donors. Further, respondents stated that they would view, on average, 4.25 non-profits' websites before deciding on a non-profit (mode=3). Such a search would likely result in seeing several sad faces in charitable ads: a review conducted by an independent party (Pilot 3) of the websites of Forbes' list of 100 largest non-profits, focusing on those who provide help directly to people in need, shows that that 54% of those non-profits incorporated photos of sad help seekers in either their home page or donation request page.

Thus, in Experiment 1B, I translate my test to an online context. Here, I can create exposure to facial expressions that more closely mimics the process that many consumers experience when seeking help.

Experiment 1B

Experiment 1B studies help-seeking tendencies using a different type of non-profit (free clinics) and a sample for whom seeking help from such non-profit is both salient and realistic.

Method

Participants and design.

Experiment 1B (N=453) used a 3 (Comparison Set (between-subjects): Sad vs. Happy; Sad vs. Neutral; Neutral vs. Happy) x 2 (Emotion of target ad (within-subjects): Sad/Happy; Sad/Neutral; Neutral/Happy) group mixed design. Because free clinics, the non-profit in this experiment, usually only help low-income people, participants were prefiltered by a fielding partner to those making less than $20,000/year. This cutoff was chosen because it is consistent with the approximate income eligibility cutoff for a household of one for many forms of income-based state government assistance (150-160% federal poverty level, $19,320-$20,608) (c.f., e.g., Pennsylvania SNAP Handbook, 512.1 General Policy; State of Louisiana LIHEAP Detailed Model State Plan, Assurances Section, Subsection 2.B.i). Qualified participants were paid a nominal fee to complete the survey.

Procedure.

Participants imagined that, on finding that they needed unaffordable medical treatment, they searched for nearby free clinics, finding two. Participants were told they visited each

website, one by one, wherein they were shown a fundraising ad which featured prominently on that non-profit's home page. As in experiment 1A, the order of differences in ad execution (ad language, background, organization name, specific person, t-shirt color, background color) were orthogonally randomized. Participants were randomly assigned one of three sets of emotion comparisons: Sad vs. Happy (N=148), Sad vs. Neutral (N=151), and Neutral vs. Happy (N=151). An example set of stimuli is in Figure 1.4.

Measures.

Participants indicated which non-profit they would go to for help as the dependent measure.

Example configuration: happy vs. sad

Figure 1.4: Example set of experiment 1B stimuli

Results

Sample size and respondent screening.

I set a target sample size of 150 per between-subject condition. Participants were screened if they reported remembering having completed a similar study (n = 13). Screened participants were replaced and randomly assigned to conditions prior to inclusion in the dataset,

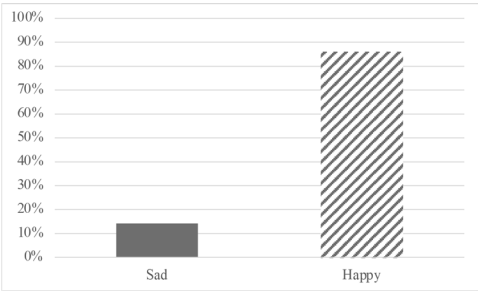
to obtain our target sample of a total of 450: 148 in the Sad vs. Happy comparison set; 151 in the Sad vs. Neutral comparison set; and 151 in the Neutral vs. Happy comparison set.

Confound check: Ad execution.

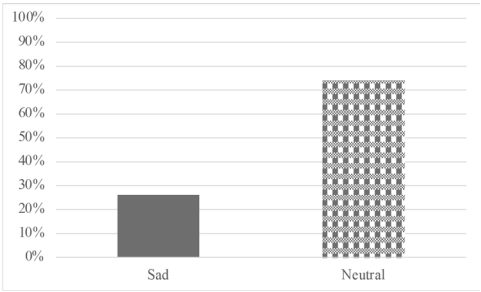
I first tested to see whether non-focal differences in ad executions explained participants' choices. A logistic regression—predicting whether the sad face was chosen, using other elements of the ad executions as IVs – suggested that variation in ad execution elements did not explain participants' choices. No significant differences based on ad execution elements emerged for the Sad vs. Happy comparison set (*ps*≥.06), the Sad vs. Neutral comparison set (*ps*≥.18), and the Neutral vs. Happy comparison set (*ps*≥.26).

Help-seeking.

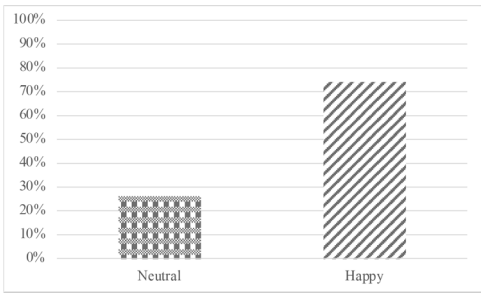
Chi-square tests indicate that, as in experiment 1A and consistent with H_1, participants were less likely to select to seek help from the non-profit whose ad featured a sad as opposed to happy facial expression ($Choice_{Sad}$=14% vs. $Choice_{Happy}$=86%, χ^2=78.81, p<.0001). I find that the sad depiction drew significantly fewer potential help-seekers even when compared to a neutral face ($Choice_{Sad}$=26% vs. $Choice_{Neutral}$=74%, χ^2=33.38, p<.0001). People were less likely to select the non-profit that depicted a neutral than happy person ($Choice_{Neutral}$=26% vs. $Choice_{Happy}$=74%, χ^2=35.29, p<.0001).[3]



Sad vs. Happy condition



Sad vs. Neutral condition



Neutral vs. Happy condition

Figure 1.5: Choice of ad in Experiment 1B

Discussion

Experiment 1B's results replicate the finding that people are less likely to choose a non-profit to seek help from when that non-profit depicts a sad as opposed to happy person in their ad. The effects in this study were larger than in experiment 1A, with happiness preferred more than six to one. Importantly, I also show that this effect emerges relative to a neutral condition. This comparison allows us to see that prior findings are not simply driven by the attractiveness associated with happiness, but that sadness in fact decreases help-seeking relative to a neutral depiction.

To this point, I have not tested my proposed respect-based process or addressed potential alternate or contributing mechanisms. Experiment 2 was designed to both replicate my findings and consider these aspects of my theory. I test not only for the role of respect in this help-seeking phenomenon, but also the five alternative mechanisms mentioned in the theory section of this manuscript. These alternative mechanisms, to reiterate, are first that people in need might infer that the non-profit utilizing sad people in their ads offer inferior quality of help (the "Quality of Help" mechanism). Second, people in need might not seek help because they feel they are not the non-profit's true target audience, or believe seeking help from that non-profit would take help

away from others with greater need (the "Downward Social Comparison" mechanism). Third, if someone believed that the non-profit featuring sad people helps people with greater need than they have, they might feel that they would be less likely to be approved for help if they apply (the "Likelihood of Approval" mechanism). Fourth, most participants might feel that the sad ad is not a reflection of their usual emotional state (the "Emotional Similarity" mechanism). Fifth, participants might feel ashamed if they think the sad person in the ad represents them and will dissociate themselves from any connection to the source of shame (the "Shame" mechanism). All of these, along with our respect-based explanation, are tested simultaneously in Experiment 2, in the context of food bank choices.

Finally, while the participants in Experiment 1B would qualify for help, there is no guarantee all participants would in fact feel that they were in need. If participants actually felt they were in need, they may be likely to only focus on how well non-profits can alleviate their need, nullifying our effect. To account for this, in Experiment 2 I both focus on a sample that would qualify for help and among whom need is salient. This is a more conservative test of the effect.

Experiment 2

Method

Participants and design.

Experiment 2 followed a two (Emotional Depiction: Sad vs. Happy) group within-subjects design. Participants were Prolific panelists pre-filtered to have an annual income less than $20,000 (N=112), who completed the survey in exchange for $1.00.

Procedure.

The procedure for experiment 2 was similar to experiment 1B: participants read that they searched online for nearby non-profits (in this case, food banks), and found two. For each non-profit they were told they visited the non-profit's site, at which point they were shown an ad featured on that non-profit's home page. After the scenario they indicated which non-profit they would seek help from. They then completed the measures for perceived respect and the alternative explanations for each of the two ads in random order. Finally, they rated their feelings of shame in the present moment. To test the Emotional Similarity interpersonal mechanism without risking contagion by negative reactions to the emotional stimuli presented, in a survey two weeks prior the participants were asked how often they generally felt sadness.

As in previous experiments, ad execution elements were fully randomized and rotated. Aside from the emotion in the ad, the ad execution differences, used to create realistically different depictions, included ad language and background, the organization name, and the person pictured. To support generalizability and coherence with a broader range of prior work, a new set of standardized emotional expression stimuli was used for this experiment (Hess, Blairy, and Kleck 1997). A sample stimulus set is in Figure 1.6.

Measures.

As in previous experiments, help-seeking was measured by asking participants to choose which non-profit they would go to for help. After participants made their selection, they completed a set of measures related to both of the ads they viewed. These measures, covering the first four mechanisms outlined in Table 1.1, were captured for each ad viewed using Strongly disagree (1) to Strongly agree (7) scales. Thus, for these four mechanisms I had within-subjects measures of each potential mechanism. My focal mediation measure of inferred respect was

adapted from Rank-Christman, Morrin, and Ringler (2017) with a focus on positively-worded items (Schmitt and Stuits 1985).

To measure the Shame mechanism, at the end of the experiment participants rated their feelings in that moment on a series of descriptors including "ashamed". To test the Emotional Similarity interpersonal mechanism without creating contagion by reactions to the stimuli presented, in a survey given two weeks prior, participants rated their agreement with the statement "in general, I feel _____" for a list of emotions including "sad". The Emotional Similarity mechanism was rated from 1 ("Not at all") to 7 ("Very much"); the Shame mechanism was rated from 1 ("Not at all") to 5 ("Very much").

Construct	Measure
Inferred Respect α=.96	"This non-profit makes me feel [respected/appreciated/valued/important]"
Inferred Quality of Help α=.93	"People who seek help from this non-profit get all the help they need", "People who seek help from this non-profit have good outcomes", "People who seek help from this non-profit are successful in seeking help".
Downward Social Comparison α=.83	"I am in a better position than the person in the ad", "The person in the ad needs help more than I do", "Other people could make better use of this non-profit's help than I could"
Likelihood of Approval α=.94	"This non-profit is not for people like me", "This non-profit would not accept me if I applied", "I don't think I would be eligible for these benefits".
Shame	Please rate the extent to which you are feeling each of the following right now: [ashamed]
Emotional Similarity	Please rate how much you agree or disagree with the following statements: in general, I feel [sad] (asked 2 weeks prior to the experiment)

Table 1.1: Mediators tested in Experiment 2

Figure 1.6: Example ad stimulus set in experiment 2

Results

Sample size and respondent screening.

While all participants would qualify to receive help from a food bank given the income limit set as in experiment 1B, to ensure participants actually had need for a food bank, they rated their agreement with the statement "I sometimes am not sure I'll be able to afford all the groceries I need (for a healthy, active lifestyle) with the money in my bank account, without help from others." Ratings were from 1 ("Strongly disagree") to 7 ("Strongly agree"). The statement corresponds with the definition of food insecurity given by the United Nations' Committee on World Food Security. Only participants who reported subjective food insecurity (rated 4 or higher) were retained. For the first survey, in which I measured the Emotional Similarity mechanism, I collected 250 responses, on the advice from a fielding partner that I could expect a response rate to the main survey of about 67% of those respondents. Of the 250, I received 167 responses to the second survey. Of those, 35 were screened out prior to analysis for not reporting salient food insecurity, 19 were screened for remembering taking a similar survey, and one was removed for not giving a valid Prolific ID that could be matched between the two surveys. In the end I collected 112 responses. Results are robust to including screened respondents.

Help-seeking.

A chi-square analysis estimated whether participants' likelihood to choose a non-profit was contingent on the emotional expression in the ad. Consistent with prior findings and H_1, help-seekers were less likely to choose the non-profit ad featuring the sad (vs. happy) person ($Choice_{Sad}$=31% vs. $Choice_{Happy}$=69%, χ^2=15.75, p<.0001).[4]

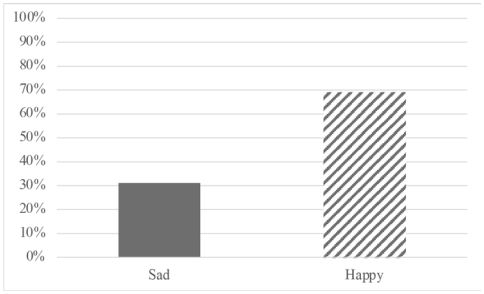


Figure 1.7: Choice of ad in Experiment 2

Inferred respect.

Help-seekers inferred less respect from the non-profit using a sad emotional depiction ($Respect_{Sad}$=3.75 vs. $Respect_{Happy}$=4.58, t=-5.63, p<.0001).[5]

Quality of help.

Help-seekers felt the non-profit that featured someone sad in their ad offered lower quality help (QoH_{Sad}=4.23 vs. QoH_{Happy}=4.93, t=-5.11, p<.0001).[6]

Downward social comparison.

Help-seekers made more downward social comparisons to the person in the non-profits' ads when that person had a sad facial expression (DSC_{Sad}=4.41 vs. DSC_{Happy}=3.87, t=4.61, p<.0001).[7]

Likelihood of approval.

Help-seekers felt a non-profit was less likely to approve them for aid when that non-profit featured someone sad ($Approval_{Sad}$=4.42 vs. $Approval_{Happy}$=4.88, t=-4.80, p<.0001).[8]

Parallel mediation.

Within-subjects mediation was measured following procedures established in Judd, Kenny, and McClelland (2001): I estimated a logistic regression with non-profit choice as the DV and each of the mechanisms as IVs. For the mechanisms measured for both ads—Respect, Quality of Help, Downward Social Comparison, and Likelihood of Approval—the mechanisms were entered both as a sum score, with the ratings for both ads summed, and as a difference score, with the rating for the happy ad subtracted from the rating for the sad ad. Mediation in these mechanisms is determined by the p-value for the difference score (Judd et al. 2001; Winterich, Mittal, and Ross Jr. 2009). P-values less than 0.05 indicate mediation. Likelihood of Approval ($Estimate_{Likelihood\ of\ Approval}$=-1.13; χ^2=6.53; p=.0106) partially mediated the relationship between emotional depiction in the ad and help-seeking. However, after accounting for this effect, inferred respect still presented a significant mediation path ($Estimate_{Respect}$=-1.08; χ^2=8.14; p=.0043) (H_2). None of the other alternative explanations provided evidence consistent with mediation ($Estimate_{Quality\ of\ Help}$=-0.63; χ^2=3.17; p=.0751; $Estimate_{Downward\ Social\ Comparison}$=0.27; χ^2=0.75; p=.3868; $Estimate_{Emotional\ Similarity}$=0.27; χ^2=2.39; p=.1223; $Estimate_{Shame}$=-0.11; χ^2=0.07; p=.7843). Findings are robust to the screening criteria ($Estimate_{Respect}$=-0.98; χ^2=11.66; p=.0006; $Estimate_{Likelihood\ of\ Approval}$=-0.67; χ^2=4.57; p=.0326; $Estimate_{Downward\ Social\ Comparison}$=0.34; χ^2=2.00; p=.1570; $Estimate_{Shame}$=0.01; χ^2=0.00; p=.9804), except Quality of Help and Emotional Similarity, which became significant ($Estimate_{Quality\ of\ Help}$=-0.71; χ^2=7.94; p=.0048; $Estimate_{Emotional\ Similarity}$=0.32; χ^2=5.43; p=.0198).

Construct	Rating	Within-Subjects Mediation
Respect	*Sad Ad:* 3.75 *Happy Ad:* 4.58 t=-5.63 p<.0001	Estimate=-1.08 χ^2=8.14 p=.0043

Quality of Help	*Sad Ad:* 4.23 *Happy Ad:* 4.93 *t*=-5.11 *p*<.0001	Estimate=-0.63 χ^2=3.17 *p*=.0751
Downward Social Comparison	*Sad Ad:* 4.41 *Happy Ad:* 3.87 *t*=4.61 *p*<.0001	Estimate=0.27 χ^2=0.75 *p*=.3868
Likelihood of Approval	*Sad Ad:* 4.42 *Happy Ad:* 4.88 *t*=-4.80 *p*<.0001	Estimate=-1.13 χ^2=6.53 *p*=.0106
Emotional Similarity	N/A	Estimate=0.27 χ^2=2.39 *p*=.1223
Shame	N/A	Estimate=-0.11 χ^2=0.07 *p*=.7843

Table 1.2: Mediator results in Experiment 2

Discussion

Results replicate our prior finding that people are less likely to seek help from non-profits when they display sad (vs. happy) people in their ads. In addition, results suggest that inferred respect plays an important role in driving this effect. Even after accounting for potential inferences about likelihood of approval, people in need who see non-profit ads using sad people feel less respected by the non-profit, which makes them less likely to seek help. I do not detect evidence that aversion to the non-profit using sad depictions was driven by perceived quality of help, downward social comparisons, emotional similarity, or felt shame—at least in this context, and when respect is taken into consideration.

While I did find that likelihood of approval mediated choice of non-profit alongside respect, previous data do not suggest it is a robust mechanism. Altogether, I have sought conceptual replication of this effect across six iterations of Experiment 2; the previous five are in Appendix C. In all six iterations I tested the Respect and Quality of Help explanations; in three I

also tested Likelihood of Approval and Downward Social Comparisons; and in two I tested Emotional Similarity and Shame. In all iterations I find evidence of mediation through inferred respect. In contrast, I find mediation through likelihood of approval only once, and evidence supporting mediation through Quality of Help three times. Taken together, the data suggest that while there might be multiple mechanisms through which emotion in non-profit ads determines help seeking, some may only mediate under specific circumstances, or carry indirect effects too small to be consistently detected. I return to this point in the General Discussion. As inferred respect constitutes a robust mediator across studies, I take it as my focus in the next experiment. I also continued to collect data related to Quality of Help, in order to further test for its role – and whether my respect-related process remains robust even when it is accounted for.

Experiment 3

While my survey data support the idea that many help-seekers will browse multiple sources before choosing between non-profits, between-subjects designs may be useful for the rare cases when people in need are specifically given an offer of help from one non-profit or are evaluating only one non-profit. Experiment 3 was designed to replicate prior effects using a third type of non-profit—a resource directory similar to 211 and HomelessSheltersDirectory.org. I also measure participants' choice to sign up to be a client of the non-profit, thus capturing their actual help-seeking behavior.

Method

Participants and design.

Experiment 3 followed a two group (Emotional Depiction: Sad vs. Happy) between-subjects design using Amazon Mechanical Turk panelists (N=150). Participants completed the survey in exchange for $0.75.

Procedure.

Participants read about Diamond Outreach, a purportedly real non-profit that provides help to gig workers, including those who earn income via platforms like MTurk. Participants read that Diamond Outreach makes available a directory of information about local non-profits for its clients. They were provided examples of the types of non-profits included in this directory (e.g., legal non-profits, medical non-profits). Participants were then shown an ad soliciting donations for Diamond Outreach, depicting either a happy or a sad person, using facial expressions previously validated in Tottenham et al. (2009) (Figure 1.8). Participants were asked about help-seeking afterward, rated perceived respect and quality of help, then answered a few filler questions.

Measures.

After seeing the ad, participants were told that they had the opportunity to seek help from Diamond Outreach now. They were told if they said "yes" to seeking help, their email addresses would be collected at the end of the survey and sent to Diamond Outreach to put on an official list of help-seekers. Answers to this question constituted the dependent variable. After participants made their selection, they filled out the respect and quality of help measures used in experiment 2.

Figure 1.8: Experiment 3 stimuli

Results

Sample size and respondent screening.

My target final sample was approximately 150 after screening (n = 4), as in previous experiments.

Help-seeking.

A chi-square analysis estimated the relationship between the emotion shown in the non-profit's ad and participants' choice whether to seek help. Help-seekers were not less likely to seek help when the person in the ad they saw was sad ($Seeking_{Sad}$=17% vs. $Seeking_{Happy}$=22%, χ^2=0.44, p=.5086).[9]

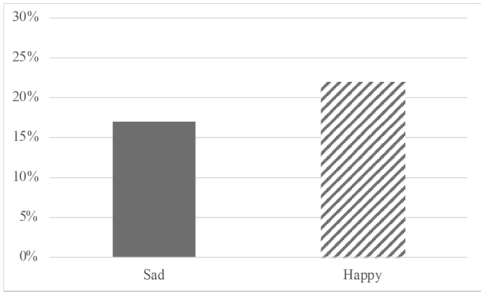


Figure 1.9: Likelihood to seek help in Experiment 3

Inferred respect.

Consistent with expectations, help-seekers inferred less respect from the non-profit when their ad featured a sad, as opposed to happy, person ($Respect_{Sad}$=3.43 vs. $Respect_{Happy}$=4.46, t=3.87, p=.0002).[10]

Quality of Help.

Help-seekers inferred lower quality of help from the non-profit when their ad featured a sad, as opposed to happy, person (QoH_{Sad}=3.68 vs. QoH_{Happy}=4.20, t=2.82, p=.0055).[11]

Parallel Mediation.

A bootstrapping mediation model (PROCESS Macro Model 4) was estimated with the emotional depiction in the ad as the independent variable, inferred respect and quality of help as the mediators, and help-seeking as the dependent variable. The decision to seek help was mediated by respect (H_2) (Indirect Effect=-.50; SE=.21; 95% CI=[-1.01, -.21]; Direct Effect=.23; SE=.25; p=.3454).[12] I do not detect mediation through inferred quality of help (Indirect Effect=.06; SE=.09; 95% CI=[-.10, .28]).

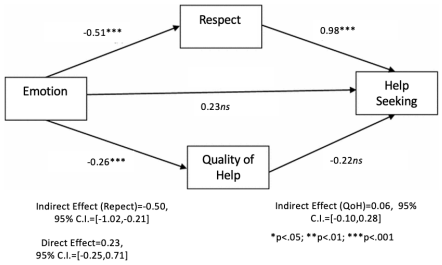


Figure 1.10: Mediation results of Experiment 3

Discussion

The results of experiment 3 reinforce our finding that the emotional depiction of people in ads has an effect on help-seeking through its effects on non-profits' perceived respect for recipients: people felt less respected by the non-profit when their ad included a sad (vs. happy)

person, which made them less likely to seek help from that non-profit (H_2). It should be noted that, while I did not see in aggregate that my emotion manipulation affected participants' likelihood to seek help, I did find full mediation through Respect, with no direct effect. As noted in seminal methodology guidelines (Zhao, Lynch, and Chen 2010), only the mediation is necessary to establish an effect. There may be several reasons for the lack of an aggregate effect. The most likely explanation is that the direct effect, though insignificant, was large and in the opposite direction to the indirect effect. This would have occluded the effect due to respect and made the total effect smaller and harder to detect through statistical testing. Compounding this, mediation analyses may have far greater power than analyses of the total effect (O'Rourke and MacKinnon 2015), The pattern of results in this experiment is not uncommon: in simulation studies, the lack of a total effect is the largest source of Type II errors in mediation (MacKinnon et al. 2002). It should be noted that this experimental paradigm was studied once before, and in the earlier version, the data showed significant aggregate differences in help-seeking. This replication is included in Appendix B.

I did not detect evidence that inferred quality of help mediates in this experiment. While participants did believe the non-profit offered lower quality help when it featured a sad person, quality of help was not related to help-seeking. Therefore, I move forward with a focus on the process carried through inferred respect only.

Experiment 4

Experiment 4 tests H_1 again in a between-subjects paradigm. In addition, I provide evidence for my theorized mechanism by showing moderation relevant to the more robust

process identified in our prior studies. In this experiment, I consider likelihood to seek help from medical debt assistance non-profits. Such non-profits usually work only with low-income clients; therefore, I test this scenario among Prolific respondents using the same income filter as in experiments 1B and 2. I also recruit a larger sample to account for the decreased power in detecting interaction vs. main effects (Wahlsten 1991; Brookes et al. 2004). Since based on prior work, negative portrayals engender perceptions of disrespect (Franklin 1999; Roy et al. 2007), I test moderation in this study by framing sadness – ordinarily seen as a socially undesirable emotion (Tiedens 2001)—in a positive light.

Method

Participants and design.

Experiment 4 followed a two (Emotional Depiction: Sad vs. Happy) x two (Sadness Framing: Desirable vs. Control) between-subjects design. Participants from Prolific (N=600), prefiltered as in prior experiments to have income of less than $20,000 per year, completed this survey in exchange for $1.00.

Procedure.

Participants imagined that a few weeks after a hospital stay, they received a $1,500 medical bill for expenses that were not covered by insurance. Participants read that the hospital had a payment plan for the bill. All read that the payment plan required would be disruptive to their month-to-month budget. The bill also informed them that the hospital had a partner non-profit which helps people with part of their medical bill costs, and if they applied for help, they would likely be approved. Participants read that they went to the partner non-profit's website to learn more and saw an ad which included a happy or sad emotional depiction, randomly assigned

as in prior studies (Figure 1.11). Participants indicated how likely they would be to seek help from that non-profit and answered filler questions.

To manipulate the framing of sadness' social acceptability, participants read a news article before the scenario as part of an ostensibly unrelated survey. The articles were from a newspaper called *The Owl*, which specializes in reporting about scientific research. For half of participants (Sadness Framing: Desirable condition), the article covered a research report that framed the expression of sadness as socially desirable. The report said that people openly expressing sadness were viewed as bravely confronting the belief that sadness should not be expressed. As a result, expressing sadness was seen as a sign of courage, strength, and self-love. Because of those traits, people were more likely to include sad people in experiences and conversation, and would pay more for the services of people who express sadness. For people randomly assigned to the Sadness Framing: Control condition, the article covered the positive repercussions of plant ownership. The positive repercussions of plant ownership resulted from the perception that plant owners are able to care for someone or something else. The positive repercussions of plant ownership were parallel to those in the sadness reframing article. Participants answered filler questions about the article before proceeding to the medical bill scenario.

Measures.

Participants reported their likelihood to seek help by rating "How likely would you be to ask for help from this non-profit?" from 1 ("Not at all likely") to 7 ("Extremely likely").

Figure 1.11: Experiment 4 stimuli

Results

Sample size and respondent screening.

In this study, I increased the sample size per cell twofold from that used in experiment 3, resulting in a target of 600 participants after screening (150 per cell). In this study in addition to filtering respondents who might have remembered completing a similar study, I employed a more stringent set of screening criteria, in response to increasing concerns about the quality of data from online panels (Chmielewski and Kucker 2020). I developed the first screening criterion based on best practices for capturing inattention: asking participants to rate impossible or nonsensical statements as true or false (Meade and Craig 2012), or looking for objectively inconsistent responses (McKibben and Silvia 2016). To this end, at the end of the survey participants answered a series of true/false questions (e.g., "I eat meat"). Two of the statements were "I have run a mile in 30 seconds or less" and "I have never run a mile in 30 seconds or less". As the first statement is impossible, and the second an inverse of the first, the only correct answers would be false and true, respectively. Participants who failed to answer as such were screened prior to inclusion in the dataset. For the second check, participants on screens with stimuli sometimes move to the next screen without viewing the stimuli as a means of finishing the survey more quickly. As a means of testing how long participants, at a minimum, should be

spending on pages with stimuli, the first author tested the amount of time needed, immediately after a screen had loaded, to find, move the cursor to, and click the "next" button. On average, this took 2.5 seconds. An analysis of the authors' previous data from earlier experiments indicated that a non-trivial number of respondents spent less than 2.5 seconds on stimuli screens. As a means of combatting the decreasing quality of responses on online survey platforms (Chmielewski and Kucker 2020), the authors instituted a new attention check: if screen timing data showed advancement before 2.5 seconds had elapsed, responses were removed prior to analysis.

671 responses were collected. Of this group, 17 were screened prior to inclusion in the dataset for failing the impossible/conflicting statement check, 18 were screened for failing the stimuli screen check, 30 were screened prior to analysis for remembering taking a similar study, and six were screened prior to analysis for failing multiple checks. This left a final sample of 600.

Help-seeking.

A two-way ANOVA yielded a significant interaction effect of Emotional Depiction and Sadness Framing (F=4.22, p=.0403) (H_3). As expected, in the control framing condition, participants said they would be less likely to seek help when the non-profit featured a sad person in their ad ($Seeking_{Sad|Control}$=4.31 vs. $Seeking_{Happy|Control}$=4.81, F=5.02, p=.0254) (H_1). When the article they read framed sadness as socially desirable, however, the difference became non-significant ($Seeking_{Sad|Desirable}$=4.79 vs. $Seeking_{Happy|Desirable}$=4.64, F=.46, p=.4994).[13]

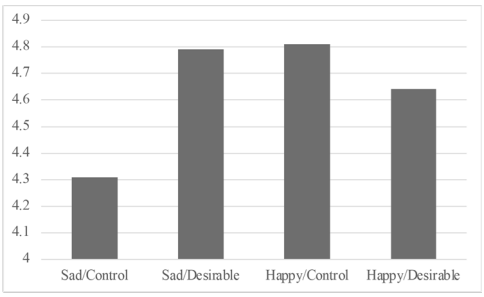


Figure 1.12: Likelihood to seek help in Experiment 4

Discussion

Results again suggest that people are less likely to seek help from non-profits which include photos of people with sad expressions in their ads, as opposed to happy expressions. This experiment further bolsters our theory by showing moderation: as negative portrayals like stigmatized sadness communicate disrespect, reframing sadness as a socially desirable expression, turning it into a positive portrayal, would attenuate the effect.

General Discussion

There is a rich literature on techniques that prosocial entities can use to increase help-giving behaviors (c.f., Bekkers and Wiepking 2011). The present manuscript attempts to add a complementary discussion, demonstrating that at least one of these techniques—featuring sad people in non-profits' ads to request donations or volunteers—can decrease help-seeking. I further show that this happens because the tactic makes people in need believe the non-profit respects them less. Experiments 1A and 1B suggest that people are less likely to seek help from non-profits when their donation request includes a sad face—as opposed to happy, consistent with previous research on the effect. Experiment 1B includes an additional neutral expression,

showing even when the comparison is neutral people prefer not seeking help from non-profits that make them appear sad. Experiments 2 and 3 replicate the findings of experiments 1A and 1B. In addition, results provide evidence that this occurs at least in part because they infer that the non-profit does not respect them. Respect explains results even when accounting for other factors that might contribute to a similar aversion to help-seeking, none of which are consistently supported as mediators in our data. Finally, experiment 4 provides a way to ameliorate this negative effect, by reframing sadness to be a socially desirable emotional expression, rather than a negative portrayal. Taken together, this work offers causal evidence related to the relationship between emotional depictions, perceived respect, and help-seeking, across multiple populations and non-profits, and in both within-subject and between-subject designs, suggesting a robust effect that can be moderated in theoretically relevant and practically applicable ways.

Theoretical and Practical Implications

The current research builds on previous theory in several ways. First, it draws attention to the differential impact of stimuli among the different populations that interact with non-profits. Previous research shows that seeing sad faces in fundraising appeals has a positive impact on non-profits through increased donations. Our research, however, suggests positive reactions might be limited to donors. Among people in need, seeing these depictions sparks feelings of disrespect and avoidance of the non-profit. That fundraising research has, until now, focused on help-givers (donors and, to a lesser extent, volunteers) may explain why previous researchers have not noticed this. I show that focusing on a broader range of people who interact with non-profit advertisements, including and especially people who are not the target audience, offers insights of use to marketers.

Second, the effect that inferred disrespect has on help-seeking has implications for the "halo effect" that prosocial organizations enjoy. Research indicates that organizations who do good are often shielded from negative inferences (Hong and Liskevich 2015; Chernev and Blair 2021). This manuscript indicates that the halo does not extend infinitely, as help-seekers may infer that non-profits do not respect them depending on the fundraising tactics they use. This provides an impetus for researchers to investigate any negative effects their tactics have on non-profits.

Third, my findings on disrespect may highlight a means by which people stay trapped in a state of need. The prospect of judgment by one's peers is a powerful determinant of whether people seek help (Vogel and Wade 2009). However, in cases where people have little choice but to seek help from a non-profit, advertisements that create feelings of disrespect would reinforce help-seekers' sense of stigmatization: not only are they judged negatively because they seek help at all, these judgments come from a non-profit that has reduced them to a sad depiction. As disrespect undermines self-efficacy, it is at least possible that such doubly-demeaning experiences could contribute to long-term disempowerment.

Finally, I provide important theoretical insights that extend beyond work focusing on interpersonal or therapeutic help-seeking. Non-profits interact with people in ways that the aforementioned do not (e.g., fundraising ads), and can change lives in highly impactful ways that consumers may otherwise have little ability to find (e.g., free legal assistance). Showing how fundraising ads affect people in need hopefully leads to further research on the unique impacts that non-profits and other charitable organizations have on potential help-seekers.

Practically, my findings suggest that non-profits may wish to reconsider the use of sad faces in advertisements. I acknowledge that this recommendation may meet with resistance.

After all, very few prospective donors might understand why earthquake survivors would be smiling in a charitable appeal. Fortunately, our research suggests that sadness can be reframed in ways that reduce its negative effect on help-seeking. Non-profits might do well to reframe sadness through concerted campaigns to reverse the general stigma associated with it. Even if they choose not to undertake this campaign, it is easy to find ways to portray help-seekers as happy, no matter how dire the subject matter. The clearest example is to show them being helped, and how happy they are to receive support.

More generally, this work calls for more robust marketing research related to solicitation tools that many non-profits display publicly. The home pages of non-profits' websites are usually targeted to help-givers, but are nearly inescapable for help-seekers. Many other vehicles through which non-profits solicit donations—billboards, flyers, TV ads, influencer endorsements, and so on—are seen both by potential givers and by people with needs. That these donation appeals might hinder non-profits in benefiting people with needs is concerning, especially considering helping people with needs is arguably the most important aim for prosocial organizations.

Limitations and Future Research Directions

Our findings include limitations that open doors to future research. First, I have focused on the way that inferred disrespect translates into the choice to begin seeking help. However, I have not considered other downstream consequences of initial inferences of disrespect, either specific to the helping non-profit or in the recipient's life as a whole. Given past findings supporting a relationship between experiences of disrespect and low self-efficacy (Honneth 1992) as well as low organizational commitment (Lind and Tyler 1988; Marinell and Coca 2013; Zea et al. 1997), I may anticipate that initial perceptions of disrespect would lead to weaker long-term engagement in help-seeking programs as well as, potentially, unwillingness to access other

resources. Marketing researchers may advance both theory and practice by exploring the mechanisms underlying these effects, as well as means by which they can be addressed. Researchers may also consider what marketing tools or aspects of customer experience may help non-profits communicate respect for their clients.

Another opportunity relates to the study of other emotional expressions on help-seeking. In our research, I focused on sadness, in order to speak most directly to research and current practice. However, future work could examine the use of other emotional expressions, like pride, love, anger, or fear. Increased examination of neutral faces may also be warranted, since they are rarely considered emotionless (Leppänen et al. 2003), and can be misinterpreted because of proximal cues such as text (Fernández-Dols et al. 2008). Future work may further explore the interplay between graphical and textual elements, as well as other message-related factors such as surrounding information, mode of delivery (e.g., mobile v. desktop v. analog), or arousal level created by the ad itself.

Third, given the effects of sadness observed in my studies, it may be fruitful to re-evaluate gender and help-seeking. The viewpoint that sadness is weakness seems to be particularly embraced among men following "traditional" gender roles (Beauboeuf-Lafontant 2007). However, the association of sadness with weakness occurs in women as well (Beauboeuf-Lafontant 2007; Oakley et al. 2012). My findings do not support greater sensitivity to disrespect among men. In only two experiments did I find an interaction of gender with help-seeking (experiment 1B: χ^2=.6.92, p=.0085; experiment 2: χ^2=7.73, p=.0054; experiment 3: χ^2=.62, p=.4310; experiment 4: F=.95, p=.3315), and those relationships ran opposite to expectations, with men more likely to choose the sad ad than women (Experiment 1B: $Seeking_{Male|Sad}$=22%; $Seeking_{Male|Happy}$=78%; $Seeking_{Female|Sad}$=9%; $Seeking_{Female|Happy}$=91%; Experiment 2:

$Seeking_{Male|Sad}$=45%; $Seeking_{Male|Happy}$=55%; $Seeking_{Female|Sad}$=27%; $Seeking_{Female|Happy}$=73%). It would be worthwhile to examine in the future, though, the nuances in how men approach states of need in marketing contexts and their reactions to marketing stimuli that might threaten more "traditional" concepts of masculinity.

Fourth, in experiment 2 and across my data, I did not detect evidence consistent with multiple possible explanations for the effect I studied. Specifically, as detailed in the Web Appendices, I observed only sporadic support for the role of inferred quality of help or likelihood of approval in explaining the effect of sad portrayals on help-seeking. However, I note that there may be situations in which those mechanisms exert powerful effects on help-seeking. For example, a prospective help-seeker may believe that sadness in ads indicates the people they usually help are in extreme need, but if help appears to be abundant, it is likely that they would be approved no matter their need level. If help is extremely limited, it might be prioritized for people who have the most need, and they would not be approved. If help is limited, help-seekers may also infer if they get approved for help, it might be denied to someone who needs it more. The prospective guilt may then heighten the attention paid to downward social comparisons (White et al. 2006), making such mechanisms more powerful. Future work may seek to identify cases when such mechanisms play a stronger explanatory role, such that marketers can address their effects using additional messages and tactics.

Finally, the fact that this tactic makes people feel unvalued, unimportant, and unappreciated should give non-profits pause. I do not recommend non-profits, or researchers, attempt to find moderators that would lead participants to seek help when non-profits use these tactics, despite any disrespect they feel (e.g., by making participants feel they have no other choice). Instead, I advocate research into moderators that make these tactics appear respectful or

that bolster the respect that help-seekers experience, hoping to turn what may otherwise be a damaging cycle into one of self-acceptance, respect, and empowerment.

Appendix A: Pilot Experiments

These five pilot experiments establish that people in need are likely to encounter donation appeals (Pilots 1-3), and confirm that sadness is associated with disrespect (Pilots 4-5). In so doing, they strengthen the arguments underpinning these experiments, and demonstrate the relevance of this issue to non-profits.

Pilots 1, 2, and 3 show, respectively, that people more often see donation appeals than help-seeking appeals, that they are most likely to go to non-profit websites when seeking help, and that non-profits include appeals for donations on parts of their websites where people in need often visit. Pilot 2 also legitimizes the within-subjects designs used in this chapter, showing that people would compare multiple non-profits when they have a problem for which seeking charitable help is an option.

Pilots 4 and 5 more explicitly establish a connection between sadness and disrespect. Pilot 4 indicates people feel sadness appeals are disrespectful when included in non-profit ads, and Pilot 5 indicates, in a role playing exercise, that the more someone believes the respect of help-seekers is important, the less likely they are to use sadness appeals in donation advertisements.

Pilot 1

For my practical focus of the effect of donation appeals on people in need to be meaningful, it was important that I establish that people who may need help would be exposed to advertisements that are designed to solicit donations. I created a survey to substantiate the prevalence of this experience.

Methods

Participants and design.

Participants were Amazon Mechanical Turk panel participants (n=103) who completed the survey in exchange for a nominal payment.

Procedure.

Participants read definitions for two types of communications from non-profits: "Donation/Volunteer Requests", defined as "messages or ads asking people to donate or volunteer to a non-profit or charitable cause", and "Offers of Help", defined as "messages or ads used to get people in need to seek help from a non-profit or charitable cause". Participants answered questions about their encounters with these communications.

Measures.

Participants were asked, separately, whether they had seen any Donation/Volunteer Requests or Offers of Help in the previous 30 days. Participants were also asked whether they had seen more Donation/Volunteer Requests than they had Offers of Help in the previous 30 days.

Results

Sample size and respondent screening.

Our target final sample was 100 respondents. There were two attention checks. The first was the impossible/conflicting statement attention check introduced in experiment 4. For the second attention check, participants were asked to repeat the definition I gave for "offers of help". Any participants who did not give a definition approximating the one I gave were screened. I received 141 responses in total. 26 were screened for not providing the correct

definition for offers of help, three were screened for failing the true/false attention check, and nine were screened for failing both. In the end I collected 103 responses.

Incidence of seeing help giving requests.

70% of participants reported seeing a Donation/Volunteer Request from a non-profit in the previous 30 days. Without screening criteria, this proportion was 73%.

Comparative encounters of help giving and help seeking communications.

Of participants who mentioned seeing at least one of these types of communications in the past 30 days, 80% of respondents reported more often seeing Donation/Volunteer Requests than Offers of Help. By definition, this precluded any participants who mentioned seeing neither sort of communication in the last 30 days (n=27). Without screening criteria this proportion was 83%.

Discussion

This survey provides evidence that people often see help giving requests from non-profits more often than they do direct appeals to encourage help-seeking. As such, tactics used in ads designed to elicit donations are likely to be seen by those who may be in a position to seek help from the same non-profit.

Pilot 2

This survey was conducted to test the external validity of the within-subjects designs used in our research. This survey was also given, in conjunction with the survey in Pilot 3, to seek evidence that help-seekers would be likely to come across donation requests or online appeals designed for donors, particularly those utilizing sad faces.

Methods

Participants and design.

Participants were Prolific panelists (n=101). Participants completed the survey in exchange for $0.75.

Procedure.

Participants were asked to think about how they would act when they need help for a problem for which they might have to go to a non-profit. They then answered several questions.

Measures.

Three measures were designed to test the external validity of our within-subjects exposures to multiple potential sources of help in a given episode of need. First, participants were asked to rate how they would go about finding a source for help, on a 7-point bipolar scale anchored at "Go with the first non-profit I know" and "Search for the options and compare them." Second, participants were asked what they would recommend a friend in need do for seeking help, using a 7 point bipolar continuum anchored at "Go with the first non-profit they know about" and "Search for information about more than one non-profit". Finally, participants were asked to select how many non-profit websites they would search before reaching a decision about seeking help. Participants were given the options 1 through 9 and "10 or more".

To analyze the likelihood people in need would rely on non-profit websites as a source of information for seeking help, participants were asked how likely they were to rely on each of a series of sources of information when deciding to seek help from a non-profit, from 1 ("Not at all") to 7 ("Extremely"). Options included "the non-profit's website". After this, participants were asked to imagine they decided to browse the internet to find help, and were asked, if they were looking at a non-profit's website, to select which parts of the site they would check out.

Options were “pages specifically for people in need”, “the non-profit’s home page”, “the non-profit’s ‘about us’ page”, and “pages specifically for non-profit donors”.

Results

Sample size and respondent screening.

Our target final sample was 100 respondents. I collected 101 responses, and had no screening criteria.

Number of sources for seeking help.

For the two questions in which participants rated, along a bipolar continuum, whether they would seek help from/recommend a friend seek help from the first non-profit they could find, or compare non-profits, one-sample t-tests were run against a null hypothesis of 4, which is the midpoint of the scale and would indicate indifference between going with one non-profit or comparing non-profits. Findings indicate people would be more likely to compare multiple non-profits than go with the first non-profit they knew (M=5.11, t=6.04, p<.0001). They would also be more likely to recommend to others that they compare non-profits rather than go with the first non-profit they knew (M=5.70, t=11.56, p<.0001). Participants, on average, said they would visit 4.25 non-profit websites before making a help seeking decision.

Reliance on different sources of information.

The non-profit’s website was the most likely source of information people would rely on when determining where to seek help (M=5.51), more than all other sources of information ($t \geq$ 2.42, $p \leq$. 0174). The means for the other sources of information were: websites that rate various non-profits in terms of how they treat people (M=5.19), word of mouth (M=5.03), other websites (M=4.34), articles about the non-profit that I might see in newspapers or magazines (M=3.94),

advertisements I see in public (M=3.25), advertisements I see in magazines or newspapers (M=3.01), and fliers (M=2.84).

Non-profit website browsing.

87% of participants said they would visit the sections of the non-profit's website for help-seekers. 71% of participants would visit the home page. 68% of participants would visit the non-profit's "about us" page. 25% of participants would visit the parts of the non-profit's website specifically for donors.

Discussion

This survey substantiates the use of a within-subject design in this context. Several metrics suggest people would compare multiple non-profits when they have a problem for which seeking charitable help is an option. This survey also supports my use of charitable websites in our scenarios: participants were most likely to rely on non-profits' websites when seeking information about where to seek help. Participants would also likely visit areas on the non-profit's website that would conceivably contain donation ads: the home page and the sections for donors. Thus, people in need of help are likely to see donation appeals from multiple non-profits, paralleling my experimental designs and stimuli.

Pilot 3

This survey provides additional evidence that help-seekers would be likely to come across donation requests, particularly those utilizing sad faces. I commissioned an independent review of the ads used in the homepage, help seeker page(s), and donor page(s) of non-profits' websites.

Methods

Procedure.

An independent third party was asked to visit the websites of the non-profits on Forbes' list of 100 largest non-profits, excluding those that do not help people. They were asked to mark, for the home page, the donations page, and the help seeking page, how many photos there were of people who may be seeking help from the non-profit, and what emotions were displayed in each photo (happiness, sadness, other). The third party was also asked to mark whether there was a donations appeal on the non-profit's home page.

Results

94% of non-profits' websites have a donation appeal on their home page, and 54% of non-profit websites feature a sad help-seeker on either their home page or donations page. While help seeking pages do not feature donation requests, and therefore are not relevant to the objectives of this survey, 50% of the websites that showed at least one photo of an ostensible help seeker on their help seeking pages included at least one photo of a sad person.

Discussion

These results highlight the general prevalence of sad faces on the web pages of major non-profits. Results suggest that a majority of non-profits feature sad help-seekers on pages commonly visited by help-seekers, making it possible that people in need would, in fact, encounter these types of donation appeals.

Pilot 4

This survey provides additional evidence that portrayals of sadness are perceived as disrespectful.

Methods

Participants and design.

Participants were Prolific panelists (n=101). Participants completed the survey in exchange for $0.50.

Procedure.

Participants were told that companies, non-profits, photographers, people who post to social media, etc. often take photos of people where they deliberately are trying to get those people to show a specific emotion.

Measures.

Participants were given a list of emotions—including sadness and neutral expression—and asked to rate how much they think an organization respects the subject of the photo if the organization makes the subject display that emotion, from 1 ("Not at all") to 7 ("Extremely").

Results

Sample size and respondent screening.

Our target final sample was 100 respondents. I collected 101 responses, and had no screening criteria.

Emotions and disrespect.

A paired t-test was run comparing the ratings between sadness (M=3.44) and neutral expressions (M=4.52). Findings indicate people find displays of sadness to be more disrespectful than neutral displays (t=-5.19, p<.0001). As an additional check, a one-sample t-test was run

against a null hypothesis of 4, which is the midpoint of the scale. Findings show ratings for sadness significantly fell below the midpoint (t=-3.49, p=.0007).

Discussion

This survey substantiates the conjecture that non-profits making prospective help seekers appear sad would be seen as disrespectful. When asked directly how respectful an organization would be if they tried to make someone in a photo appear sad, people said it was less respectful than a neutral expression.

Pilot 5

To provide greater evidence that portrayals of sadness are disrespectful, I ran a behavioral experiment wherein participants imagined themselves working for a non-profit, and experimentally manipulated the importance of respect. If portrayals of sadness are disrespectful, I would find that the more important respect is, the less likely participants would be to let help-seekers appear sad in a non-profit advertisement.

Method

Participants and design.

Pilot 5 followed a two (Emotional Depiction: Sad vs. Happy) within-subjects x two (Priority: Respect vs. Money vs. Control) between-subjects mixed design. Participants from Prolific (N=300), completed this survey in exchange for $1.50.

Procedure.

Participants imagined that they were the head of marketing for a non-profit and were putting together the next donations ad for the website. It would be in a prominent place, large

and at the top of the home page. A junior member of the team came to them with two photos they took of themselves (Figure 1.13). The junior member suggests the participant use one of the two photos exactly as is to represent one of the people who seek help from their non-profit. One of the photos showed the junior member with a happy expression, and in the other they had a sad expression. Gave their relative preference between the two photos and answered mediator questions, outlined below.

To manipulate the importance of respect, participants read information about a news article before the scenario as part of an ostensibly unrelated survey. One in three participants (Priority: Respect condition) read that an article was posted on ABC pointing out that many non-profit donation ads are placed in prominent places where people in need can easily find them. According to the article, in these cases many non-profits have to balance portraying people in the ads in a way that helps them raise money vs. a way that helps people in need feel respected. All of the non-profits interviewed for the article said in this case they should be prioritizing making sure the people who see the ad feel respected instead of raising money. Another one in three participants (Priority: Money condition) read the same blurb except all of the non-profits said they should be prioritizing raising money instead of making sure people feel respected. In both conditions, participants were asked to write 2 paragraphs on why the non-profits' opinion is correct. The remaining third of participants (Priority: Control condition) were asked to write two paragraphs on what they did the previous day.

Measures.

Participants rated their relative preference between the first and second photos from 0-100, where the first photo was anchored at 0, the second photo as 100, and "no preference" at 50. If the sad photo was shown first, responses were reverse coded, otherwise ratings were not

altered. In this way a measure was developed where a rating of 100 was complete preference for the sad photo, 0 complete preference for the happy photo, and 50 was no preference. This constituted the dependent variable.

Following the dependent variable, participants rated to what extent they felt it was important to choose a photo that would make people in need feel respected, from 0 ("Not at all important") to 100 ("Extremely important"). They also rated importance of choosing a photo that would raise money.


Figure 13: Pilot 5 stimuli

Results

Sample size and respondent screening.

In this study, I targeted a sample of 300 participants after screening (100 per between-subjects cell).

306 responses were collected, of which 6 were screened prior to inclusion in the dataset for indicating they remembered taking a similar survey. In this way, I ensured that participants might not be biased by having taken a previous survey in this stream of research. This left a final sample of 300.

Choice of Emotional Expression.

A one-way ANOVA on the Priority conditions with preference between the sad and happy photos as the DV yielded a significant effect of the Priority manipulation (F=16.36, p<.0001). In the Priority: Money condition, relative to the other conditions, participants showed a greater preference for the sad face than the happy face ($Preference_{PriorityMoney}$=36.68 vs. $Preference_{PriorityRespect}$=19.67, F=17.69, p<.0001; vs. $Preference_{PriorityControl}$=15.71, F=29.92, p<.0001). There was no difference between the High Importance and Control conditions (F=1.01, p=.3145).

Importance of respect.

A one-way ANOVA with the importance of respect in choosing a photo as the DV and Priority manipulation as the IV yielded a significant effect (F=34.47, p<.0001). As expected, in the Priority: Money condition, participants found respect to be less important when choosing a photo for the ad than in either other condition ($RespectImportance_{PriorityMoney}$=54.50 vs. $RespectImportance_{PriorityRespect}$=83.01, F=58.59, p<.0001; vs. $RespectImportance_{PriorityControl}$=77.95, F=44.14, p<.0001). There was no difference between the High Importance and Control conditions (F=1.97, p=.1619).

Importance of raising money.

A one-way ANOVA with the importance of money in choosing a photo as the DV and the Priority manipulation as the IV yielded a significant effect (F=10.42, p<.0001). As expected, in the Priority: Money condition, participants found raising money to be more important when choosing a photo for the ad than in the Respect Priority condition ($MoneyImportance_{PriorityMoney}$=78.47 vs. $MoneyImportance_{PriorityRespect}$=67.44, F=9.35, p=.0024). It was not any different from control, though ($MoneyImportance_{Control}$=83.02, F=1.91, p=.1681).

Participants in the Priority: Respect condition felt raising money was less of a priority than Priority: Control participants did (F=20.28, p<.0001).

Mediation.

A parallel bootstrapping mediation model (PROCESS Macro Model 4) was estimated for the Priority: Respect and Priority: Money conditions. Priority was the independent variable, importance of respect in choosing a photo and importance of raising money were the two mediators, and emotional expression preference was the dependent variable. The preference between the two photos was mediated by perceived importance of respect (Indirect Effect=-5.04; SE=1.56; 95% CI=[-8.23, -2.17]; Direct Effect=-3.47; SE=2.38; p=.1459). The importance of raising money did not affect preference (Indirect Effect=-.03; SE=.41; 95% CI=[-.93, .79]).

Discussion

Results again suggest that displays of sadness are seen as disrespectful. When roleplaying a non-profit marketer, people preferred using a happy photo for an ad more when told they should prioritize making sure people in need seeing the ad feel respected, rather than being told they should priority making money. The priority manipulation did also change how important participants felt raising money was when making the ad, but only respect determined their preferences.

Appendix B: Replications and Follow-Up Experiment

EXPERIMENT 2 REPLICATIONS

Appendix B contains five within-subjects tests of the main effect, hypothesized process, and alternative explanations explored in this chapter (hereafter referred to as Replications). These tests were referenced in this chapter as evidence of the replicability of the hypothesized mediator—perceived respect—and the non-replicability of the alternative mechanisms studied in experiment 2. Results from all five replications are included below in Tables 1.2-1.5.

Analyses in Replication 5 were identical to that in experiment 2 of this chapter. For Replications 1-4, in order to test mediation, I asked participants to rate perceived respect and each of the alternative explanations only for the first of two ads they saw. For those experiments, PROCESS Macro 4 was run for parallel mediation with the emotion shown in the first ad as the IV, whether participants chose that first ad as the DV, and ratings of the first ad as mediators. For consistency, in Table 1.2, I report tests of H_1 in Replications 1-4 as the likelihood that participants chose the first ad of two to seek help, contingent on the emotion shown in the ad. This is how the analysis was originally made, and is consistent with the way mediation was calculated. I do, however, show the results for help-seeking charity choice across all replications in this appendix, in aggregate, using the method in experiment 2.

Replication	Result	Result (Including Excluded Participants)
Aggregate	$Choice_{Sad}$=18% vs. $Choice_{Happy}$=82% χ^2=252.88, p<.0001	$Choice_{Sad}$=20% vs. $Choice_{Happy}$=80% χ^2=298.33, p<.0001
1	$Choice_{Sad}$=19% vs. $Choice_{Happy}$=89% χ^2=60.23, p<.0001	$Choice_{Sad}$=24% vs. $Choice_{Happy}$=88% χ^2=54.40, p<.0001

2	$Choice_{Sad}$=23% vs. $Choice_{Happy}$=89% χ^2=51.23, p<.0001	$Choice_{Sad}$=21% vs. $Choice_{Happy}$=85% χ^2=51.34, p<.0001
3	$Choice_{Sad}$=14% vs. $Choice_{Happy}$=78% χ^2=63.48, p<.0001	$Choice_{Sad}$=13% vs. $Choice_{Happy}$=78% χ^2=65.84, p<.0001
4	$Choice_{Sad}$=23% vs. $Choice_{Happy}$=83% χ^2=54.45, p<.0001	$Choice_{Sad}$=21% vs. $Choice_{Happy}$=78% χ^2=63.22, p<.0001
5	$Choice_{Sad}$=22% vs. $Choice_{Happy}$=78% χ^2=24.01, p<.0001	$Choice_{Sad}$=21% vs. $Choice_{Happy}$=79% χ^2=57.32, p<.0001

Table 1.3: Effects of emotional depiction on help seeking across experiment 2 replications

Replication	Result	Result (Including Excluded Participants)
1	$Respect_{Sad}$=3.78 vs. $Respect_{Happy}$=5.23 t=5.61, p<.0001	$Respect_{Sad}$=3.88 vs. $Respect_{Happy}$=5.24 t=5.72, p<.0001
2	$Respect_{Sad}$=3.30 vs. $Respect_{Happy}$=4.47 t=4.37, p<.0001	$Respect_{Sad}$=3.31 vs. $Respect_{Happy}$=4.51 t=4.79, p<.0001
3	$Respect_{Sad}$=3.08 vs. $Respect_{Happy}$=4.53 t=6.79, p<.0001	$Respect_{Sad}$=3.04 vs. $Respect_{Happy}$=4.53 t=6.97, p<.0001
4	$Respect_{Sad}$=3.24 vs. $Respect_{Happy}$=4.91 t=7.67, p<.0001	$Respect_{Sad}$=3.21 vs. $Respect_{Happy}$=4.86 t=8.51, p<.0001
5	$Respect_{Sad}$=3.69 vs. $Respect_{Happy}$=4.39 t=-4.48, p<.0001	$Respect_{Sad}$=3.61 vs. $Respect_{Happy}$=4.51 t=-8.44, p<.0001

Table 1.4: Effects of emotional depiction on perceived respect across experiment 2 replications

Replication	Result	Result (Including Excluded Participants)
1	Indirect Effect = -1.11; SE = .43 95% CI = [-2.19, -.59]	Indirect Effect = -0.92; SE = .29 95% CI = [-1.63, -.49]
2	Indirect Effect = -.63; SE = .29 95% CI = [-1.42, -.27]	Indirect Effect = -.50; SE = .22 95% CI = [-1.08, -.22]
3	Indirect Effect = -.87; SE = .30 95% CI = [-1.66, -.51]	Indirect Effect = -.91; SE = .30 95% CI = [-1.67, -.55]
4	Indirect Effect =-.50; SE=.24 95% CI=[-1.07, -.16]	Indirect Effect =-.51; SE=.21 95% CI=[-1.00, -.20]
5	Estimate =-4.30 χ^2=8.32; p=.0039	Estimate =-0.56 χ^2=3.72; p=.0539

Table 1.5: Mediation results across experiment 2 replications, Respect explanation

Explanation	Replication	Result	Result (Including Excluded Participants)
Quality of Help	1	Indirect Effect = -.03; SE = .18 95% CI = [-.27, .44]	Indirect Effect = .01; SE = .13 95% CI = [-.23, .29]
	2	Indirect Effect = -.06; SE = .16 95% CI = [-.43, .23]	Indirect Effect = -.06; SE = .13 95% CI = [-.36, .17]
	3	Indirect Effect = -.26; SE = .21 95% CI = [-.72, .09]	Indirect Effect = -.20; SE = .18 95% CI = [-.62, .10]
	4	Indirect Effect = -.44; SE=.25 95% CI = [-1.07, -.10]	Indirect Effect = -.37; SE=.20 95% CI=[-.85, -.07]
	5	Estimate = -1.42 χ^2=3.04; *p*=.0812	Estimate = -1.02 χ^2=9.81; *p*=.0017
Downward Social Comparison	4	Indirect Effect =-.07; SE=.11 95% CI = [-.35, .13]	Indirect Effect = -.01; SE=.10 95% CI=[-.22, .19]
	5	Estimate = 0.80 χ^2=1.31; *p*=.2527	Estimate = -0.45 χ^2=3.03; *p*=.0819
Likelihood of Approval	4	Indirect Effect =-.06; SE=.08 95% CI = [-.26, .05]	Indirect Effect = -.08; SE=.06 95% CI=[-.24, .01]
	5	Estimate = -2.13 χ^2=2.59; *p*=.1076	Estimate = 0.13 χ^2=0.12; *p*=.7324
Emotional Similarity	5	Estimate = -0.04 χ^2=0.01; *p*=.9054	Estimate = -0.12 χ^2=0.13; *p*=.3556
Shame	5	Estimate = 0.78 χ^2=2.10; *p*=.1475	Estimate = 0.06 χ^2=0.06; *p*=.8118

Table 1.6: Mediation results across experiment 2 replications, alternative explanations

<u>Replication 1</u>

This replication tests both my main effect and proposed process, using a within-subjects design. In this study, I collect a measure of perceived Quality of Help as an alternative contributing mediator.

Method

Participants and design.

The replication followed a two (Emotional Depiction: Happy vs. Sad) group within-subjects design. Participants were Prolific panelists (n=121) who completed the survey in exchange for $0.50.

Procedure.

Participants imagined that they saw fundraising ads for two different food banks on a grocery store bulletin as they were leaving the store. They were asked to pick the one from which they would want to seek help. The stimuli were presented in pairs, and the order of emotion and organization was randomized. Standardized photos from Ekman (1993) and Tottenham et al. (2009) were used. These stimuli were used in experiment 2 replications 1-4. An example stimulus set is included in Figure 1.14. Afterward, participants answered measures for perceived respect and quality of help, for the first of the two ads they were shown. Finally, participants underwent two screening checks, contained in experiment 4 of the manuscript and described below.

Figure 1.14: Example stimulus set in Replication 1

Measures.

The Rank-Christman et al. (2017) measure of respect included two additional, negatively worded descriptors, which I removed for the version used in the manuscript ("worthless" and

"unimportant") (Schmitt and Stuits 1985). Participants also rated an earlier version of the Quality of Help measure included in experiment 2. Participants rated their agreement with three statements from 1 to 7: "People who seek help from this charity get all the help they need", "People who seek help from this charity have good outcomes", and "People who seek help from this charity are successful." This is virtually identical to the measure in experiment 2, but with two differences. First, the third statement used in experiment 2 was phrased "…are successful in seeking help" rather than "…are successful". Second, each statement was displayed on a separate screen. The latter decision was made for two reasons: first to reduce straight-lined responses, and second, because at the time there were concerns some of these statements might constitute separate constructs, and showing the items this way was a more conservative way to test correlation and make sure there was no cross-contamination of meanings.

Results

Sample size and respondent screening.

This replication incorporated both of the screening criteria utilized in experiment 4. I collected 129 responses, of which six respondents were screened for failing the true/false attention check and two respondents failed the timing check, leaving 121 respondents.

Help-seeking.

A chi-square test examined whether participants' likelihood of choosing to seek help from the charity whose ad was shown first was contingent on the emotion shown in the ad. This parallels the mediator measures, which were asked only of the first ad participants saw. Consistent with H_1, help-seekers were less likely to choose the charity ad they rated when the woman in the ad was sad ($Choice_{Sad}$=19% vs. $Choice_{Happy}$=89%, χ^2=60.23, p<.0001). Findings were robust to the screening criteria ($Choice_{Sad}$=24% vs. $Choice_{Happy}$=88%, χ^2=54.40, p<.0001).

Inferred respect.

All items on the respect measure loaded onto one factor (λ_1=4.40, λ_2=.88), with a high Cronbach's alpha (α=.93). Help-seekers felt less respect from the charity whose ad was rated when the woman in that ad was sad ($Respect_{Sad}$=3.78 vs. $Respect_{Happy}$=5.23, t=5.61, p<.0001). Findings were robust to the screening criteria ($Respect_{Sad}$=3.88 vs. $Respect_{Happy}$=5.24, t=5.72, p<.0001).

Quality of help.

All items on the quality of help measure loaded onto one factor (λ_1=2.00, λ_2=.63), with a high Cronbach's alpha (α=.75), so their scores were averaged to create an index. Help-seekers felt the charity that they rated offered lower quality help when it featured someone sad (QoH_{Sad}=3.90 vs. QoH_{Happy}=4.80, t=4.70, p<.0001). Findings were robust to the screening criteria (QoH_{Sad}=3.95 vs. QoH_{Happy}=4.80, t=4.41, p<.0001).

Mediation.

A parallel mediation model was estimated (PROCESS Macro, Model 4) with whether the first charity was chosen as the dependent variable, the emotion shown in the rated charity ad as the independent variable, and Respect and Quality of Help as parallel mediation pathways. Results indicate mediation through Respect, but not through Quality of Help (Indirect $Effect_{Respect}$ = -1.11; SE = .43; 95% CI = [-2.19, -.59]; Indirect $Effect_{Quality\ of\ Help}$ = -.03; SE = .18; 95% CI = [-.27, .44]). Findings were robust to the screening criteria (Indirect $Effect_{Respect}$ = -.92; SE = .29; 95% CI = [-1.62, -.51]; Indirect $Effect_{Quality\ of\ Help}$ = .01; SE = .13; 95% CI = [-.25, .31]).

Replication 2

This study tests both my main effect and proposed process, within-subjects. In this study, I use a measure adapted from Huo, Binning, and Molina (2010) as my proposed mediator of Respect, and collect a measure of perceived Quality of Help as an additional potential process variable.

Method

Participants and design.

The replication followed a two (Emotional Depiction: Happy vs. Sad) group within-subjects design. Participants were Prolific panelists (n=119). Participants completed the survey in exchange for $0.50.

Procedure.

The procedure for this replication was identical to that of Replication 1.

Measures.

Participants completed a modified version of a widely-used respect measure in psychology journals. The respect measure developed in Huo et al. (2010) has been used in dozens of citing works. It was notably expanded in Mahadevan et al. (2016). While Huo et al. (2010) argue that respect follows a dual pathway through status and liking, the items in the liking subscale (e.g., "I think this person [considers me to be a nice person to have around/includes me in their social activities/would be happy to have me as a friend]") are not applicable to a relationship with a disembodied entity like a charity. Therefore, I only collected items from the status subscale, retaining the items that would be applicable to this type of relationship and modifying the language as needed. Ultimately, participants rated their agreement with the following statements from 1 to 7: "I think this charity…" "respects me", "values my opinions

and ideas", "thinks highly of me", "admires me", "considers me a success", "sees me as an important person".

Participants also rated Quality of Help using the measure described in Replication 1.

Results

Sample size and respondent screening.

My target final sample was approximately 120, with two screening criteria, used in Replication 1, both of which were used to filter respondents prior to data analysis. I collected 129 responses, of which seven respondents were screened for failing the true/false attention check, two respondents failed the timing check, and one respondent failed both checks, leaving 119 respondents.

Help-seeking.

A chi-square test examined whether participants' likelihood of choosing to seek help from the charity whose ad they saw first was contingent on the emotion shown in the ad. This parallels the Respect and Quality of Help measures, which were asked only of the first ad participants saw. Consistent with H_1, help-seekers were less likely to choose the the rated charity ad when the woman in the ad was sad ($Choice_{Sad}$=23% vs. $Choice_{Happy}$=89%, χ^2=51.23, p<.0001). Findings were robust to the screening criteria ($Choice_{Sad}$=21% vs. $Choice_{Happy}$=85%, χ^2=51.34, p<.0001).

Inferred respect.

All items on the respect measure adapted from Huo, Binning, and Molina (2010) and Mahadevan et al. (2016) loaded onto one factor (λ_1=4.98, λ_2=.39; α=.96). Help-seekers felt less respect from the charity whose ad was rated when the woman in that ad was sad

($Respect_{Sad}$=3.30 vs. $Respect_{Happy}$=4.47, t=4.37, p<.0001). Findings were robust to the screening criteria ($Respect_{Sad}$=3.31 vs. $Respect_{Happy}$=4.51, t=4.79, p<.0001).

Quality of help.

All items on the quality of help measure loaded onto one factor (λ_1=2.42, λ_2=.37), with a high Cronbach's alpha (α=.88). Help-seekers felt the rated charity ad offered lower quality help when they featured someone sad (QoH_{Sad}=3.98 vs. QoH_{Happy}=4.80, t=3.60, p=.0005). Findings were robust to the screening criteria (QoH_{Sad}=3.99 vs. QoH_{Happy}=4.85, t=4.00, p=.0001).

Parallel mediation.

The three statements that constituted the Quality of Help measure loaded onto 1 factor (λ_1=2.42, λ_2=.37) with a high Cronbach's alpha (α=.88), so we averaged their scores to create an index. A parallel mediation model was estimated (PROCESS Macro, Model 4) with whether participants chose to seek help from the charity whose ad they rated as the dependent variable, the emotion shown in that charity's ad as the independent variable, and Respect and Quality of Help as separate mediational pathways. Results indicate mediation through Respect, but not Quality of Help (Indirect $Effect_{Respect}$ = -.63; SE = .29; 95% CI = [-1.42, -.27]; Indirect $Effect_{Quality\ of\ Help}$ = -.06; SE = .16; 95% CI = [-.43, .23]). Findings were robust to the screening criteria (Indirect $Effect_{Respect}$ = -.50; SE = .22; 95% CI = [-1.06, -.21]; Indirect $Effect_{Quality\ of\ Help}$ = -.06; SE = .13; 95% CI = [-.34, .17]).

Replication 3

This replication tests both my main effect and proposed process, within-subjects. In this study, I collect a measure of Respect. I also collect a measure of perceived Quality of Help,

which is slightly different from that used in Replications 1 and 2, as an additional potential process variable.

Method

Participants and design.

The replication followed a two (Emotional Depiction: Happy vs. Sad) group within-subjects design. Participants were Prolific panelists who were pre-filtered to have annual income of $20,000 or less (n=150). Participants completed the survey in exchange for $0.33.

Procedure.

The procedure for this replication was the same as for Replication 1.

Measures.

The Respect and Quality of Help measures were identical to those used in experiment 2.

Results

Sample size and respondent screening.

My target final sample was approximately 150, same as in experiments 1A, 1B, 2, and 3. The screening criteria were the same as in Replication 1, with the exception that participants were also asked to rate their food insecurity, as they did in experiment 2. Participants who rated their food insecurity 1 or 2 out of 7 were screened.

Ultimately, I collected 194 responses, of which 41 were screened for not agreeing that they were currently experiencing food insecurity. Of the remaining 153 respondents, three respondents were screened for failing the true/false attention check. No one failed the timing check.

Help-seeking.

A chi-square test examined whether participants' likelihood of choosing to seek help from the charity whose ad they saw first was contingent on the emotion shown in the ad. This parallels the Respect and Quality of Help measures, which were asked only of the first ad participants saw. Help-seekers were less likely to choose the charity ad they rated when the woman in the ad was sad ($Choice_{Sad}$=14% vs. $Choice_{Happy}$=78%, χ^2=63.48, p<.0001). Findings were robust to the screening criteria ($Choice_{Sad}$=13% vs. $Choice_{Happy}$=78%, χ^2=65.84, p<.0001).

Inferred respect.

All items on the respect measure loaded onto 1 factor (λ_1=3.60, λ_2=.17), with a high Cronbach's alpha (α=.96). Help-seekers felt less respect from the rated charity when the woman in that ad was sad ($Respect_{Sad}$=3.08 vs. $Respect_{Happy}$=4.53, t=6.79, p<.0001). Findings were robust to the screening criteria ($Respect_{Sad}$=3.04 vs. $Respect_{Happy}$=4.53, t=6.97, p<.0001).

Quality of help.

All items on the quality of help measure loaded onto one factor (λ_1=2.56, λ_2=.26), with a high Cronbach's alpha (α=.91). Help-seekers felt the rated charity offered lower quality help when they featured someone sad (QoH_{Sad}=3.79 vs. QoH_{Happy}=4.96, t=6.15, p<.0001). Findings were robust to the screening criteria (QoH_{Sad}=3.86 vs. QoH_{Happy}=4.96, t=5.70, p<.0001).

Parallel mediation.

A parallel mediation model was estimated (PROCESS Macro, Model 4) with whether participants chose to seek help from the charity whose ad they rated as the dependent variable, the emotion shown in that charity ad as the independent variable, and Respect and Quality of Help as separate mediational pathways. Results show respect was the only mechanism that mediated choice of charity (Indirect $Effect_{Respect}$ = -.87; SE = .30; 95% CI = [-1.66, -.51]; Indirect $Effect_{Quality\ of\ Help}$ = -.26; SE = .21; 95% CI = [-.72, .09]). Findings were robust to the screening

criteria (Indirect Effect$_{\text{Respect}}$ = -.91; SE = .30; 95% CI = [-1.67, -.55]; Indirect Effect$_{\text{Quality of Help}}$= -.20; SE = .18; 95% CI = [-.62, .10]).

Replication 4

This replication tests both my main effect and proposed process, within-subjects. In this replication, I collect a measure of Respect and measures of three alternative explanations, which were tested in experiment 2 in the manuscript: Quality of Help, Downward Social Comparisons, and Likelihood of Approval.

Method

Participants and design.

The replication followed a two (Emotional Depiction: Happy vs. Sad) group within-subjects design. Participants were Prolific panelists who were pre-filtered to have annual income of $20,000 or less (n=150). Participants completed the survey in exchange for $1.00.

Procedure.

The procedure was identical to that in Replication 1

Measures.

The Respect, Quality of Help, Downward Social Comparison, and Likelihood of Approval measures were identical to those used in experiment 2.

Results

Sample size and respondent screening.

The screening criteria were the same as in Replication 3, with two taken from Replication 1 and an additional screening criterion where participants were asked to rate their food insecurity. Participants who rated their food insecurity 1 or 2 out of 7 were screened.

My target final sample was 150 respondents. In the end, I collected data from 195 responses, of which 37 were screened for not agreeing that they were currently experiencing food insecurity. Of the remaining 158 respondents, eight were screened for failing the true/false attention check. No one failed the timing check.

Help-seeking.

A chi-square test examined whether participants' likelihood of choosing to seek help from the charity whose ad they saw first was contingent on the emotion shown in the ad. This parallels the Respect and Quality of Help measures, which were asked only of the first ad participants saw. Consistent with prior findings and H_1, help-seekers were less likely to choose the charity whose ad they rated when the person in the ad was sad (vs. happy) ($Choice_{Sad}$=23% vs. $Choice_{Happy}$=83%, χ^2=54.45, p<.0001). Findings are robust to the screening criteria ($Choice_{Sad}$=21% vs. $Choice_{Happy}$=78%, χ^2=63.22, p<.0001).

Inferred respect.

All items on the respect measure loaded onto 1 factor (λ_1=3.60, λ_2=.18; α=.96). Help-seekers inferred less respect from the rated charity when its emotional depiction was sadness ($Respect_{Sad}$=3.24 vs. $Respect_{Happy}$=4.91, t=7.67, p<.0001). Findings are robust to the screening criteria ($Respect_{Sad}$=3.26 vs. $Respect_{Happy}$=4.87, t=7.26, p<.0001).

Quality of help.

All items on the quality of help measure loaded onto one factor (λ_1=2.64, λ_2=.21; α=.93). Help-seekers felt the rated charity offered lower quality help when they featured someone sad

(QoH_{Sad}=3.88 vs. QoH_{Happy}=5.11, t=6.26, p<.0001). Findings were robust to the screening criteria (QoH_{Sad}=3.88 vs. QoH_{Happy}=5.05, t=5.95, p<.0001).

Downward social comparison.

All items on the downward social comparison measure loaded onto one factor (λ_1=2.32, λ_2=.43; α=.85). Help-seekers made more downward social comparisons to the person featured in the ad they rated when that person had a sad facial expression (DSC_{Sad}=4.59 vs. DSC_{Happy}=3.85, t=-3.49, p=.0006). Findings were robust to the screening criteria (DSC_{Sad}=4.55 vs. DSC_{Happy}=3.82, t=-3.48, p=.0007).

Likelihood of approval.

All items on the Likelihood of Approval measure loaded onto one factor (λ_1=2.57, λ_2=.29; α=.92). Help-seekers felt the charity whose ad was rated was less likely to approve them for aid when that charity featured someone sad, albeit marginally ($Approval_{Sad}$=4.48 vs. $Approval_{Happy}$=4.97, t=1.93, p=.0551). Findings were robust to the screening criteria ($Approval_{Sad}$=4.49 vs. $Approval_{Happy}$=4.99, t=2.02, p=.0450).

Parallel mediation.

Respect and all three alternative mechanisms were placed into a bootstrapping parallel mediation model (PROCESS Macro, Model 4). Quality of Help (Indirect $Effect_{Quality\ of\ Help}$=-.44; SE=.25; 95% CI = [-1.07, -.10]) partially mediated the relationship between emotional depiction in the ad and help-seeking. However, after accounting for this effect, respect still presented a significant mediation path (Indirect $Effect_{Respect}$=-.50; SE=.24; 95% CI = [-1.07, -.16]) (H_2). None of the other alternative explanations resulted in significant mediation (Indirect $Effect_{Downward\ Social\ Comparison}$=-.07; SE=.11; 95% CI = [-.35, .13]; Indirect $Effect_{Likelihood\ of\ Approval}$=-.06; SE=.08; 95% CI = [-.26, .05]). Findings are robust to the screening criteria (Indirect

$Effect_{Respect}$=-.51; SE=.21; 95% CI=[-1.00, -.20]; Indirect $Effect_{Quality\ of\ Help}$= -.37; SE=.20; 95% CI=[-.85, -.07]; Indirect $Effect_{Downward\ Social\ Comparison}$= -.01; SE=.10; 95% CI=[-.22, .19]; Indirect $Effect_{Likelihood\ of\ Approval}$= -.08; SE=.06; 95% CI=[-.24, .01]).

Replication 5

This study tests both our main effect and proposed process, within-subjects. This study is an exact replication of experiment 2 in the manuscript, which contained a fielding error wherein close to half of the people who took this experiment also took a similar experiment I ran less than a month prior. To generate data without so many biased participants, I reran the experiment for the dissertation.

Method

Participants and design.

The replication followed a two (Emotional Depiction: Happy vs. Sad) group within-subjects design. Participants were Prolific panelists who were pre-filtered to have annual income of $20,000 or less (n=78). Participants completed the survey in exchange for $1.00.

Procedure.

The procedure and stimuli were identical to that used in experiment 2.

Measures.

The measures in this experiment were identical to those used in experiment 2 in the manuscript.

Results

Sample size and respondent screening.

My target final sample was 150 respondents, after applying the same screening procedures used in experiment 2. Of the 300 respondents I gathered for the first survey, I received 172 responses to the second survey. Of those, 24 were screened for not experiencing food insecurity, 70 were screened for remembering taking a similar survey, and 1 did not provide a user ID I could match to the first survey they completed two weeks prior. In the end I collected 77 responses.

Help-seeking.

Consistent with prior findings and H_1, help-seekers were less likely to choose the charity whose ad contained a person who was sad (vs. happy) to seek help ($Choice_{Sad}$=22% vs. $Choice_{Happy}$=78%, χ^2=24.01, p<.0001). Findings are robust to the screening criteria ($Choice_{Sad}$=21% vs. $Choice_{Happy}$=78%, χ^2=63.22, p<.0001).

Inferred respect.

All items on the respect measure loaded onto 1 factor (λ_1=3.49, λ_2=.24; α=.95). Help-seekers inferred less respect from charities when the emotional depiction in their ad was sadness ($Respect_{Sad}$=3.69 vs. $Respect_{Happy}$=4.39, t=-4.48, p<.0001). Findings are robust to the screening criteria ($Respect_{Sad}$=3.61 vs. $Respect_{Happy}$=4.51, t=-8.44, p<.0001).

Quality of help.

All items on the quality of help measure loaded onto one factor (λ_1=2.64, λ_2=.21; α=.93). Help-seekers felt the charity that featured someone sad in their ad offered lower quality help (QoH_{Sad}=4.10 vs. QoH_{Happy}=4.73, t=-3.81, p=.0003). Findings were robust to the screening criteria (QoH_{Sad}=4.09 vs. QoH_{Happy}=4.86, t=-7.45, p<.0001).

Downward social comparison.

All items on the downward social comparison measure loaded onto one factor (λ_1=2.32, λ_2=.43; α=.85). Help-seekers made more downward social comparisons to the person in the charities' ads when that person had a sad facial expression (DSC_{Sad}=4.21 vs. DSC_{Happy}=3.52, t=5.39, p<.0001). Findings were robust to the screening criteria (DSC_{Sad}=4.36 vs. DSC_{Happy}=3.77, t=6.53, p<.0001).

Likelihood of approval.

All items on the Likelihood of Approval measure loaded onto one factor (λ_1=2.57, λ_2=.29; α=.92). Help-seekers felt a charity was less likely to approve them for aid when that charity featured someone sad ($Approval_{Sad}$=4.59 vs. $Approval_{Happy}$=4.88, t=-2.82, p=.0062). Findings were robust to the screening criteria ($Approval_{Sad}$=4.42 vs. $Approval_{Happy}$=4.77, t=-5.18, p<.0001).

Parallel mediation.

Inferred respect was the only mechanism that mediated the effect ($Estimate_{Respect}$=-4.30; χ^2=8.32; p=.0039) (H_2). Only the measure of Quality of Help suggested a marginally significant mediation; none of the others approached significance ($Estimate_{Quality\ of\ Help}$=-1.42; χ^2=3.04; p=.0812; $Estimate_{Downward\ Social\ Comparison}$=0.80; χ^2=1.31; p=.2527; $Estimate_{Likelihood\ of\ Approval}$=-2.13; χ^2=2.59; p=.1076; $Estimate_{Emotional\ Similarity}$=-0.04; χ^2=0.01; p=.9054; $Estimate_{Shame}$=0.78; χ^2=2.10; p=.1475). The effect of respect on charity choice became marginally significant when including screened participants ($Estimate_{Respect}$=-0.56; χ^2=3.72; p=.0539). Quality of Help became significant ($Estimate_{Quality\ of\ Help}$=-1.02; χ^2=9.81; p=.0017) and Downward Social Comparisons became marginally significant ($Estimate_{Downward\ Social\ Comparison}$=-0.45; χ^2=3.03; p=.0819). All other mechanisms remained non-significant ($Estimate_{Likelihood\ of\ Approval}$=0.13;

χ^2=0.12; *p*=.7324; Estimate$_{Emotional\ Similarity}$=-0.12; χ^2=0.85; *p*=.3556; Estimate$_{Shame}$=0.06; χ^2=0.06; *p*=.8118).

Discussion

Results from this experiment suggest that people are less likely to seek help from charities when they display sad (vs. happy) people in their ads. We detect (marginally) significant mediation through the Quality of Help mechanism. No evidence of mediation was found for any of the other mechanisms.

EXPERIMENT 3 REPLICATION

This study provides evidence of the replicability of findings reported in manuscript Experiment 3.

Method

Participants and design.

This experiment followed a two group (Emotional Depiction: Sad vs. Happy) between-subjects design using Amazon Mechanical Turk workers making less than $20,000 per year (n=151). Participants completed the survey in exchange for $0.50.

Procedure.

The procedure for this was identical to that of experiment 3, with the exception that participants did not fill out any measures for Quality of Help. The stimuli are included in Figure 1.15.

Measures.

The DV was identical to that in experiment 3. The respect measure was identical to that in experiment 3.

Figure 1.15: Experiment 3 Replication stimuli

Results

Sample size and respondent screening.

My target final sample was approximately 150 respondents with income of less than $20,000 per year. Due to an issue with the fielding vendor's information about their Amazon Mechanical Turk panel, reliable income information could not determined prior to the survey for prescreening. To avoid the possibility that querying income as a screening measure would make financial constraints salient, participants were asked their income at the end of the study.

This experiment included the screening criterion that participants not remember having taken a similar study. It also included the impossible/conflicting statement attention check and the timing attention check used in experiment 4, and used in Appendix C, Replications 1-4. To obtain a valid sample of 150 participants making less than $20,000 per year, I collected a total of 759 responses, of which 593 were compensated but removed prior to analysis for exceeding my income threshold. Of the remaining 166 participants, nine respondents were screened for failing the impossible/conflicting statement attention check, five were screened for failing the stimuli

screen check, and one was screened for remembering taking a similar study, leaving a final sample of 151.

Help-seeking.

Consistent with H_1, help-seekers were less likely to seek help when the person in the ad they saw was sad ($Seeking_{Sad}$=16% vs. $Seeking_{Happy}$=32%, χ^2=4.98, p=.0257). Findings were robust to the screening criteria ($Seeking_{Sad}$=16% vs. $Seeking_{Happy}$=32%, χ^2=5.57, p=.0182). Findings were also robust to including participants making more than $20,000/year ($Seeking_{Sad}$=12% vs. $Seeking_{Happy}$=18%, χ^2=5.57, p=.0183).

Inferred respect.

All items on the respect measure loaded onto one factor (λ_1=3.56, λ_2=.18; α=.96). Consistent with expectations, help-seekers inferred less respect from the charity when their ad featured a sad, as opposed to happy, person ($Respect_{Sad}$=3.83 vs. $Respect_{Happy}$=4.88, t=4.17, p<.0001). Findings were robust to the screening criteria ($Respect_{Sad}$=3.80 vs. $Respect_{Happy}$=4.85, t=4.41, p<.0001). Findings were also robust to including participants making more than $20,000/year ($Respect_{Sad}$=3.76 vs. $Respect_{Happy}$=4.47, t=6.20, p<.0001).

Mediation.

A bootstrapping mediation model (PROCESS Macro Model 4) was estimated with the emotional depiction in the ad as the independent variable, respect as the mediator, and help-seeking as the dependent variable. The decision to seek help was fully mediated by respect (H_2) (Indirect Effect=-.53; SE=.16; 95% CI=[-.91, -.27]; Direct Effect=-.13; SE=.23; p=.5859). Findings were robust to the screening criteria (Indirect Effect=-.51; SE=.15; 95% CI=[-.87, -.26]; Direct Effect=-.15; SE=.22; p=.5032). Findings were also robust to including participants

making more than $20,000/year (Indirect Effect=-.32; SE=.06; 95% CI=[-.45, -.21]; Direct Effect=.01; SE=.12; p=.9520).

FOLLOW-UP EXPERIMENT AMONG SEEKERS AND DONORS

This experiment was designed to test the effect of sad portrayals on both help seekers and donors. If the results of Small & Verrochi (2009) were upheld, we would see people prefer seeking help from non-profits that utilize happy people in their ads, but prefer donating to non-profits that utilize sad people. This assumes, however, that people perceive sad portrayals the same way they did in 2009. On the contrary, particularly since 2020 we as a society have been more aware of the negative treatment of marginalized people, including disrespect. Therefore, results counter to Small & Verrochi (2009) would be expected.

Unfortunately, no stimuli from Small & Verrochi (2009) still exist. In addition, all experiments were conducted using stimuli for children's cancer research non-profits. Not only can people not seek help from that type of non-profit, there might be different dynamics when seeking help for oneself versus one's child. I approximated the conditions of Small & Verrochi (2009) by studying help seeking from a different type of medical non-profit (free clinics).

Method

Participants and design.

This experiment (N=300) used a 2 (Role (between-subjects): Donor vs. Seeker) x 2 (Emotion of target ad (within-subjects): Sad vs. Happy) group mixed design. As in experiment 1B, because free clinics, the non-profit in this experiment, usually only help low-income people, participants were prefiltered by a fielding partner to those making less than $20,000/year.

Procedure.

Participants imagined either that they needed medical treatment, but cannot pay for it (Role: Seeker condition) or that they had extra money and wanted to donate to a free clinic (Role: Donor condition) They searched online for nearby free clinics, finding two. Participants were told they visited each website, one by one, wherein they were shown a fundraising ad which featured prominently on that non-profit's home page. As in prior experiments, the order of differences in ad execution (organization/ad language, person, background color) were orthogonally randomized. The stimuli (an example set of which is below in Figure 1.16) were created to approximate pre-existing non-profit donation appeals (Figure 1.17).

Measures.

Participants indicated which non-profit they would go to for help/donate to as the dependent measure.

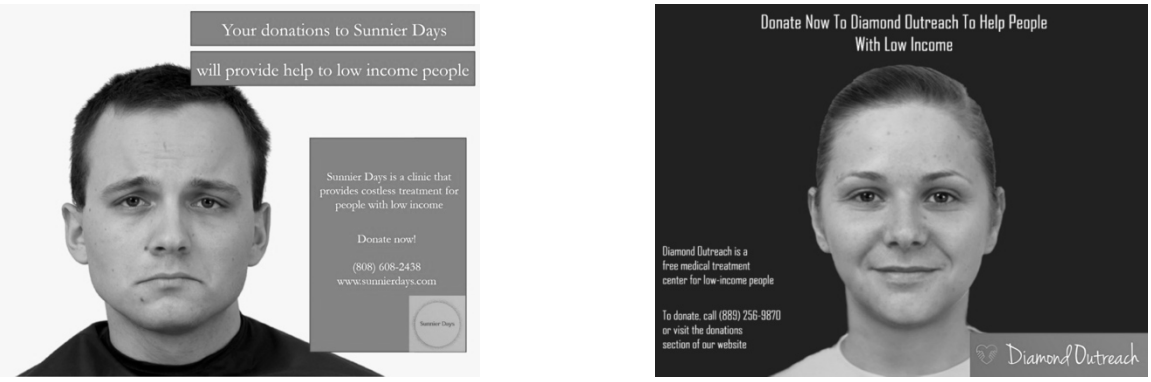


Figure 1.16: Example stimulus set in donors and seekers experiment

Figure 1.17: Examples of real ads on which Figure 1.16 stimuli were based

Results

Sample size and respondent screening.

I set a target sample size of 150 per between-subject condition. There was one screening criterion: participants were asked whether they recall ever taking a survey with a similar ad, charity, or questions. If a participant said "yes" they were screened. 303 responses were collected and three were screened prior to analysis, leaving 300 participants.

Confound check: Ad execution.

I first tested to see whether non-focal differences in ad executions explained participants' choices. A logistic regression—predicting whether the sad face was chosen, using other elements of the ad executions as IVs – suggested that variation in ad execution elements did not explain participants' choices. No significant differences based on ad execution elements emerged for the Help Seeker ($ps \geq .25$), or Donor ($ps \geq .13$) conditions.

Help-seeking.

Chi-square tests indicate that, consistent with H_1, participants were less likely to select to seek help from the non-profit featuring a sad as opposed to happy facial expression ($Choice_{Seeker|Sad}$=9% vs. $Choice_{Seeker|Happy}$=91%, χ^2=101.54, p<.0001). They were also less likely to donate to the non-profit featuring a sad as opposed to happy facial expression ($Choice_{Donor|Sad}$=23% vs. $Choice_{Donor|Happy}$=77%, χ^2=45.62, p<.0001).

Discussion

This experiment shows that people in need are not the only ones who have an aversion to non-profits that use sadness appeals. People are also less likely to donate to non-profits when they use sadness appeals. While this runs counter to the findings of Small & Verrochi (2009),

social perceptions are different in 2024 than they were in 2009. With the rise of the #MeToo movement and Black Lives Matter, among others, people have become much more aware of negative treatment of marginalized peoples, including people in need. It may be that now, more than fifteen years ago, people are able to see the negative connotations of the sad portrayals.

Endnotes

1. Respondents were initially asked to first imagine they saw both ads at bus stops. This instruction was removed after 6 respondents, given the observed lack of clarity it introduced. Results are robust to those responses' inclusion.

2. Findings remain robust to including the four screened responses ($Choice_{Sad}$=35% vs. $Choice_{Happy}$=65%, χ^2=13.59, p=.0002).

3. Findings are robust to the screening criteria (All Participants: Sad vs. Happy Comparison: $Choice_{Sad}$=13% vs. $Choice_{Happy}$=87%, χ^2=82.53, p<.0001; Sad vs. Neutral Comparison: $Choice_{Sad}$=27% vs. $Choice_{Neutral}$=73%, χ^2=33.94, p<.0001; Neutral vs. Happy Comparison: $Choice_{Neutral}$=27% vs. $Choice_{Happy}$=73%, χ^2=33.66, p<.0001).

4. Findings are robust to the screening criteria ($Choice_{Sad}$=32% vs. $Choice_{Happy}$=68%, χ^2=21.69, p<.0001).

5. Findings are robust to the screening criteria ($Respect_{Sad}$=3.69 vs. $Respect_{Happy}$=4.54, t=-7.40, p<.0001).

6. Findings were robust to the screening criteria (QoH_{Sad}=4.19 vs. QoH_{Happy}=4.90, t=-6.30, p<.0001).

7. Findings were robust to the screening criteria (DSC_{Sad}=4.47 vs. DSC_{Happy}=3.90, t=6.06, p<.0001).

8. Findings were robust to the screening criteria ($Approval_{Sad}$=4.47 vs. $Approval_{Happy}$=4.87, t=-4.71, p<.0001).

9. Findings remained the same when removing the screening criteria ($Seeking_{Sad}$=17% vs. $Seeking_{Happy}$=21%, χ^2=0.43, p=.5106).

10. Findings were robust to the screening criteria ($Respect_{Sad}$=3.47 vs. $Respect_{Happy}$=4.47, t=3.84, p=.0002).

11. Findings were robust to the screening criteria (QoH_{Sad}=3.68 vs. QoH_{Happy}=4.19, t=2.82, p=.0055).

12. Findings were robust to the screening criteria (Indirect $Effect_{Respect}$=-.48; SE=.20; 95% CI=[-.96, -.20]; Indirect $Effect_{QoH}$=.05; SE=.09; 95% CI=[-.11, .27]; Direct Effect=.21; SE=.24; p=.3777).

13. Differences in help-seeking did not change with the inclusion of screened participants ($Seeking_{Sad|Control}$=4.31 vs. $Seeking_{Happy|Control}$=4.75, F=4.26, p=.0393; $Seeking_{Sad|Positive}$=4.73 vs. $Seeking_{Happy|Positive}$=4.63, F=.25, p=.6149), The interaction effect remained marginally significant after including screened participants (F=3.26, p =.0715).

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CHAPTER 2: THANKS BUT NO THANKS: HOW UNUSED DELEGATION OPPORTUNITIES BOLSTER FIRMS AGAINST BLAME FOR NEGATIVE EXPERIENCES

Andrew Morningstar, Cait Lamberton, and Patti Williams

Abstract

Though delegation services can provide aid to overwhelmed, inexperienced, or harried consumers, previous research has shown that not only do consumers rarely delegate, but those who do may do so in order to use firms as scapegoats in the case of bad decisions. Given these findings, what reason would firms have to implement delegation services? In this paper, I find that merely offering a delegation service may protect firms against blame for negative consumption experiences. Specifically, consumers who have the opportunity to use a delegation service – but who choose not to – see firms as more competent and, subsequently, blame companies less for negative consumption experiences than consumers who do not have the opportunity to delegate. The consumer's common "thanks, but no thanks," response to delegation can protect firms, and may make delegation a tool that can be used to signal competence in a way that is both credible and impactful.

Introduction

Delegation—a form of decision support where consumers voluntarily cede control over at least part of their consumption decision (Steffel and Williams 2017)—might be expected to be of substantial benefit to both consumers and firms. In addition to reducing consumer decision time and effort (Solomon 1986), as a type of decision support, it might also offer reduced anxiety, greater satisfaction, greater decision quality, and reduced choice conflict (O'Connor et al. 1999; Häubl and Trifts 2000; Seong and Bisantz 2008; Say et al. 2011). Further, delegation might be widely available to firms, as it can theoretically occur any time consumers might make decisions, and is already present in several different industries. Several examples are given in Figure 2.1.

Panel A: Subscription Boxes

Panel B: Consumer Surrogates (e.g., wardobe consultant, interior designer)

Panel C: Medical decision making

Panel D: AI Services

Panel E: "Blind Date with A Book"

Panel F: Chef's Choice/Tasting Menus/Omakase

Figure 2.1: Examples of delegation consumption

Previous research, however, does not indicate that these benefits are likely to materialize reliably. First, consumers do not delegate often—approximately one in three do so, according to prior research (Bartling and Fischbacher 2011; Steffel, Williams, and Perrmann-Graham 2016a; Steffel and Williams 2017; Gawn and Innes 2019; Gawn and Innes 2021). This is not surprising, given previous literature suggests personal control is a biological need (Leotti, Iyengar, and Ochsner 2010) and consumers value having their own choices, even at the cost of satisfaction (Botti and Iyengar 2004). Second, research shows people are motivated to delegate in order to use the delegatee as a scapegoat (Bartling and Fischbacher 2011; Steffel et al. 2016a; Steffel and Williams 2017). It would seem delegation services would therefore largely be harmful for companies: consumers would not be likely to use such a service, and if they do, they may use it to blame companies when something goes wrong.

I argue that even if – and particularly if– consumers do not use a delegation service, it may still be useful for firms to offer such options. Across five experiments, I find that the mere presence of a delegation option can protect firms against blame when consumption outcomes are bad. Specifically, consumers who have the opportunity to delegate but instead make choices themselves blame companies less for negative consumption experiences than those who made choices themselves without the ability to delegate. This occurs because the availability of a

delegation service signals the company's competence, which in turn buffers the company against damage related to experienced bad outcomes. Therefore, ironically, delegation services may benefit firms because of their effects specifically on people who will not use them.

This finding contributes to theoretical and managerial knowledge in several ways. First, I show that delegation services are signals of a firm's competence. By identifying this signal, I am able to show positive outcomes of offering consumers the opportunity to delegate. This is a complementary set of outcomes to prior work which suggests primarily negative attributional outcomes associated with delegation (Bartling and Fischbacher 2011; Steffel et al. 2016a; Steffel and Williams 2017). Second, dominant marketing paradigms assume that consumers find greater utility in services with which they engage, often targeting program take-up, adoption, or use as goals (e.g., Vargo and Lusch 2004; "value in use"). By contrast, I offer the novel insight that there may be value in *disuse*. This finding suggests that fruitful research may explore other cases where unused goods or services provide meaningful value to consumers and firms.

Practically, my research paradigm offers better insight than has prior work into the question of whether firms should offer delegation services. As prior research has always held the ability to delegate constant, it could not provide clear guidance on the question of whether firms should or should not offer delegation services. This guidance relies on comparing outcomes when the service, versus is not, available, which this paper does. Finally, my work helps explain why many firms may continue to offer delegation services, despite the risk and low levels of use identified in prior work.

Below I present a theoretical framework for understanding this phenomenon. I then present five experiments to test my predictions. Across these experiments, I find that the mere availability of a delegation service – when it is not used – protects the firm from blame following

negative consumption experiences. I provide evidence that this attribution of blame is driven by the perceived competence of firms that offer delegation services, over and above three separate alternative explanations: differences in perceived autonomy, in counterfactual thinking, and in the perceived novelty of the company. Further, I show that these processes have a beneficial, managerially-relevant downstream effect: people are more likely to return to the firm if they have a delegation service, even after failure.

Theoretical Background

Delegation

A delegation service is any offer or program by an individual or firm to take over part or all of a consumption decision. An example of delegation occurs in grocery delivery apps like Instacart, wherein consumers outsource the choice of specific items of produce to personal shoppers, and cannot change the shopper's choice. Previous research indicates people may use these delegation offerings for either functional (saving effort, greater access to consumer goods) or symbolic (guidance, status) reasons, and that delegation can be very useful to people who need help (Solomon 1986; Solomon 1987; Forsythe, Butler, and Schaefer 1990). While there has been no research on how delegation services affect the firms who offer it, research on other decision supports suggests consumers are more engaged with (Jannach and Jugovac 2019) and more loyal to (Ansari, Essegaier, and Kohli 2000) companies that offer decision supports; they are also more likely to purchase supported products (Senecal and Nantel 2004). Theoretically, then, delegation services should be beneficial for both consumers and firms.

There are reasons, however, for firms to hesitate to offer such delegation services. Research has shown that people want to avoid responsibility for bad outcomes, and will delegate to pass responsibility to the delegatee (Bartling and Fischbacher 2011; Steffel et al. 2016a; Steffel and Williams 2017). This desire to transfer blame is greater when people might be punished for unfair behavior (Bartling and Fischbacher 2011), when making difficult decisions (Steffel and Williams 2017), and when making decisions for others (Steffel et al. 2016a). This scapegoating does work: people attribute negative experiences to others more when a consumer delegates the decision (Steffel and Williams 2017); those consumers also avoid punishment to a greater degree by delegating (Bartling and Fischbacher 2011). Given the results of these recent papers, and their coverage in managerial magazines (Steffel, Williams, and Perrmann-Graham 2016b; Steffel and Williams 2016), it is fair to wonder what manager would want to institute a service that would purportedly be used to transfer blame to them.

I suggest, however, this is not the full story. In describing why and when consumers delegate—as examined in previous research on delegation and blame—researchers always gave all participants the ability to delegate. In evaluating the worth of a service, however, managers must compare consumers' behavior when the service is present to when it is not present (i.e., when there is no ability to delegate). Delegation services do not merely offer a means by which consumers can use and then blame firms for negative outcomes. I argue that they constitute an important signal. This signal can protect firms from blame, rather than increasing their exposure, especially when they are not used. I explain this process and the hypotheses it suggests next.

Delegation Services and Competence

Competence—the ability to carry out one's intentions (Aaker, Garbinsky, and Vohs 2012)—is a very important social inference. Research indicates that competence is one of only

two dimensions that universally underlie perceptions of others (Fiske, Cuddy, and Glick 2007; Cuddy, Fiske, and Glick 2008; Aaker et al. 2012; Kervyn, Fiske, and Malone 2012; Wang, Mao, Li, and Liu 2016). Competence is also a determinant of trust (Bhattacherjee 2002; McKnight, Choudhury, and Kacmar 2002; Xie and Peng 2009) and is positively related to willingness to buy from companies (Aaker, Vohs, and Mogilner 2010).

I propose that offering a delegation service is perceived as a signal of competence. With advice or recommendations, the consumer must decide whether to take the company's advice after it is given, which may make the customer feel liable for the outcome. With delegation, since the company makes the ultimate decision alone, the company is less able to deflect blame. Since receiving blame may have negative consequences for companies (Folkes 1984; Richins and Verhage 1985), consumers may believe the firm would not willingly accept such risks unless it had high opinions of its abilities (Krueger Jr and Dickson 1994). The mere availability of the delegation service, then, may serve as a strong indication of a firm's capabilities.

However, among consumers who have the opportunity to delegate, this competence signal will be meaningful only for consumers who do *not* use the delegation service. Watching firms choose products themselves is a direct indication of competence. The few consumers who choose to delegate will base their evaluation on the experience that is provided rather than on the availability of that experience; if this experience is negative, it will undermine the original signaling power of the opportunity (Skowronski and Carlston 1987; Feldman and Lynch 1988). The signal value of offering to delegate will still be preserved for those who opt not to use the service. In summary, this leads me to propose a set of planned contrasts that depend first on whether consumers have a delegation opportunity, and subsequently, on whether they take that opportunity:

H1: The extent to which consumers interpret the presence of a delegation option as a sign of competence depends on whether they use it, such that:

a. Consumers who have the opportunity to delegate but choose not to will perceive firms as more competent than consumers without the opportunity to delegate, while
b. Consumers who have the opportunity to delegate and do delegate will not perceive the firm as more competent than consumers without the opportunity to delegate

Competence and Attributions of Blame

As noted, past work shows that one down-side of delegation is blame. If a firm that offers (unused) delegation is seen as more competent than one that doesn't, what implications does this have for consumers' attributions for product failures?

If the competency signaling value of delegation exists, an expectancy perspective to attributions (Feather 1969; Feather and Simon 1971; Miller and Ross 1975) can help answer this question. Under this framework people make attributions based on how likely they thought the outcome would be: they believe unexpected outcomes are more likely caused by situational factors such as luck or fate, while expected outcomes are caused by the person performing the task. As failure is unexpected from competent people or organizations, and delegation services make companies appear more competent, consumers who see a company offering a delegation service should attribute failure less to the company than would consumers who do not have access to a delegation service. Further, since the competence inference from offering to delegate

exists only among those who do not take the offer, the firm is most protected from blame in cases when people do not use the delegation service. As choosing a product is a more direct indication of competence, when the experience is negative delegators will use this more diagnostic (negative) indicator to determine blame.

In summary:

H2: The attribution of blame for negative consumption experiences depends on consumers' opportunity and adoption of delegation services, such that:

a. Consumers who have the opportunity to delegate but choose not to will attribute negative consumption experiences to firms less than consumers without the opportunity to delegate, while
b. Consumers who have the opportunity to delegate, and choose to, will not attribute negative consumption experiences to firms less than consumers without the opportunity to delegate

H3: The effects in H2a will be mediated by differences in perceptions of firm competence.

I make no hypotheses about whether the effect in H2b is mediated by competence. The two groups of people in H2b differ not just in the opportunity to delegate, as in H2a, but also who chooses the product. There is no guarantee that this additional difference will not affect perceived competence, or affect how competence relates to blame.

Overview Of Studies

I test my predictions in five experiments. In all experiments I focus strictly on negative consumption experiences and blame. This is a direct response to the focus on blame in the delegation papers of the last decade. I recognize that evaluating the full worth of a service involves many additional considerations, including responses to positive experiences. I return to this in the general discussion.

Experiment 1, a pilot study, tests H1a and H1b. I find that consumers who have the opportunity to use a delegation service, but choose not to, perceive the company as more competent than people without the service (H1a) and people who have the service available and use it perceive the company as less competent (H1b). Experiment 2, in a consequential, behavioral paradigm about book choice, tests H2a and H2b. Here, I find that, relative to consumers buying from a firm without a delegation service, consumers given the opportunity to delegate, but who do not use it, blame companies less for negative consumption experiences (H2a), and consumers offered the opportunity and do delegate blame companies more (H2b). Experiment 3, which uses a gift-giving scenario, replicates support for H2a and H2b. In addition, experiment 3 eliminates alternative explanations for H2a and H3: these effects do not occur because consumers who have the opportunity to delegate are ruminating on the delegation decision as a foregone alternative, nor is it because having the opportunity to delegate changes consumers' perceived autonomy. Experiment 4, which uses a vacation scenario, and focuses specifically on people who choose themselves (with or without the opportunity to delegate), replicates H2a. Experiment 4 also shows that the differences in company blame are not caused by the novelty of delegation services. In experiment 4 I also demonstrate a behavioral

consequence from the opportunity to delegate: consumers who choose themselves are more likely to return to the company despite the negative experience if they had the opportunity to delegate. Most importantly, experiment 4 measures company competence and tests it as a mediator for blame (H1a and H3). Finally, experiment 5 provides more evidence that competence mediates blame by offering a more diagnostic signal. Its results support H1a, H2a, and H3.

All findings are robust to the inclusion of screened participants. Results including screened participants are included in endnotes.

Experiment 1

This experiment establishes that delegation services affect firms' perceived competence, but only among people who do not use the service, consistent with H1a and H1b. Participants were randomly assigned to one of three delegation conditions (Delegated, Did Not Delegate, No Service). This experiment followed a scenario paradigm. Those in the first two conditions were told that the company had a delegation service which could choose the product for them, and were told, in the scenario, whether they did (Delegated) or did not (Did Not Delegate) use it. Participants who did not have the option to delegate (No Service) were never told about the service. Regardless of delegation condition, all participants subsequently experienced a failed experience in the scenario.

Method

Participants and design.

Experiment 1 followed a three (No Service, Did Not Delegate, Delegated) group between-subjects design. Three hundred participants (Gender: 40% Female, 59% Male, 0% Non-Binary, 0% Other; Median Age: 34 years) were recruited from Prolific, of whom all 300 were retained. Preregistration outlined at least 100 people per condition. Delineation and assignment of conditions are explained below under Procedure. The exclusion criterion (see Sample Size and Respondent Screening) was preregistered. Participants were paid $1.00 each.

Procedure.

Participants imagined they had visited a website to book a flight and hotel for a vacation with a significant other. Participants in the Delegated and Did Not Delegate conditions were told that the company had a delegation service (called "Let Us Choose") which could choose the hotel and flight for them. They were told, in the scenario, whether they did (Delegated) or did not (Did Not Delegate) use it. Participants who did not have the option to delegate (No Service) were never told about the service, and were simply told after they reached the website they found and booked a hotel and flight themselves. To create a negative consumption experience, all participants were told the hotel beds were lumpy, the showers had no water pressure, and they got injured on one of the activities during the vacation. They were told their significant other tried to be cheerful, but that it was obvious that person was miserable. After finishing the scenario, participants completed a competence measure.

Measures.

To measure competence, participants rated their agreement with a series of statements including "The website performs its role very well" and "The website is an expert in what it's supposed to do" on a scale from 1 (Strongly disagree) to 7 (Strongly agree) (McKnight et al. 2002).

Results

Sample size and respondent screening.

My target final sample was 100 respondents per condition, after screenouts. I collected 300 responses. One screening criterion was applied before analysis. This criterion was developed in the paper comprising the previous chapter of this dissertation, based on best practices for capturing inattention: asking participants to rate impossible or nonsensical statements as true or false (Meade and Craig 2012), or looking for objectively inconsistent responses (McKibben and Silvia 2016). To this end, at the end of the survey participants answered a series of true/false questions (e.g., "I eat meat"). Two of the statements were "I have run a mile in 30 seconds or less" and "I have never run a mile in 30 seconds or less". As the first statement is impossible, and the second an inverse of the first, the only correct answers would be false and true, respectively. Participants who failed to answer as such were screened prior to inclusion in the dataset. No participants were excluded because of this mile running attention check. This left 300 participants (102 No Service condition; 98 Did Not Delegate condition; 100 Delegated condition).

Competence.

Two t-tests (No Service vs. Did Not Delegate; No Service vs. Delegated) estimated whether there were differences in perceived competence between the groups (H1a and H1b, respectively). As hypothesized, relative to No Service participants ($M_{No\ Service}$=2.34), Did Not Delegate participants perceived the firm as more competent ($M_{Did\ Not\ Delegate}$=3.64, t(198)=6.30, p<.0001) (H1a). Conversely, Delegated participants perceived the company as less competent than No Service participants did ($M_{Delegated}$=1.77, t(185.63)=-3.54, p=.0005) (H1b).

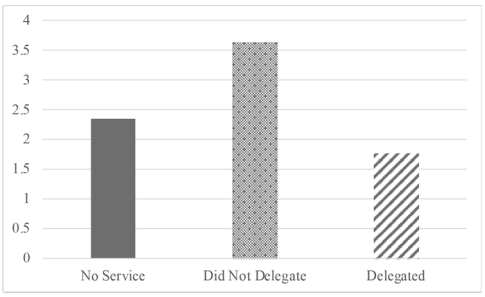

Figure 2.2: Perceived competence in Experiment 1

Discussion

The results of this study suggest that providing a delegation service affects firms' perceived competence: compared to consumers who do not have the opportunity to delegate, consumers who have the opportunity to delegate, and choose not to, perceive the company as more competent (H1a). Consumers who chose to delegate, however, perceive the company as less competent (H1b). This supports my hypotheses that offering a delegation service can make firms appear more competent, but only among the people who do not use the service. Following this I examine my hypotheses about company blame. I test this first in a behavioral, consequential paradigm for external validity. Participants were able to self-select whether they would delegate, unlike in this experiment.

Experiment 2

This experiment establishes how delegation services affect attributions of blame after negative consumption experiences. Participants were randomly assigned to one of three delegation conditions (Delegated, Did Not Delegate, No Service). Those in the first two conditions were told that the company had a delegation service which could choose the product for them; participants decided whether they would (Delegated) or would not (Did Not Delegate)

use it. Participants who did not have the option to delegate (No Service) were never told about the service. Regardless of delegation condition, all participants subsequently experienced a failed experience with the company.

Method

Participants and design.

Experiment 2 followed a three (No Service, Did Not Delegate, Delegated) group between-subjects design. Three hundred forty three participants (Gender: 47% Female, 51% Male, 2% Non-Binary; Median Age: 35 years) were recruited from Prolific, of whom 337 were retained (Gender: 47% Female, 51% Male, 2% Non-Binary; Median Age: 35 years). Preregistration outlined at least 100 people per condition. Oversampling was needed to reach the quota in all groups because some conditions were self-selected, and participants did not self-select into those conditions equally. Delineation and assignment of conditions are explained below under Procedure. The exclusion criterion (see Sample Size and Respondent Screening) was preregistered. Participants were paid $1.00 each.

Procedure.

Participants were told that the researchers were partnering with an eBook company named UbiLit, which sells books by independent authors. Participants were told they would choose one of the books that UbiLit was going to release the following week, read an excerpt from it, and answer some questions. To make the book choice consequential, they were told they would receive the full book for free and be entered into a lottery to receive a collection of books UbiLit had in stock by that book's author. UbiLit, and its books, were fictional.

Before seeing the list of excerpted books, some participants were told about a delegation service UbiLit offered called "Let Us Choose," which chooses books for users based on their

budget and their responses to a few questions (e.g., favorite genre). Participants were informed they could use that service to choose the book excerpt they would read in this experiment. As mentioned previously, delegation requires ceding full control over the decision. To maintain that requirement, participants were told if they used the service they could not change the selection afterwards; if they won the abovementioned lottery, they would receive books by the author that UbiLit chose for them.

Participants who chose to use the service constituted the Delegated condition; participants who chose not to use the service constituted the Did Not Delegate condition. All other participants, who were in the No Service condition, were given the instructions for the task and then chose a book excerpt without being told about the Let Us Choose service.

Regardless of the book chosen, all participants read the same excerpt, which was written to be uninteresting in order to induce a negative (failed) product experience. The excerpt was written with text from a manual on repairing potholes, in a section on diagnosing pothole severity. The text was difficult to understand and monotonous, without any apparent plot development or stimulation. Researchers have shown these characteristics are associated with uninteresting activities (Parker and Lepper 1992; Reeve et al. 2002; Swarat 2008). To convincingly portray this manual text as a book excerpt, it was written as dialogue of a lesson being given to an apprentice on a construction site. The scene was written to not involve any connection to an overarching plot, in a setting that lacks embellishments which would lead to interest (Parker and Lepper 1992). The text of the book excerpt can be found in Appendix A. To confirm that the book excerpt was perceived negatively, after reading participants were asked to write down thoughts they had while reading the book excerpt (up to three thoughts). Each

participant's remarks were coded as positive or negative. After recording their thoughts, participants completed my measure of blame and several filler questions.

Measures.

I measured attributions of blame for the negatively received book excerpt by asking participants to rate their agreement with the statement "If it weren't for ____, I would not have read this excerpt" for four entities: "Me", "UbiLit", "The author", "Someone/something else" on a 1 (Strongly disagree) to 9 (Strongly agree) scale. The score for "UbiLit" was used to measure company attributions. The other entities in the measure were fillers, chosen to provide a complete list of the possible entities the consumer could have blamed for the experience.

Results

Sample size and respondent screening.

My target final sample was 100 respondents per condition, after screenouts. Because the Did Not Delegate and Delegated conditions were self-selected, and not evenly selected, oversampling was needed to reach the quota for every condition. To that end, I collected 343 responses. The only screening criterion was the true/false attention check introduced in experiment 1. Six participants were excluded because of this true/false attention check. This left 337 participants (108 No Service condition; 100 Did Not Delegate condition; 129 Delegated condition).

Excerpt perceptions.

An analysis of the thoughts participants had while reading the book excerpt confirmed that participants perceived the book negatively. 80% of participants reported at least one negative thought about the excerpt. Responses included "Wow that is boring", "Wouldn't read more if there was more", and "Aweful". To confirm that participants felt the experience was purely

negative, the responses were also coded for positive thoughts. Only 15% of participants recorded at least one positive thought about the excerpt.

Blame.

Two t-tests (No Service vs. Did Not Delegate; No Service vs. Delegated) estimated whether there were differences in attributions between the groups (H2a and H2b, respectively). As hypothesized, relative to No Service participants ($M_{No\ Service}$=6.56), Did Not Delegate participants attributed blame less to companies ($M_{Did\ Not\ Delegate}$=5.35, t(206)=-2.92, p=.0039) (H2a). Conversely, Delegated participants attributed the negative experience more to companies than No Service participants did ($M_{Delegated}$=7.91, t(169.95)=4.32, p<.0001)[1] (H2b).

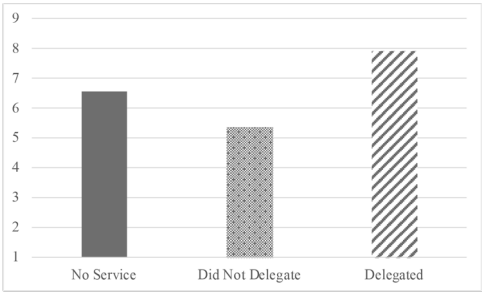

Figure 2.3: Perceived blame in Experiment 2

Discussion

The results of this study suggest that providing a delegation service affects consumers' blame for a negative consumption outcome: compared to No Service consumers, Delegated consumers attributed the negative experience to the company more (H2b), but Did Not Delegate consumers, who had the opportunity to delegate but chose not to use it, attributed the experience less to the company (H2a). The latter effect is a crucial counter to current theories of service value. Value co-creation posits that the value of a service comes from consumers' use of the service. If the use of the service has negative consequences to the company (H2b), it should not be created. I show, in the experiment, comparing No Service and Did Not Delegate participants,

that companies may derive worth from the people who do not use the service. In my next experiment, I replicate H2a and H2b in a gift-giving scenario.

While using a behavioral paradigm in experiment 2 offers external validity, it is subject to potential self-selection biases. Experiment 3 assigns all participants to conditions, eliminating self-selection concerns.

Reading book excerpts for upcoming releases, the focus of this experiment, is not a common consumption experience. For greater generalizability I make experiment 3 a scenario about gift giving, an act that occurs regularly. By creating a gift-giving scenario, I also demonstrate that this effect occurs both in decisions for the self and decisions for others.

Experiment 3

Experiment 3 replicates H2a and H2b, using a different consumption category for generalization, and assigning all participants to conditions to account for possible self-selection biases. Experiment 3 also tests two alternative explanations. The first explanation is that consumers who have the opportunity to delegate blame companies differently from consumers without the opportunity because they are ruminating on the opportunity to delegate as a foregone alternative. When people encounter failure, they generate counterfactuals (thoughts about how the situation could have turned out differently) to make sense of the situation (Roese 1997). These counterfactuals can be divided into upward (how the situation could have been better) and downward (how the situation could have been worse) counterfactuals. Counterfactuals are associated with blame (Roese and Olson 1996), in that if people think the outcome could have been different due to a controllable decision they or someone else made, they would blame that

person for not making the opposite decision (Mandel 2003). Consumers with access to a delegation service have a salient counterfactual that consumers without this service do not: "what would have happened if I had actually delegated/not delegated in this scenario?"

This experiment also addresses a second alternative explanation. The differences between consumers who have the opportunity to delegate but choose themselves, and consumers who do not have the opportunity to delegate, may occur because refusing to use a delegation service may increase one's sense of autonomy. Autonomy is defined as one's capacity for control over one's environment, in accordance with one's desires (Deci and Ryan 1985; Sheldon and Deci 1996). The greater autonomy one feels, the less one attributes actions and outcomes to others (Turban, Tan, Brown, and Sheldon 2007). While, in the previous experiment, both the aforementioned groups of participants chose books themselves, the participants with the opportunity to delegate had to decide first whether they would use the delegation service instead. Making this additional decision might reaffirm to consumers that they are able to control the outcome, increasing autonomy.

I included one control specific to this scenario: participants might naturally differ in how close they think they are to the friend for whom they purchased a gift in this experiment's scenario. This would be a concern, because choosing gifts can cause an identity threat depending on whether the friend is close or distant (Ward and Broniarczyk 2011), and identity threats have been shown to change attributions (Costarelli 2011; Costarelli and Gerłowska 2014). Therefore, I control for how close participants felt they were to their friend in the scenario.

Method

Participants and design.

Experiment 3 followed a three (No Service, Did Not Delegate, Delegated) group between-subjects design. Three hundred and four participants (Gender: 64% Female, 34% Male, 3% Non-Binary; Median Age: 29.5 years) were recruited from Prolific, of whom I retained 300 (Gender: 63% Female, 34% Male, 3% Non-Binary; Median Age: 29.5 years). All exclusion criteria were preregistered. Participants were paid $1.00 each.

Procedure.

Participants were asked to read a scenario in which they went to a website to buy a birthday gift for a friend. They were told that, upon arriving to the website, they saw a pop-up asking if they were buying a gift for someone; when participants said yes, the website asked questions about the recipient (e.g., gender, age). Participants in the Did Not Delegate and Delegated conditions were then told the website had a gift giving service which would choose a gift, wrap it, and ship it to the gift-giver without any further input, chosen based on answers to the aforementioned questions. Participants were then told whether they had, in the scenario, decided to use the service (Delegated) or find the gift themselves (Did Not Delegate). Participants with no access to a delegation service in the scenario (No Service) were told the pop-up disappeared after answering its questions about the recipient, and they continued to search for and choose a gift. In other words, the difference between No Service and Did Not Delegate participants is No Service participants were never told about the delegation service. All participants were told the gift came from that site; they did not buy a gift elsewhere. To create a negative product experience, participants were told their friend was visibly disappointed upon opening the gift. After finishing the scenario, participants completed measures for blame and the two alternative explanations. They also answered a question about how close they thought they were to the friend in the scenario.

Measures.

To assess attributions of blame toward the company for the unwanted gift, participants rated their agreement with the following statements: "I feel as if the website is completely to blame for this gift" and "I feel as if the website caused this gift to be given," on a 1 (Strongly disagree) to 9 (Strongly agree) scale. The two statements correlated highly ($r = 0.79$) and so were averaged to create a blame index. To avert potential demand effects, participants also rated corresponding statements about blame for themselves and for external circumstances (e.g., luck, fate). Those statements were included as filler and to avoid demand effects, as in experiment 2.

I examined the counterfactual explanation for blame in two ways. First, I asked questions to directly measure both types of counterfactuals, upward and downward. To measure upward counterfactuals I asked participants "People who experience negative events might think about how the event might have turned out better than it actually did. Did you have any such thoughts while you were imagining yourself in this scenario?" To measure downward counterfactuals I asked "People who experience negative events might think about how the event might have turned out worse than it actually did. Did you have any such thoughts while you were imagining yourself in this scenario?" Second, I indirectly tested the counterfactual explanation through the effects of counterfactuals on emotions. Counterfactuals are associated with specific emotions (Mandel 2003), primarily regret (Gilovich and Medvec 1995; Zeelenberg 1999). If participants differentially generated counterfactuals after a negative consumption experience, I would expect participants to feel different levels of those emotions between conditions. Participants rated the degree to which the scenario made them feel a series of emotions, from 1 (Not at all) to 7 (Extremely) (Mandel 2003). The emotions included several associated with counterfactuals (Regret, Relief, Disappointment, Guilt, and Shame) and others not associated with

counterfactuals (Delight, Embarrassment, Sadness, Happiness, Pride, Anger, Thankful, and Frustration). The emotions not associated with counterfactuals were fillers.

To examine differences in perceived autonomy, participants completed the Self-Determination Scale (Sheldon 1995; Sheldon and Deci 1996). The original measure contains two subscales: a sense of choice in one's actions, and awareness of the self. I utilized the statements in the former subscale only. Participants rated their relative agreement between pairs of statements such as "I decide how to live my life"/"My life is decided for me."

Finally, participants were asked to indicate how close they felt to the friend in the scenario, from 1 (Distant friend) to 7 (Close friend). As previously mentioned, this was a covariate included in all analyses.

Results

Sample size and respondent screening.

My target final sample was approximately 300 respondents, after screenouts. To that end, I collected 304 responses. The only screening criterion was the true/false attention check introduced in experiment 1. Four participants were screened for failing that check, leaving a final sample of 300.

Blame.

A one-way ANOVA omnibus test indicated differences among the groups in company blame ($F(3,296)=26.41$, $p<.0001$). Planned contrasts confirmed that Did Not Delegate participants blamed the company less than No Service participants did ($M_{No\ Service}=3.03$, $M_{Did\ Not\ Delegate}=2.24$, $F=7.23$, $p=.0076$) (H2a). Delegated participants blamed the company more than No Service participants did ($M_{No\ Service}=3.03$, $M_{Delegated}=4.72$, $F=29.12$, $p<.0001$)[2] (H2b).

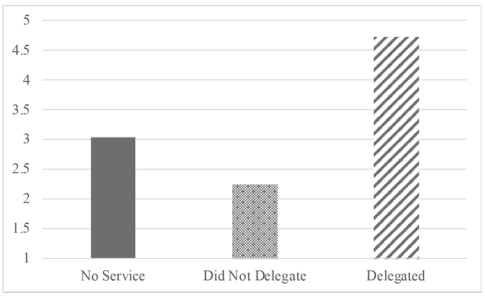

Figure 2.4: Perceived blame in Experiment 3

Direct measure of counterfactuals.

Logistic regressions estimated whether participants' likelihood of having counterfactual thoughts while reading the scenario was affected by the manipulations. Did Not Delegate participants were no more likely than No Service participants to have upward counterfactuals (No Service: 81%, Did Not Delegate: 80%, $\chi^2(1)=0.02$, p=.9014) or downward counterfactuals (No Service: 27%, Did Not Delegate: 25%, $\chi^2(1)=0.04$, p=.8495)[3].

Delegated participants were also no more likely than No Service participants to have made downward counterfactual thoughts (No Service: 27%, Delegated: 24%, $\chi^2(1)=0.17$, p=.6760). They were, however, marginally more likely than No Service participants to generate upward counterfactuals while reading the scenario (No Service: 81%, Delegated: 91%, $\chi^2(1)=3.59$, p=.0581). A marginal effect would not show significant mediation, so to further investigate this finding I ran a regression with company blame as the dependent variable and incidence of upward counterfactuals as the independent variable. There was no significant relationship between the two (β=0.01, t=0.02, p=.9802)[4].

Counterfactual emotions.

One-way ANOVA omnibus tests were run for each of the counterfactual emotions measured. The omnibus tests indicated differences among the groups in all emotions (all

p≤.0116). These effects are attributable to differences between the Delegated and No Service conditions. Participants in the Did Not Delegate condition felt none of the counterfactual emotions to a different intensity than No Service participants did (all p≥.1027). Table 2.1 contains the full results[5].

Contrasts comparing the Delegated and No Service conditions found significant differences for all individual emotions studied (all p≤ .0314). A bootstrapping analysis (PROCESS Macro 4) was run with company blame as the DV, the Delegated vs. No Service manipulation as the IV, and each of the counterfactual emotions as the mediators. There was no mediation for any of the emotions. Table 2.1 contains the full results[5].

	Regret	Relief	Guilt	Shame	Disappoint-ment
$M_{No\ Service}$	5.24	1.80	4.63	4.46	5.24
$M_{Did\ Not\ Delegate}$	4.89	1.64	4.19	4.17	5.23
$M_{Delegated}$	6.06	1.32	5.49	5.09	6.07
Omnibus Test	$F(3,296)$ =8.58, p<.0001	$F(3,296)$ =3.73, p=.0116	$F(3,296)$ =9.32, p<.0001	$F(3,296)$ =3.84, p=.0102	$F(3,296)$ =5.65, p=.0009
Contrast: No Service vs. Did Not Delegate	F=2.00, p=.1587	F=1.30, p=.2558	F=2.68, p=.1027	F=1.04, p=.3076	F=0.00, p=.9891
Contrast: No Service vs. Delegated	F=10.28, p=.0015	F=9.86, p=.0019	F=8.45, p=.0039	F=4.67, p=.0314	F=9.69, p=.0020
Mediation: No Service vs. Delegated (Indirect Effect)	-0.03 95% CI = [-0.13, 0.05]	-0.04 95% CI = [-0.13, 0.02]	-0.01 95% CI = [-0.11, 0.07]	0.03 95% CI = [-0.02, 0.10]	-0.02 95% CI = [-0.11, 0.05]
Mediation: No Service vs. Delegated (Direct Effect)	0.85 95% CI = [0.53, 1.18]	0.86 95% CI = [0.54, 1.19]	0.84 95% CI = [0.51, 1.16]	0.79 95% CI = [0.47, 1.11]	0.85 95% CI = [0.52, 1.17]

Table 2.1: Results of counterfactual emotion analyses in experiment 3

Autonomy.

A factor analysis showed that items in the autonomy measure loaded onto one factor (λ_1=3.21, λ_2=0.61) with a high Cronbach's alpha (α=0.86). An omnibus test showed no differences between the conditions on the autonomy measure ($M_{No\ Service}$ = 3.60, $M_{Did\ Not\ Delegate}$ = 3.68, $M_{Delegated}$ = 3.59, $F(3, 296)=0.70$, p=.5554)[6].

Discussion

This experiment replicates my prior finding that offering a delegation service changes how much people blame companies for negative consumption experiences, depending on whether they use the service. Compared to consumers who do not have access to a delegation service, consumers who chose not to delegate attributed the negative experience to the company less (H2a) and consumers who delegated blamed the company more (H2b). Not only has this effect been generalized to two types of consumption categories—books and gifts—it has been generalized both to decisions for the self and decisions for others. This is especially significant as several papers document differences in consumer behavior when choosing for oneself versus someone else (e.g., Choi et al. 2006; Laran 2010).

This study also eliminates two explanations for these effects. First, the differences in blame did not occur because the members of the groups differentially generated counterfactuals, tested directly—by asking participants whether they had any counterfactual thoughts—and indirectly—by measuring emotions associated with counterfactuals. Second, the differences in blame did not occur because participants felt different levels of autonomy.

Experiment 4

Experiments 2 and 3 demonstrated that the degree to which consumers blame companies for negative consumption experiences changes when companies offer delegation services, but changes differently depending on whether those consumers use the services. The reason that consumers who delegate blame companies more than consumers who can't delegate (H_{2b}) is self-evident: when people delegate it is the company that chooses the product, the company that causes the negative experience, and, therefore, the company which is to blame. The fact that delegation services might be a signal of competence is an indirect heuristic; by using the service consumers actually see the company perform, a direct signal of competence.

The finding that consumers who cannot delegate and consumers who can but choose not to blame companies differently is more interesting. These two groups both choose themselves. Their outcomes with the company are identical since Did Not Delegate consumers chose not to interact with the delegation service. They would be expected, then, to have identical responses. Experiment 4 tests the hypothesized process behind the differences between those two groups: consumers perceive the opportunity to delegate as a signal that the company is competent, which decreases blame (H_{1a} and H_3). It does so in a different, service-based, consumption context (travel agencies).

This experiment also addresses a potential concern from the scenario in the previous experiment. Participants in experiment 3 had no details about the gift to judge why their friend did not like it, and perceived incompleteness of information may bias evaluations (Simmons and Lynch Jr. 1991). In this experiment, as part of the scenario I included specific details about why the consumption experience was negative.

Experiment 4 also examines a third alternative mechanism: the novelty of delegation services. Previous work on salience and attributions (McArthur and Post 1977; Taylor and Fiske

1978) suggests that the behavior of novel objects—in this literature, defined as anything that is unusual or surprising (Mehrabian and Russell 1974)—are judged to be influenced more by situation than disposition. Thus, more novel entities are blamed less. Delegation services are currently not found in all industries, and so participants might consider them novel. Experiment 4 includes a novelty measure to test this. I did not include the recipient closeness covariate from the previous experiment because any differences in closeness I found between conditions did not mediate blame.

Finally, I measured a downstream effect of blame: likelihood to return to the company. If company blame leads to a decreased desire to buy from the company in the future (Hess Jr., Ganesan, and Klein 2003), then participants who choose not to delegate should be more likely than participants who cannot delegate to return to the company, even after a negative consumption experience.

Method

Participants and design.

Experiment 4 had a two (No Service, Did Not Delegate) group between-subjects design. Two hundred and three participants (Gender: 65% Female, 31% Male, 4% Non-Binary; Median Age: 29 years) were recruited from Prolific, of whom I retained 200 (Gender: 66% Female, 31% Male, 4% Non-Binary; Median Age: 29 years). Participants completed the survey in exchange for $0.50. All exclusion criteria were preregistered.

Procedure.

The scenario was identical to that in experiment 1, except with only two of the conditions: Did Not Delegate and No Service. Half of the participants (Did Not Delegate condition) were told about a service called Let Us Choose that would pick the hotel and flight for

them. Participants were told they decided to book the hotel and flight themselves. The other half of participants (No Service condition) did not receive any information about the Let Us Choose service, and were simply told after they reached the website they found and booked a hotel and flight themselves. After finishing the scenario, participants completed measures for blame, competence, the novelty alternative explanation, and likelihood to return to the company.

Measures.

The attribution measure was the same as in experiment 3. The competence measure was the same as in experiment 1. To measure the novelty of the service participants rated their agreement with "While I was on the website, I encountered things that I consider _____" for the following adjectives: unusual, unexpected, surprising. These descriptors were chosen based on the definition of novelty used in the literature (Mehrabian and Russell 1974).

Results

Sample size and respondent screening.

My target final sample was 200 respondents, after screenouts. To that end, I collected 203 responses. The only screening criterion was the true/false attention check introduced in experiment 1. Three participants were screened for failing that check prior to the end of data fielding, leaving a final sample of 200.

Blame.

T-tests were used for all variable comparisons between conditions in this experiment, unless otherwise specified. The two statements measuring company blame were highly correlated (r=0.85), so, as in experiment 3, they were combined into an index. Replicating results from experiments 1 and 2, participants who chose themselves attributed the bad experience to the

company less when they had the opportunity to delegate ($M_{No\ Service}$ = 5.13, $M_{Did\ Not\ Delegate}$ = 3.52, t(186.81) = -5.53, p<.0001).[7]

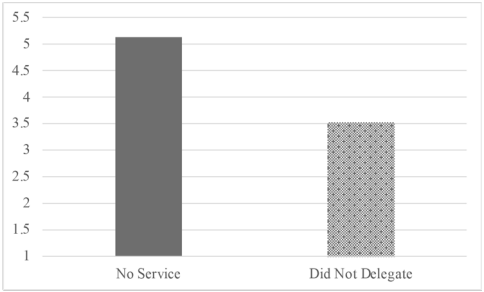

Figure 2.5: Perceived blame in Experiment 4

Competence.

The items in the competence measure loaded onto one factor (λ_1=4.91, λ_2=0.41) with a high Cronbach's alpha (α=0.95), and were combined into an index. Consistent with hypothesis 1, participants who booked the hotel and flight themselves thought the company was more competent when they had the opportunity to delegate ($M_{No\ Service}$ = 2.64, $M_{Did\ Not\ Delegate}$ = 3.78, t(181.29) = 6.18, p<.0001).

To test whether perceived competence mediates changes in company blame, I utilized a bootstrapping mediation model (PROCESS Model 4; Preacher and Hayes 2004), with the manipulation of opportunity to delegate as the IV, perceived competence as the mediator, and company attributions as the DV. Consistent with hypothesis 3, the mediation analysis yielded a significant indirect effect (Indirect Effect = -0.37, 95% CI = [-0.55, -0.22]), though also a significant direct effect (Direct Effect = -0.43, 95% CI = [-0.71, -0.14]).[8]

Novelty.

The items in the novelty measure loaded onto one factor (λ_1=2.52, λ_2=0.31) with a high Cronbach's alpha (α=0.90), and were combined into an index. Did Not Delegate participants,

who had access to a delegation service, said they encountered more novel things on the website than No Service participants did ($M_{No\ Service}$ = 3.35, $M_{Did\ Not\ Delegate}$ = 4.21, t(198) = 4.10, p <.0001). A bootstrapping mediation model, with novelty as the mediator, failed to find mediation (Indirect Effect = 0.05, 95% CI = [-0.04, 0.17]; Direct Effect = -0.86, 95% CI = [-1.16, -0.56]).[9]

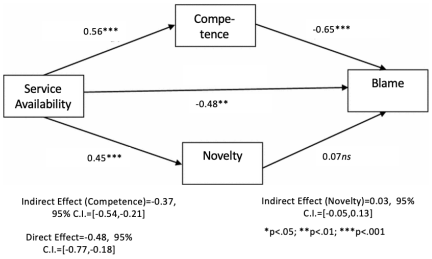

Figure 2.6: Mediation results in Experiment 4

Likelihood to return to the company.

Did Not Delegate participants said they would be more likely to revisit the website than No Service participants did ($M_{No\ Service}$ = 1.94, $M_{Did\ Not\ Delegate}$ = 3.69, t(174.42) = 7.23, p<.0001), as predicted. A bootstrapping analysis found mediation through company blame (Indirect Effect = 0.26, 95% CI = [0.14, 0.39]; Direct Effect = 0.62, 95% CI = [0.38, 0.86]).[10]

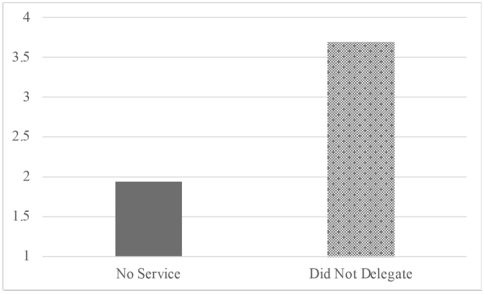

Figure 2.7: Rated likelihood to return to website in Experiment 4

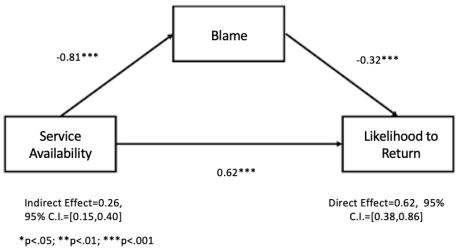

Figure 2.8: Mediation of likelihood to return to website in Experiment 4

Discussion

Experiment 4 demonstrates that giving the opportunity to delegate increases a company's perceived competence (H1a), which mediates (H3) the observed effect that consumers who choose themselves blame companies less when they have the opportunity to delegate (H2a). Results also suggest that these effects are not caused by the novelty of the delegation service: while participants who had the opportunity to delegate said that they encountered more unusual or surprising things on the travel website, it did not decrease blame. In fact, novelty was, directionally, positively associated with blame.

This experiment also established a downstream, managerially relevant consequence of the differences in blame: consumers who choose products and services themselves are more likely to return to the company after negative consumption experiences if they had the opportunity to delegate.

Experiment 5

This final experiment more deeply examines the effect of the differences in the company's perceived competence on attributions of blame. Using the existence of a service,

without any prior interactions, to infer competence is an indirect heuristic. The presence of a more diagnostic signal of competence should then alter blame. In this experiment I manipulate information about the service's previous performance in choosing vacations the company's customers like. Previous performance is not an absolute indicator of the service's current capabilities (e.g., the service might have been changed recently), but performance has been measured in previous research as an indicator of current competence (Ahmed, Rafiq, and Saad 2003). Therefore, it should be a more diagnostic signal of competence, and therefore shift blame in the scenarios.

Method

Participants and design.

Experiment 5 followed a four (Did Not Delegate—Control, Did Not Delegate—High Performance, Did Not Delegate—Low Performance, No Service) group between-subjects design. Four hundred ten participants (Gender: 59% Female, 39% Male, 2% Non-Binary; Median Age: 33 years) were recruited from Amazon Mechanical Turk, of whom I retained 400 (Gender: 59% Female, 39% Male, 3% Non-Binary; Median Age: 32 years). All exclusion criteria were preregistered. Participants were paid $0.50 each.

Procedure.

The No Service condition scenario was identical to the No Service scenario in experiment 4, and the Did Not Delegate – Control condition scenario was identical to the Did Not Delegate scenario in experiment 4. Participants in the Did Not Delegate – High Performance and Did Not Delegate – Low Performance conditions read a scenario identical to that in the Did Not Delegate – Control condition, with one exception: while participants were told about the service there was an additional message about how well the service had predicted consumer preferences in the

past. Did Not Delegate – Low Performance participants saw this text: "The website adds the following message: 'We are learning how to predict consumers' preferences. In the last year, our advisors chose products that customers liked 48% of the time.'" Did Not Delegate – High Performance participants saw this message: "The website adds the following message: 'We are very good at predicting consumers' preferences. In the last year, our advisors chose products that customers liked 96% of the time.'"

After reading the scenario, participants rated blame and competence.

Measures.

The blame and competence measures were identical to those used in experiment 4.

Results

Sample size and respondent screening.

My target final sample was 400 respondents, after screenouts. To that end, I collected 410 responses. Ten participants were screened for failing the true/false attention check introduced in experiment 1, leaving a final sample of 400.

Competence.

A 1-way ANOVA omnibus test indicated differences among the groups in perceived competence ($F(3,396)=12.31$, $p<.0001$). Planned contrasts compared (1) the No Service and Did Not Delegate – Control conditions, and (2) the Did Not Delegate – High Performance and Did Not Delegate – Low Performance conditions. The former contrast establishes that the scenario causes effects consistent with our hypotheses, while the latter contrast tests whether information about performance changes said effects. Consistent with H_{1a}, participants in the Did Not Delegate – Control condition rated the company as more competent than the No Service participants did ($M_{No\ Service}=2.80$, $M_{Did\ Not\ Delegate\ -\ Control}=3.66$, $F=19.02$, $p<.0001$). Consistent with

expectations, participants in the Did Not Delegate – High Performance condition felt the company was more competent than participants in the Did Not Delegate – Low Performance condition ($M_{Did\ Not\ Delegate\ –\ High\ Performance}$=3.96, $M_{Did\ Not\ Delegate\ –\ Low\ Performance}$=3.40, F=7.99, p=.0049).

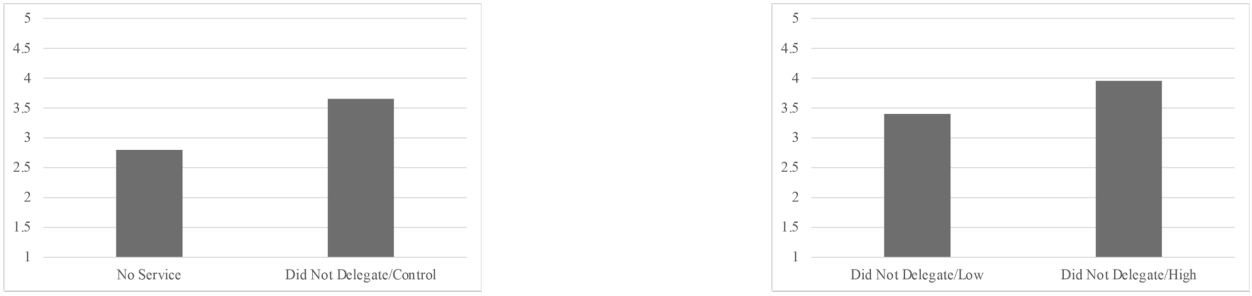

Figure 2.9: Perceived competence in Experiment 4

I utilized two bootstrapping mediation models to test the effect of competence on blame (PROCESS Model 4; Preacher and Hayes 2004). The first model contained the No Service and Did Not Delegate – Control contrast as the IV, perceived competence as the mediator, and company attributions as the DV. Consistent with H_3, the mediation analysis yielded a significant indirect effect (Indirect Effect = -0.32, 95% CI = [-0.49, -0.17]), though also a significant direct effect (Direct Effect = -0.31, 95% CI = [-0.59, -0.04]).

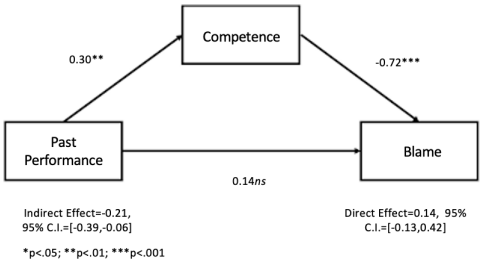

Figure 2.10: Mediation of blame in Experiment 5 - Control conditions

The second model contained the Did Not Delegate – High Performance and Did Not Delegate – Low Performance contrast as the IV, perceived competence as the mediator, and

company attributions as the DV. As expected, the mediation analysis yielded a significant indirect effect (Indirect Effect = -0.20, 95% CI = [-0.38, -0.05]), though there was a countervalenced direct effect (Direct Effect = 0.11, 95% CI = [-0.16, 0.38]).[11]

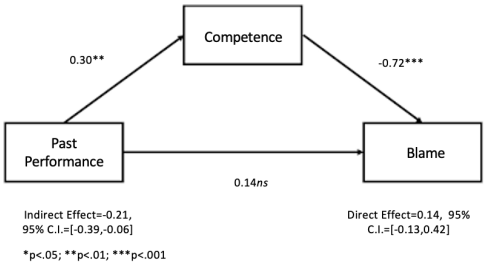

Figure 2.11: Mediation of blame in Experiment 5 - Past Performance manipulation

Blame.

A one-way ANOVA omnibus test indicated differences among the groups in company blame ($F(3,396)=10.28$, $p<.0001$). As with analyses of competence, planned contrasts compared (1) the No Service and Did Not Delegate – Control conditions, and (2) the Did Not Delegate – High Performance and Did Not Delegate – Low Performance conditions. Consistent with H_{2a}, participants in the Did Not Delegate – Control condition blamed the company less for the negative experience than the No Service participants did ($M_{No\ Service}=4.57$, $M_{Did\ Not\ Delegate\ -\ Control}=3.31$, $F=17.44$, $p<.0001$). While participants in the High Performance service condition blamed the company less than participants in the Low Performance service condition, it was not significant ($M_{Did\ Not\ Delegate\ –\ High\ Performance}=3.07$, $M_{Did\ Not\ Delegate\ –\ Low\ Performance}=3.25$, $F=0.34$, $p=.5627$).

Discussion

Experiment 5 replicated results from H_{1a}, H_{2a}, and H_3 found in previous experiments: people who choose products themselves blame companies less if they had the ability to delegate.

This occurred partially because people view companies as more competent if they offer a delegation service. I also strengthened the theory that competence drives changes in blame by manipulating competence through a different signal: the service's performance history. Telling participants that the company has chosen products 96% of its customers have liked made it appear more competent than telling participants customers only liked 48% of the products it chose. I found a significant indirect-only effect on blame through competence (Zhao, Lynch, and Chen 2010). I discuss why, despite mediation, the performance manipulation did not have an aggregate effect on blame in the general discussion.

General Discussion

Previous delegation research (Bartling and Fischbacher 2011; Steffel et al. 2016; Steffel and Williams 2017) has indicated that consumers are motivated to delegate so they can pass blame to the delegatee in case of a bad outcome, suggesting offering to delegate would be unpalatable for companies. My research builds on that research, finding a more complex effect. Understanding the value of offering a service necessitates a comparison between people who have the service available to them, and people who do not have it available. The previous research examining the effects of delegation has only ever studied people who all have the opportunity to delegate. Using this previous research to draw conclusions about delegation service value would assume that people who do not have the opportunity to delegate behave identically to people who do have the opportunity to delegate, but choose not to. I show that—at least in this case—that is not true. I find that people who decide to choose products or services themselves blame companies less for negative consumption experiences if that company gave

them the opportunity to delegate. I have shown that one reason for this is consumers perceive companies who offer delegation services to be more competent. I find a downstream consequence of this relationship: people who choose not to delegate are more likely to return to the company, despite the negative experience, than people who do not have the opportunity to delegate. Finally, I have provided evidence against three alternative explanations: counterfactual thinking, autonomy, and novelty.

Through these results I counter decades of conventional wisdom on service value, positing for the first time to my knowledge that it is possible to have a service that has negative effects for companies when it is used, and still have value. The key is that the service has value through the people who do not use it, not the people who do.

Practical Implications

The most salient managerial contribution this paper makes is through its contribution to the concept of service value. Firms have always recognized that services should be implemented if they have value. However, value cocreation, the predominant service value paradigm, maintains value is cocreated with the customer, and that the customer perceives value on the basis of "value in use" (Vargo and Lusch 2004). Many papers have gone so far as to say if consumers do not use services, there is no value in them (Grönroos 2011; Yu et al. 2019). Even research focusing more broadly on service profit suggests that if using a service ultimately imposes costs on companies, it is not worthwhile (Sasser, Schlesinger, and Haskett 1997). I counter that there may also be services for which people not using the service provide more value than people who do use it, and more value than if there were no service to begin with. Such Disuse Services, admittedly, must also be infrequently used. Delegation services fit this extra criterion because people in general are not very likely to delegate. In all previous research

on delegation for which it was possible to measure incidence, only one in three people chose to delegate. Companies may decrease delegation even further with tactics like implementing service fees.

This research gives managers evidence that adopting delegation services can actually have positive consequences. The only prior research that indicates how delegation can impact companies suggests that people would use delegation to shift blame to the company (Bartling and Fischbacher 2011; Steffel et al. 2016a; Steffel and Williams 2017), which has many negative repercussions (Folkes 1984; Richins and Verhage 1985; Hess Jr., Ganesan, and Klein 2003). This research, in contrast, shows that there are circumstances in which delegation can *decrease* company blame. This decreased blame makes people more likely to return to the companies, which is one key metric used to evaluate service success (Sasser et al. 1997). By highlighting positive benefits, I hope managers can see delegation services are worthwhile, even in counterintuitive ways.

Of course, not all companies should create delegation services and then hope consumers do not use them. Similarly, the benefits companies receive from consumers who do not use a delegation service may not always outweigh the costs from consumers who do use it. This depends on how often people would use the service (which would vary depending on the industry, the target audience, and how companies structure the service), how many of those users would delegate purely to scapegoat (which has not been established), what happens when delegation leads to a successful outcome (which has not been studied), and how often delegation services fail vs. succeed (for which I largely do not have data and which will vary based on the strengths of the service). The fact that previous research says most consumers on average will

not delegate—even in situations where researchers deliberately try to get them to—is at least an encouraging sign.

Limitations

While my experiments suggest robust effects across experimental designs and for many types of products, my findings include limitations that open doors to future research. The primary limitation of this work is that my hypothesized mechanism, perceived competence, only partially mediates the effect of the opportunity to delegate on blame. The full set of mechanisms may be many, and possibly intertwined. I have, however, eliminated three alternative mechanisms that previous literature would claim cause this effect. Future research might illuminate other contributing factors.

In experiment 5, mediational evidence demonstrated that providing a diagnostic signal of the service's competence would change consumers' perceptions of the company's competence, and that this change in competence mediates changes in company blame. However, in aggregate, the effect of my signal on blame was directional. An explanation can be found in the fact that the direct effect in my mediation was a different sign from the indirect effect. According to standard literature (Zhao et al. 2010), these competing effects in aggregate can obscure the effect of the mediator. The pattern of results is not uncommon: in simulation studies, the lack of a total effect is the largest source of Type II errors in mediation (MacKinnon et al. 2002). Guidelines suggest only the mediation is necessary to establish an effect (Zhao et al. 2010). While the direct effect in the mediation was not significant, it may still be the performance manipulation affected another construct. One possibility is that the manipulation altered both perceived competence and warmth. Telling participants "I choose products people like less than half of the time" may be a signal of the company's benevolence: they warn their customers there are risks to using the

service, even if this means they will not use it. In contrast, in the High Performance condition, telling participants "only 4% of people did not like the products I chose" would not communicate risk. Research has shown, at least in some contexts, that perceived benevolence may lessen retaliatory behaviors associated with blame (Barbarossa and Mandler 2021).

Directions for Future Research

Further research would do well to examine other services that may have worth in not being used. What are these services? Are there common characteristics underlying them? Are there ways to easily determine when such services warrant creation?

Since the opportunity to delegate can change company perceptions in a positive way, it would be useful to study in greater depth how those perceptions bleed over into future interactions. If a company is seen as more competent, that may imply that the quality of their goods is higher, and therefore the goods are more likely to meet consumers' needs. Would having the opportunity to delegate, therefore, make it more likely that consumers will buy if they visit in the future? Could the opportunity to delegate also increase satisfaction with any future products consumers buy?

It would also be helpful to expand research on delegation to how delegation changes perceptions of the delegator. If someone finds out a friend delegated the choice of a product, would they perceive the friend as less competent, thinking the friend delegated because they were incapable of making the decision themselves? Or would they perceive the friend as more practical, because by delegating they save time and effort? Would knowing someone has delegated to others in the past make people think that person is more receptive to assistance, and would they therefore be more likely to offer that person advice or give honest opinions?

In this research I only examined how delegation services change perceptions following failure. I did this as a direct response to previous research about how people would use delegation services to divert blame in case of failure. It would be interesting to see how this mechanism works if participants achieve success with the product they chose.

In summary, I believe that this research has successfully built upon previous delegation and attributions literature by establishing that delegation services can alter how much blame people place toward companies after negative consumption experiences, even when consumers do not use them. I showed one mechanism by which this difference in blame occurs: delegation services are a signal of competence. Through this, I counter previous service value wisdom to show that services can have value specifically from the people not using them. I hope that this research not only has created rich avenues for further research. I hope that it inspires business managers to start offering delegation service. Most importantly, I hope that this research would teach researchers and companies to broaden their understanding of how services may provide value.

Appendix A: Experiment 2 Book Excerpt

My boss jogged up to me. “Are you ready?”

“Yes I am!”

“Good. Now, based on severity, spalls can be categorized as minor, intermediate, or major. Appropriate materials and methods differ significantly depending on the blech depth, size, cause, and configuration (horizontal, vertical, overhead, etc.). The definitions I’ll give you are general; depending on the circumstances, one may define spall severity differently than these definitions on a case by case basis. Okay?”

I nodded. “Please continue. This is very helpful.”

“For a minor spall, the blech is less than 1 inch deep and it covers an area less than 12 square inches. However, if the majority (more than 50%) of a reinforcing bar or strand circumference is exposed then the spall would be classified as Intermediate even if the blech is less than 1 inch deep. A deeper spall (2 inches maximum) can be categorized as minor as long as it does not progress beyond the outer layer of reinforcement.”

“I see.”

“For an intermediate spall, the spall exposes a majority (more than 50%) of the outer cage of reinforcing bar or strand circumference, or the blech is greater than 2 inches deep. The maximum depth of an intermediate spall is—oh wait.” He checked the time. “I forgot I had a meeting. We’ll continue this in an hour, okay?” He ran off.

It took me a few moments to finish writing my notes. I smiled. A pleasant day during my apprenticeship at the construction site.

Endnotes

1. Findings are robust to the inclusion of screened participants
$M_{No\ Service}$=6.54, $M_{Did\ Not\ Delegate}$=5.42, t(210)=-2.71, p=.0072
$M_{No\ Service}$=6.54, $M_{Delegated}$=7.89, t(173.11)=4.35, p<.0001

2. Findings are robust to the inclusion of screened participants
F(3,300)=27.93, p<.0001
$M_{No\ Service}$=3.01, $M_{Did\ Not\ Delegate}$=2.24, F=6.99, p=.0086
$M_{No\ Service}$=3.01, $M_{Delegated}$=4.77, F=32.28, p<.0001

3. Findings are robust to the inclusion of screened participants
Upward Counterfactuals: No Service: 81%, Did Not Delegate: 80%, $\chi^2(1)$=0.03, p=.8734
Downward Counterfactuals: No Service: 26%, Did Not Delegate: 25%, $\chi^2(1)$=0.02, p=.8823

4. Findings are robust to the inclusion of screened participants
Upward Counterfactuals: No Service: 81%, Delegated: 90%, $\chi^2(1)$=3.03, p=.0819
Downward Counterfactuals: No Service: 26%, Delegated: 24%, $\chi^2(1)$=0.10, p=.7507
β=-0.16, t=-0.34, p=.7363

5. Findings are robust to the inclusion of screened participants

	Regret	Relief	Guilt	Shame	Disappoint-ment
$M_{No\ Service}$	5.20	1.83	4.64	4.43	5.20
$M_{Did\ Not\ Delegate}$	4.89	1.64	4.19	4.17	5.23
$M_{Delegated}$	5.94	1.36	5.37	5.00	5.95
Omnibus Test	F(3,300) =6.93, p=.0002	F(3,300) =3.59, p=.0141	F(3,300) =7.99, p<.0001	F(3,300) =3.20, p=.0238	F(3,300) =4.65, p=.0034
Contrast: No Service vs. Did Not Delegate	F=1.44, p=.2307	F=1.77, p=.1838	F=2.64, p=.1050	F=0.86, p=.3558	F=0.02, p=.8762
Contrast: No Service vs. Delegated	F=7.81, p=.0055	F=9.02, p=.0029	F=5.77, p=.0169	F=3.61, p=.0585	F=7.39, p=.0070
Mediation: No Service vs. Delegated (Indirect Effect)	-0.04 95% CI = [-0.12, 0.03]	-0.03 95% CI = [-0.11, 0.03]	-0.02 95% CI = [-0.10, 0.05]	0.02 95% CI = [-0.03, 0.08]	-0.03 95% CI = [-0.10, 0.03]
Mediation: No Service vs.	0.89 95% CI = [0.57, 1.21]	0.89 95% CI = [0.57, 1.21]	0.87 95% CI = [0.55, 1.19]	0.84 95% CI = [0.52, 1.15]	0.88 95% CI = [0.56, 1.20]

Delegated (Direct Effect)					

6. Findings are robust to the inclusion of screened participants
λ_1=3.21, λ_2=0.61, α=0.86
$M_{No\ Service}$ = 3.61, $M_{Did\ Not\ Delegate}$ = 3.68, $M_{Delegated}$ = 3.58, $F(3, 300)$=0.80, p=.4958

7. Findings are robust to the inclusion of screened participants
r=0.85
$M_{No\ Service}$ = 5.15, $M_{Did\ Not\ Delegate}$ = 3.53, $t(188.77)$ = -5.65, p<.0001

8. Findings are robust to the inclusion of screened participants
λ_1=4.90, λ_2=0.41
α=0.95
$M_{No\ Service}$ = 2.64, $M_{Did\ Not\ Delegate}$ = 3.77, $t(182.59)$ = 6.20, p<.0001
Indirect Effect = -0.37, 95% CI = [-0.54, -0.21]
Direct Effect = -0.44, 95% CI = [-0.72, -0.16]

9. Findings are robust to the inclusion of screened participants
λ_1=2.52, λ_2=0.31
α=0.90
$M_{No\ Service}$ = 3.33, $M_{Did\ Not\ Delegate}$ = 4.23, $t(201)$ = 4.26, p <.0001
Indirect Effect = 0.05, 95% CI = [-0.04, 0.17]
Direct Effect = -0.87, 95% CI = [-1.16, -0.57]

10. Findings are robust to the inclusion of screened participants
$M_{No\ Service}$ = 1.92, $M_{Did\ Not\ Delegate}$ = 3.69, $t(175.88)$ = 7.36, p<.0001
Indirect Effect = 0.26, 95% CI = [0.15, 0.40]
Direct Effect = 0.62, 95% CI = [0.38, 0.86]

11. Findings are robust to the inclusion of screened participants
$F(3,406)$=13.07, p<.0001
$M_{No\ Service}$=2.81, $M_{Did\ Not\ Delegate\ -\ Control}$=3.65, F=18.72, p<.0001
$M_{Did\ Not\ Delegate\ –\ High\ Performance}$ =3.99, $M_{Did\ Not\ Delegate\ –\ Low\ Performance}$=3.39, F=9.37, p=.0023
Mediation: No Service vs. Did Not Delegate – Control
Indirect Effect = -0.31, 95% CI = [-0.48, -0.16]
Direct Effect = -0.31, 95% CI = [-0.58, -0.03]
Mediation: Did Not Delegate – High Performance vs. Did Not Delegate – Low Performance
Indirect Effect = -0.21, 95% CI = [-0.39, -0.07]
Direct Effect = 0.14, 95% CI = [-0.13, 0.42]

12. Findings are robust to the inclusion of screened participants
$F(3,406)$=9.70, p<.0001
$M_{No\ Service}$=4.59, $M_{Service\ -\ Control}$=3.36, F=16.60, p<.0001
$M_{Did\ Not\ Delegate\ –\ High\ Performance}$ =3.16, $M_{Did\ Not\ Delegate\ –\ Low\ Performance}$=3.30, F=0.21, p=.6460

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