CHRISTILENE DU PLESSIS

Influencers

The Role of Social Influence in Marketing

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De rol van sociale invloed in marketing

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TABLE OF CONTENTS

CHAPTER 1

INTRODUCTION

Social influence is the corner stone of consumer psychology. In fact, in the last decade of the 19th century the study of consumer psychology emerged from an interest in advertising and its influence on people (Schumann, Haugtvedt, and Davidson 2008). Broadly speaking, the phenomena of social influence involves a dyadic relation between two agents (e.g., a company and a consumer, a salesperson and a consumer, or a consumer and another consumer) in which one party tries to persuade or influence the other. Traditionally research on social influence has focused on understanding how people respond to influence attempts (e.g., Milgram 1963; Asch 1955; Freedman and Fraser 1966; Petty and Cacioppo 1986; Chen and Chaiken 1999; Fazio and Schwen 1999) and how social influence emerges (e.g., French and Raven 1959). In this dissertation, I challenge common methodological conventions used to study social influence and, in so doing, make new theoretical contributions.

Past research has largely focused on how one agent (e.g., a company or a review writer) influences another (e.g., a review writer or reader) within a single dyadic relationship. In chapter 2 and 3, my co-authors and I instead focus on the dynamic nature of social influence through consideration of how one dyadic relationship between two agents (e.g., a company and the writer it incentivizes) may influence another dyadic relation (e.g., a consumer reading the review written by the writer).

Specifically, in chapter 2, we investigate how monetary incentives affect the generation of reviews by writers and the reception of reviews by readers by altering writers' self-perceived legitimacy and uncertainty, which subsequently decreases product evaluations by readers. Specifically, we suggest that writers infer their legitimacy as reviewers from the size of the reward offered: a small incentive lowers their sense of legitimacy compared to a large incentive, or a no monetary incentive condition in which consumers derive legitimacy from their product experience (H1). Building on conceptual work linking lack of legitimacy with uncertainty, we hypothesize that small (vs. large or no) monetary incentives increase writers' uncertainty (H2) and that the shift in writers' self-perceived legitimacy mediates the effect of monetary incentives on writers' uncertainty (H3). We further propose that reviews written for small (vs. large or no) monetary incentives yield lower product evaluations among readers blind to the incentive condition (H4), because readers infer increased uncertainty in these reviews, doubt product quality, and hence *lower* their product evaluations (H5). Three studies involving real incentives and actual products experiences (headphones; online game; yogurt) support these hypotheses.

Due to the proliferation in incentivized reviews, many firms, as well as government regulators and consumer protection agencies, have established policies requiring disclosure of incentives given to review writers (e.g., Australian Competition and Consumer Commission 2016; U.S. Federal Trade Commission 2009). In chapter 3, my co-authors and I briefly consider how such disclosure statements affect review persuasiveness. Disclosure likely induces uncertainty about writer trustworthiness, leading readers to discount writers' opinions when forming expectations about product quality. However, we show this is not always the case. Instead, the extent to which disclosing incentives affects review persuasiveness depends on whether readers deem their disclosure-induced uncertainty to be *integral* or *incidental* to judgment formation. This occurs through a metacognitive process in which readers elaborate on the relevance of their uncertainty. Using a field study and two experiments, we show that disclosure-induced uncertainty about reviewer trustworthiness deemed integral to judgment formation, leads to lower product evaluations based on the incentivized review. However, when uncertainty is judged incidental to judgment formation, product evaluations are unaffected by incentive disclosure.

In chapter 4, my co-authors and I challenge another prevalent methodological convention: the use of 2-cell designs instead of 3-cell designs in the study of social power, an important source of social influence. A pervasive assumption in the social power literature is that powerfulness is the driving causal force behind power's far-reaching effects. In chapter 4, we show that this preoccupation with the powerful has led to the proliferation of experimental designs that contrast high power to either low power *or* a control condition. We review evidence suggesting that such designs pose both theoretical and methodological challenges. Across a content analysis, an experiment, and a large-scale meta-analysis, we show that effects of studies comparing only high and low power tend to be attributed to powerfulness, and that comparing high power to a control condition in the absence of low power weakens construct validity and inflates the high-power effect. This quantitative review demonstrates how a prevailing methodological tradition in the study of social power limits our understanding of powerlessness, powerfulness and the social influence more generally. We conclude this chapter with a discussion of the implications of our findings for theory and experimental design in social power and the relevance of our findings to related fields.

DECLARATION OF CONTRIBUTION

The research presented in chapter 2 was with David Dubois and is currently under revision. The research presented in chapter 3 was with Andrew T. Stephen, Yakov Bart and Dilney Goncalves and is under revision. The research presented in chapter 4 was with Michael Schaerer, Stefan Thau and Andy Yap, is currently under review at Organizational Behavior and Human Decision Processes, won the Best Student Paper Award (2016) at the International Association for Conflict Management Conference and the Best Graduate Student Poster Award (2016) at the Society for Personality and Social Psychology Conference.

Chapter 1. I wrote this chapter and implemented feedback from my supervisor (Steven Sweldens).

Chapter 2. I formulated the research question in cooperation with my co-author (David Dubois), performed the literature review, designed the studies, collected and analyzed the data, and wrote the manuscript. My co-author provided feedback at each stage of the process.

Chapter 3. I formulated the research question, performed the literature review, designed the studies, collected and analyzed the data, and wrote the manuscript in cooperation with my co-authors (Andrew Stephen, Yakov Bart, and Dilney Goncalves). My supervisor also provided feedback on this chapter.

Chapter 4. I formulated the research question, performed the literature review, designed the studies, collected and analyzed the data, and wrote the manuscript in cooperation with my co-author (Michael Schaerer). We received feedback on the manuscript from the other two co-authors (Stefan Thau and Andy Yap), my supervisor, and friendly reviewers who are experts in the area of social power research (e.g., Adam Galinsky and Roderick Swaab).

Chapter 5. I wrote this chapter and implemented my supervisor’s feedback.

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CHAPTER 2

PAYING PEANUTS LIMITS LEGITIMACY: WHEN AND WHY MONETARY INCENTIVES AFFECT REVIEW GENERATION AND RECEPTION

Electronic word-of-mouth (eWOM), notably online reviews, increasingly sway consumer opinions. More than 26,000 reviews are posted every minute on Yelp alone (Vendasta 2016). Online reviews are important to business executives and managers as they guide consumers' choice of restaurants (The Guardian 2012), hotels (PwC 2015), and even e-commerce websites (Econsultancy 2015). In fact, 92% of consumers trust product reviews (Nielsen 2013) and 88% of consumers rely on online reviews to judge the quality of a local business (BrightLocal 2016). Further, academic research shows that online reviews are a significant predictor of sales (Chevalier and Mayzlin 2006).

To stimulate the production of online reviews, companies increasingly offer incentives to potential reviewers. An analysis of 7 million reviews on Amazon.com in 2016 revealed that while incentivized reviews accounted for only 2% of reviews in 2012, they now represent more than half of the reviews on this platform (ReviewMeta 2016). Beyond Amazon.com, an entire platform ecosystem has emerged with the express purpose of providing incentives to consumers in exchange for posting reviews online (e.g., PayPerPost, ReviewStream, and SponsoredReviews), often in the form of small monetary rewards. For instance, a pilot study (n = 216) conducted on Amazon Mechanical Turk, a platform frequently used by companies to access "… a diverse, on-demand, scalable workforce…" (Amazon Mechanical Turk 2016), found that 56.9% of participants had reviewed a product on the platform at least once before. Of these, 69.9% were paid less than $1 to write a review.

Paying consumers to review products is not only common but considered essential for obtaining reviews that are likely to win over potential customers (Bloomberg 2016), perhaps because incentivized reviews tend to be longer (Burtch et al. 2016) and more positive (ReviewMeta 2016). Yet the evidence accumulated also suggests that neither review length nor positivity determine how readers respond to reviews or whether they subsequently change their product evaluations. In fact, Burtch and colleagues (2016) found no effect of length on reader perceptions of the helpfulness of the review, and Rucker, Petty, and Briñol (2008) showed that balanced reviews (i.e., those containing both positive and negative information) are more convincing than one-sided reviews.

The current paper aims to contribute to past efforts to understand how monetary incentives affect review generation and reception by investigating the psychological effect of monetary incentives on writers' self-perceived legitimacy and uncertainty. Our reasoning originates from the idea that monetary incentives can represent a social signal of the value ascribed to a writer. Thus, writers may infer their legitimacy – the perception that their actions are consistent and appropriate within a social role (for related definitions, see Suchman 1995 and Tyler 2006) – from the size of the monetary incentive provided. In essence, small (vs. large or no) monetary incentives reduce a writer's sense of legitimacy.

Building on conceptual work linking a lack of legitimacy and greater uncertainty (Rucker et al. 2014), we propose that lower self-perceived legitimacy among writers increases their inclination to question the validity of their attitude and, consequently, triggers greater attitude uncertainty – the subjective sense of conviction one has about one's attitude (Petty et al 2007; Rucker et al. 2014). We further propose that readers infer uncertainty from the content of the review, which leads them to doubt product quality and hence lower their evaluation of the product. By identifying how people interpret and internalize monetary incentives, we provide both a parsimonious account of when and why monetary incentives can influence review generation and reception, and identify practical recommendations to marketing executives regarding the use of monetary incentives in online marketing campaigns. That is, we show that paying consumers "peanuts" to write reviews may have harmful consequences for (1) the generation of reviews by writers, and (2) the reception of these reviews by readers.

The first part of this paper focuses on the effect of monetary incentives on writers' self-perceived legitimacy and uncertainty. A lab study (study 1) and a field study (study 2) using real products and real monetary incentives test the proposed effect and psychological process through mediation as well as a moderation-of-process procedure. The second part of the paper focuses on how writers' uncertainty influences readers' doubt in the quality of the reviewed product and subsequent product evaluations. A final lab study (study 3) tests the effect of monetary incentives on readers' reception of reviews and on their product evaluations.

THE IMPACT OF MONETARY INCENTIVES ON WRITER UNCERTAINTY

Monetary Incentives as a Social Signal of Writer Legitimacy

For thousands of years, people have given value to objects – from cowry shells, to coins, notes, and more recently computer code as a medium of exchange (Davies 2010). Because of its ability to signal value, money is used to motivate and direct the behavior of individuals (Gerhart and Fang 2015). For example, employees are paid bonuses to perform better (Warner and Tosi 1995), sales representatives receive premiums to sell more (Walker, Churchill, and Ford 1977), and consumers are incentivized to promote a brand or a product (Anghelcev 2015; Burtch et al. 2016; Du Plessis et al. 2016). A large body of work suggests that monetary incentives have a profound influence on the effort individuals exert when performing a task. For example, meta-analytic reviews of the literature show that financial rewards tend to increase productivity (Camerer and Hogarth 1999; Jenkins et al. 1998).

Studies of the way monetary incentives influence review writing have found that financial incentives increase the length of product reviews (Burtch et al. 2016). Yet the fact that providing incentives to writers leads to longer reviews does not necessarily imply that these are more *persuasive*. In fact, Burtch and colleagues (2016) found no link between review length and perceived review helpfulness, indicating that the additional effort writers invest due to monetary incentives may not render their reviews more influential.

The current work proposes that monetary incentives act as a signal of the appropriateness of an individual's actions when money is exchanged for a service (Fiske 1992). Supporting this argument, neuroscience research suggests that money is represented in the same areas of the brain as other signals of the value placed on an individual by society (see Saxe and Haushofer 2008 for a review). Studies (Izuma, Saito, and Sadato 2008; Zink et al 2008) have found that activity in specific regions of the brain, notably the striatum, reflects reward signals regardless of whether it derives from the economic (e.g., money) or social (e.g., praise and status) domains. For example, when Izuma and colleagues (2008) directly compared participants' neural responses to receiving money versus praise using fMRI, they found a substantial overlap between the neural representations of monetary and social rewards. The left putamen and caudate nucleus showed greater activity in response to both higher monetary payoffs and more positive social evaluations of the self (e.g., being described as "sincere"). Zink and colleagues (2008) showed a similar overlap of monetary and social rewards (e.g., status conferral by others).

The idea that money signals social value is important because it implies that monetary incentives may affect writers' perceptions of themselves. Specifically, we propose that the size of a monetary incentive influences how legitimate a writer perceives him/herself to be in his/her role as a reviewer, where legitimacy refers to the perception that an individual's actions are appropriate to a social role (for related definitions see Suchman 1995; Tyler 2006). An individual assumes multiples social roles in everyday life, be it spouse, parent, doctor or coach (Biddle 1979), and in each respective role they scan their environment for information to determine how well they are performing. In other words, people look for social cues to infer their legitimacy in each role (Eagly, Wood, and Diekman 2000). For example, an individual may look at the number of people cured to infer her legitimacy as a doctor, and at signals of gratitude from a partner to infer her legitimacy as a spouse. Within organizations, employees look for social cues from their superiors to infer the legitimacy of their ideas before they share them (Morrison 2011).

Following this logic, a consumer writing a product review may look for cues to determine how good a reviewer she is, and to what extent her judgment is valued. Such cues include the size of the monetary incentive a company is willing to pay her to write a review. If money sends a social signal of how much a writer's judgment is valued, this consumer may infer her legitimacy as reviewer from the size of the incentive offered. In the absence of such an incentive she is likely to use first-hand experience with the product as evidence that she is a legitimate reviewer (i.e., she is a legitimate reviewer because she has tried the product). As such, a small (compared to a large or no) monetary incentive may signal that the consumer is a less valued writer, in turn lowering her self-perceived legitimacy. More formally:

> **H1:** Writers paid a small (vs. large or no) monetary incentive will perceive themselves as less legitimate reviewers.

Writer Self-Perceived Legitimacy and Attitude Uncertainty

We further propose that the extent to which a writer sees herself as a legitimate reviewer will influence the certainty of her attitude towards the product reviewed. Past research has shown that attitudes and the (un)certainty with which they are held are different psychological constructs, with distinct antecedents and consequences (Tormala and Rucker 2007). An attitude is a primary cognition (e.g., I like this product), whereas attitude (un)certainty represents a secondary or meta-cognition reflecting one's subjective sense of conviction or lack thereof about an attitude (e.g., I'm certain/uncertain that I like this product; Gross, Holtz, and Miller 1995; Petty et al. 2007; Rucker et al. 2014;). Attitudes and the (un)certainty with which they are held are psychologically distinct, hence two writers can have the same attitude about a product (i.e., they both like it) but one can be more uncertain of her attitude than the other (Tormala and Rucker 2007). Importantly, past work suggests that a writer who becomes more uncertain of her attitude will also be less persuasive (Price and Stone 2004; Sniezek and Van Swol 2001; Yates et al. 1996). For example, Price and Stone (2004) showed that financial advisors who conveyed their attitude with less certainty were assumed to make fewer correct judgments and were seen as less knowledgeable. Thus, two writers with the same attitude may differ in how persuasive they are simply because they differ in how uncertain they are of their attitude.

Attitude uncertainty stems from a range of information dimensions such as its accuracy (e.g., Festinger 1954; Petrocelli, Tormala, and Rucker 2007; Visser and Mirabile 2004), completeness (e.g., Priester, Petty, and Park 2007), or relevance (Snyder and Kendzierski 1982). One dimension that has received little empirical attention is the perceived legitimacy of the source or information upon which an attitude is based (Rucker et al. 2014). The current work builds on this conceptual link and suggests that lacking self-perceived legitimacy in a role associated with the expression of an attitude (e.g., expressing an attitude about a product as a product reviewer) increases attitude uncertainty. Hence a writer inferring lower legitimacy from social cues (e.g., a small monetary incentive) may be more uncertain of her attitude compared to one who infers higher legitimacy from social cues (e.g., a large monetary incentive) or from her own experience (no monetary incentive).

In support of this proposition, past research has shown that social cues can help people appraise their uncertainty. For example, Petrocelli and colleagues (2007) found that undergraduates asked to report their own attitudes on a topic (i.e., a new university policy) were more certain of their attitude when they were also told that a majority (vs. minority) of students agreed with them (see also Campbell and Kirmani 2008; Tormala, DeSensi, and Petty 2007). Thus, we propose that the extent to which a writer sees herself as legitimate will influence her uncertainty about the product. Specifically, we predict that a writer paid a small (vs. large or no) incentive will be more uncertain of her attitude because she will view herself as lacking legitimacy as a reviewer. Stated formally:

H2: Writers paid a small monetary incentive will be more uncertain of their attitudes than those receiving a large incentive or no incentive.

H3: The effect of receiving a small monetary incentive on writer uncertainty will be mediated by the writer's perception of her own legitimacy.

OVERVIEW OF STUDIES

The current work proposes that monetary incentives can influence the generation of online product reviews and affect subsequent product evaluations by readers. Our reasoning builds on the idea that monetary incentives are a social signal of the value ascribed to a writer. Specifically, we hypothesize that writers infer their legitimacy as reviewers from the size of the incentive provided: small (vs. large or no) incentives will reduce writers' perceptions of their own legitimacy as reviewers. In turn, we propose that differences in legitimacy will alter writers' uncertainty, with lower legitimacy yielding greater uncertainty in their attitude toward the product.

One laboratory study (study 1) and one field study (study 2) focus on writers and test whether and how monetary incentives affect writers' legitimacy and subsequent uncertainty in their attitude about the product. After study 2, we develop two additional hypotheses (H4-5) tied to the downstream consequences of monetary incentives for review reception. A third laboratory study (study 3), including both writers *and* readers, examines the effect of monetary incentives on the extent to which readers infer that writers are uncertain of their attitudes and consequently lower their product evaluations.

Because our perspective rests on a metacognitive account rather than a motivational one, we demonstrate that our effect occurs independent of variations in writers' effort (Burtch et al. 2016; Clark and Mills 1993; Fehr and Falk 2002; Heyman and Ariely 2007; Rabin 1993). To do so we include review length – a proxy measure for effort used in the study of online reviews (Burtch et al. 2016) as a covariate in all our studies.

Across all studies, participants were paid real monetary incentives, tried the products they were asked to review (headphones in study 1, an online game in study 2, and yogurt in study 3), and wrote actual product reviews (in the case of writers) or read and evaluated reviews by incentivized writers (in the case of readers).

In all three studies, we used 10c as a small incentive and $10 (or €10) as a large incentive for the following reasons. First, an initial review of publicly available industry research revealed that the range of incentives companies pay consumers goes from as little as 5c per review (The Penny Hoarder 2016) to as much as $50 (Vindale Research 2016). Second, we conducted a pilot study on Amazon Mechanical Turk – a platform that companies frequently use to generate incentivized reviews (hereafter, MTurk), in which we asked participants (n = 220) to imagine that a company had recruited them to write a product review in exchange for 10c [$10]. Next, participants indicated their perception of the incentive from 1 = "very small" to 7 = "very large". Results revealed that participants viewed 10c as significantly smaller than the scale midpoint (M = 2.05, SD = 1.07, t = -19.00, p < .001) and $10 as significantly larger than the scale midpoint (M = 5.28, SD = 1.22, t = 11.02, p < .001). Third, the same pilot study revealed that 26.2% of our participants reported having been paid 10c or less to write a product review on MTurk in the past, while 13.6% of them had been paid $10 or more. Overall, these results support the use of 10c and $10 (or €10) as common values offered in the marketplace to operationalize small and large incentives from both a theoretical and managerial standpoint.

Due to the expensive nature of the studies conducted, we took several steps to increase statistical power. Specifically, we decided a priori to consistently exclude participants who (1) failed to follow instructions (Oppenheimer, Meyvis, and Davidenko 2009), (2) had used or were exposed to the product they were asked to review before participating in the study since they would thus be more certain of their attitude irrespective of the incentive offered (Fazio and Zanna 1978; Rucker et al. 2014) and (3) whose score on our key dependent variable (uncertainty) fell more than 3 standardized residuals from the mean (Cook 1977; Stevens 1984). Finally, the significant costs associated with data collection also led us to focus on experimental designs that strictly tested our set of hypotheses (e.g., an affirmation condition only in the small incentive condition; study 2).

STUDY 1

Study 1 aimed to find initial evidence for the hypothesis that providing a small (vs. a large or no) monetary incentive increases writer uncertainty. In addition, we collected process evidence testing the proposed mediating role of self-perceived legitimacy. Specifically, given that legitimacy appraisals are foundational to attitude uncertainty (Rucker et al., 2014), we expected that the extent to which a writer perceived herself to be a legitimate reviewer would mediate the effect of monetary incentives on uncertainty.

Design and Procedure

Two-hundred and ninety-seven students (39.06% female; M_{age} = 20.46) from a Dutch university took part in a study presented as a product trial experience during which they tried a pair of headphones. Participants were told they would write a review of the headphones as if they were to write and share an actual review online. After reading a brief description of the headphones, participants used the headphones while watching two short videos that were 5 minutes and 26 seconds long (Figure 1). They then proceeded to write their review.

Key to the study, participants were randomly assigned to receive a small (10 cents), large (€10) or no monetary incentive, in addition to the course credit they expected to receive. We selected these amounts based on the monetary reward that participants from a paid pool associated with the same lab typically get when completing an equivalent study. Specifically, given that participants from the paid pool would be paid €5 for an equivalent study, we reasoned that a 10c payment would be perceived as a small incentive and a €10 payment would be perceived as a large one, compared to a baseline (see also page 15).

In the no incentive condition, no payment was mentioned. Participants merely received the course credit they expected in the form of an email as well as a blank envelope containing a receipt of their participation at the end of the study. In both the small and large incentive conditions participants were (1) reminded they would receive a payment in exchange for generating a review prior to using the headphones as well as prior to writing the review (see Appendix A), and (2) received a physical payment (10 cents / €10 respectively) in a blank envelope at the end of the experiment. As in the no incentive condition, participants also received the expected course credit after completing the study in the form of an email. In addition, they received the monetary incentive they were promised and a participation receipt in a blank envelope at the end of the experimental session.

Figure 1: Product trial, study 1

Next, we measured participants' uncertainty using four scale items adapted from Fazio and Zanna (1978; see also Tormala and Petty 2002; α = .70; e.g., "I was uncertain of my attitude of the headphones" anchored at *strongly disagree* (1) to *strongly agree* (7); see Appendix A). We then measured participants' self-perceived legitimacy on nine scale items adapted from past work (Leach and Spears 2008; Rousseau and Tijoriwala 1999; α = .84; e.g., "I feel that my review is legitimate" anchored at *strongly disagree* (1) to *strongly agree* (7); see Appendix A). Participants then provided demographic information, were debriefed and paid.

Based on our a priori exclusion criteria, we excluded participants who failed to follow the instructions (n = 59) or reported having already used the same headphones before the study (n = 11), as well as those whose score on our key dependent variable (uncertainty) fell more than 3 standardized residuals from the mean (n = 2), resulting in a final sample of 226 participants (40.71% female, M_{age} = 20.5 years). Of note, the study took place at the very end of an experimental session consisting of multiple unrelated experiments, which may have increased fatigue and participants' likelihood to fail the instruction manipulation check (Oppenheimer et al. 2009).

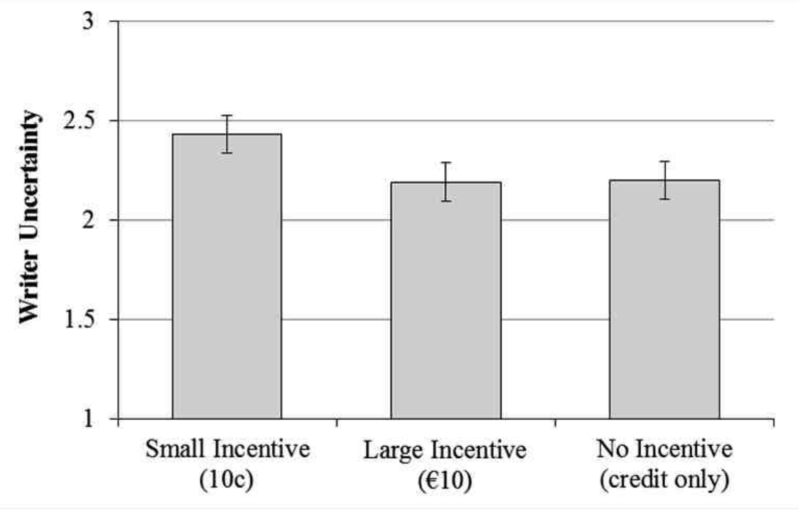
Results

Incentive conditions and writer uncertainty. To test the hypothesis that receiving a small (vs. large or no) incentive would increase writers' uncertainty, we ran an ANCOVA including review length (i.e., the number of words in the review) as a covariate to account for potential variations in effort (Burtch et al. 2016). This analysis revealed that participants felt *more* uncertain of the attitude expressed in their review in the small incentive condition (M = 2.43, SD = .85) than in the large incentive (M = 2.19, SD = .88) and no incentive (M = 2.20, SD = .77, $F(1, 222) = 4.25$, $p < .05$; Figure 2) conditions. Uncertainty did not differ between the large and no incentive conditions ($F(1, 222) = .00$, $p = .95$; see Appendix D for all planned contrast results).

Incentive conditions and writer legitimacy. Next, we tested whether varying monetary incentives affected self-perceived legitimacy. As hypothesized, participants perceived themselves as less legitimate in the small incentive (M = 3.52, SD = 1.14) than in the large incentive (M = 4.46, SD = 1.05) and no incentive conditions (M = 4.38, SD =

.77, $F(1, 222) = 46.27$, $p < .001$; Figure 3). There was no difference in self-perceived legitimacy between participants in the large and the no incentive conditions ($F(1, 222) = .01$, $p = .90$, see Appendix D for all planned contrast results).

Figure 2: Effect of incentive condition on uncertainty, study 1



Mediation by self-perceived legitimacy. We conducted a test of mediation to assess the extent to which the effect of small incentives on writer uncertainty stemmed from perceptions that they lacked legitimacy as reviewers compared to writers in the large or no incentive conditions. To do this, we dummy coded the independent variable (1 = small incentive, 0 = large incentive, 0 = no incentive) and used bias-corrected bootstrapping to generate a 95% confidence interval around the indirect effect of self-perceived legitimacy, where mediation occurs if the confidence interval excludes zero (Hayes 2013). The analysis (5,000 bootstrap samples; bias-corrected confidence intervals estimated and reported) revealed a significant indirect effect ($ab = .16$, $SE = .06$; 95% LLCI = .05, 95% ULCI = .30; see Appendix D for mediation results when the small incentive condition is compared to the large and no incentive conditions separately). As predicted, receiving a small incentive, compared to a large or no monetary incentive decreased writers' self-perceived legitimacy, which subsequently increased writer uncertainty (Figure 4).

Figure 3: Effect of incentive condition on writer legitimacy, study 1

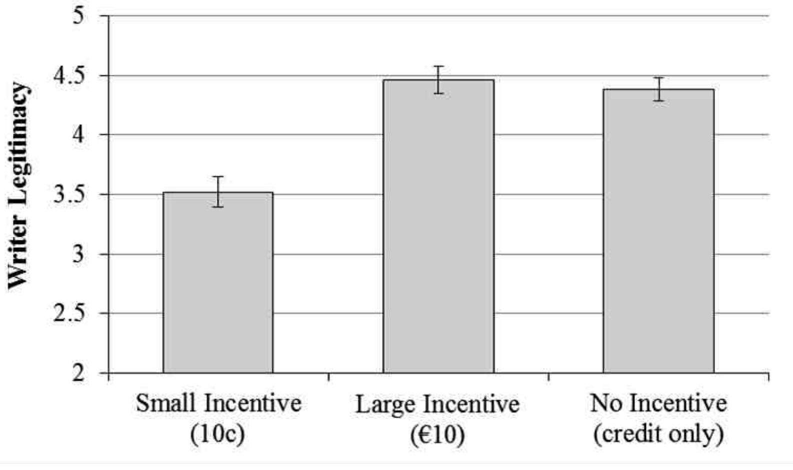


Figure 4: Mediation results, study 1

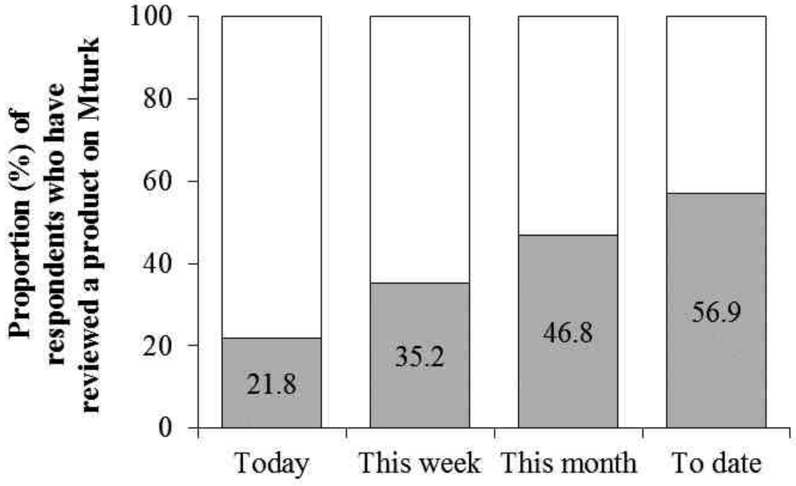
Discussion

Study 1 provides initial evidence for the idea that paying writers a small (vs. large or no) incentive for generating a review about a product makes them more uncertain of their attitude towards the product. This study also provides evidence for the underlying role of self-perceived legitimacy: a small incentive increases writer uncertainty by decreasing the perception that they are legitimate reviewers, presumably because these individuals rely on the size of their reward as a signal of the extent to which they are appropriate reviewers. Participants in the small incentive condition thus perceived that they were less legitimate reviewers than did participants paid no incentive (who relied on their experience to infer their legitimacy) or participants paid a large incentive (who saw the large reward as an indication that they are a legitimate reviewer) conditions.

STUDY 2

Study 2 had three goals. First, it aimed to replicate our initial findings while increasing ecological validity. Indeed, past work suggests that study setting (i.e., laboratory vs. field studies) is a particularly important moderator of the effect of monetary incentives (Jenkins 1986) because "money may have a simpler or even different meaning in artificial laboratory settings than in real field settings" (Jenkins et al. 1998). To address this issue, we conducted a field study on MTurk. Writing product reviews is a task frequently performed by MTurkers (Manga 2016) and ranks among of the top-50 task types offered on the platform (Ipeirotis 2010). For example, MTurk is one of the main platforms through which companies solicit reviews for online games, software, TV shows, movies and websites (Ipeirotis 2009). In addition, a pilot study (*n* = 216) revealed that 56.9% of MTurkers in our sample had been paid to write product reviews at least once in the past through MTurk (Figure 5).

Figure 5: Prevalence of incentivized reviews on Amazon Mechanical Turk



Design and Procedure

Three-hundred and thirty-five participants recruited on MTurk (52.94% female; M_{age}= 32.96 years) took part in an online study presented as a product test for an online game (Flea 2) and subsequently wrote a product review about this game.

We varied whether participants received a small (10c) or a large ($10) incentive. Payment amounts were selected based on the minimum wage in the U.S., which is $7.25 per hour or $1.80 for 15 minutes of work (United States Department of Labor 2009, see also page 15).

In addition to manipulating monetary incentives, we also varied whether participants who received a small incentive had the opportunity to affirm their legitimacy as reviewers. Specifically, half of the participants in the small incentive condition gave three reasons why they were a legitimate reviewer of the game (our affirmation condition adapted from Dalton and Huang 2014, experiment 4). The other half did not have this opportunity. Participants were thus assigned to one of three conditions: a small incentive without affirmation condition, a small incentive with affirmation condition, or a large incentive condition.

To further increase ecological validity and avoid the possibility of participants discussing differences in reimbursement on forums such as Reddit, we collected data in two waves: the first wave consisted of the small incentive without affirmation and the small incentive with affirmation conditions (all participants paid 10c). The second wave, collected a week later, consisted of the large incentive condition (all participants paid $10).

On MTurk, participants browse hundreds of studies, called Human Intelligence Tasks or HITs, and accept the one(s) they wish to complete. When browsing HITs, participants see both how long the HIT will take and how much they will be paid for completing it, and use this information to decide whether to 'accept' the HIT and start working on the task (Paolacci and Chandler 2014). Thus, participants in this study were exposed to the incentive manipulation *before* starting the study, as they would be in the real world.

After accepting our HIT, participants were again reminded of the incentive (see Appendix B) before reading a description of the online game they would review, and before playing it for at least 1 minute (M = 416.06 seconds, max = 6361.76 seconds, min = 75.78 seconds; excluding participants whose playtime was more than 3 standardized residuals from the average time spent playing the game did not change our results). Next, participants in the small incentive with affirmation condition wrote down three reasons for why they were an appropriate reviewer for the game (Appendix B) before writing the review. Participants in the small incentive without affirmation and large incentive conditions wrote their review immediately after playing the game.

Finally, participants indicated how legitimate they felt using six scale items adapted from past work (Rousseau and Tijoriwala 1999; Leach and Spears 2008; $\alpha = .89$; e.g., “I felt legitimate in expressing my view about the game” anchored at *disagree* (1) to *strongly agree* (11); see Appendix B). We also measured participants’ uncertainty using three scale items adapted from past work (Fazio and Zanna 1978; Tormala and Petty 2002; $\alpha = .77$; “I was uncertain of my opinion of the game” anchored at *strongly disagree* (1) to *strongly agree* (11); see Appendix B). In addition, participants completed the PANAS scale (Watson et al. 1988) to assess mood, indicated whether they had played the game before and provided demographic information.

As in study 1, we excluded participants who failed to follow the instructions (n = 0), had played the game before (n = 0), or whose score on the key dependent variable (uncertainty) fell more than 3 standardized residuals from the mean (n = 7), resulting in a final sample of 328 participants (52.13% female, M_{age} = 32.92 years).

Results

Incentive size and length of product experience. There was no difference in the amount of time participants spent playing the game between the small incentive without affirmation (M = 441.59s, SD = 670.97s) and the small incentive with affirmation (M =

414.61s, *SD* = 337.63s, *F*(1, 324) = .18, *p* = .67) or the large incentive (*M* = 390.16s, *SD* = 239.82s, *F*(1, 324) = .74, *p* = .39) conditions. There was also no difference in time spent playing the game between participants in the small incentive with affirmation and large incentive conditions (*F*(1, 324) = .16, *p* < .69).

Incentive size and writer uncertainty. To test our predictions, we ran an ANCOVA with review length as a covariate. As predicted, participants in the small incentive without affirmation condition reported feeling *more* uncertain of the attitude expressed in their review (*M* = 2.58, *SD* = 1.83) than participants in the small incentive *with* affirmation (*M* = 1.89, *SD* = 1.39) and large incentive conditions (*M* = 1.74, *SD* = 1.14, *F*(1,324) = 12.72, *p* < .001; Figure 6). There was no difference in uncertainty between participants in the small incentive with affirmation and large incentive conditions (*F*(1,324) = .00, *p* = .98, see Appendix D for all planned contrast results).

Incentive size and writer self-perceived legitimacy. Participants in the small incentive without affirmation condition perceived that they were *less* legitimate reviewers (*M* = 9.24, *SD* = 1.50) than participants in the small incentive *with* affirmation (*M* = 9.85, *SD* = 1.19) and large incentive conditions (*M* = 9.92, *SD* = 1.06, *F*(1, 324) = 13.48, *p* < .001; Figure 7). There was no difference in self-perceived legitimacy between participants in the small incentive with affirmation and large incentive conditions (*F*(1, 324) = .00, *p* = .97, see Appendix D for all planned contrast results).

Figure 6: Effect of incentive condition on writer uncertainty, study 2

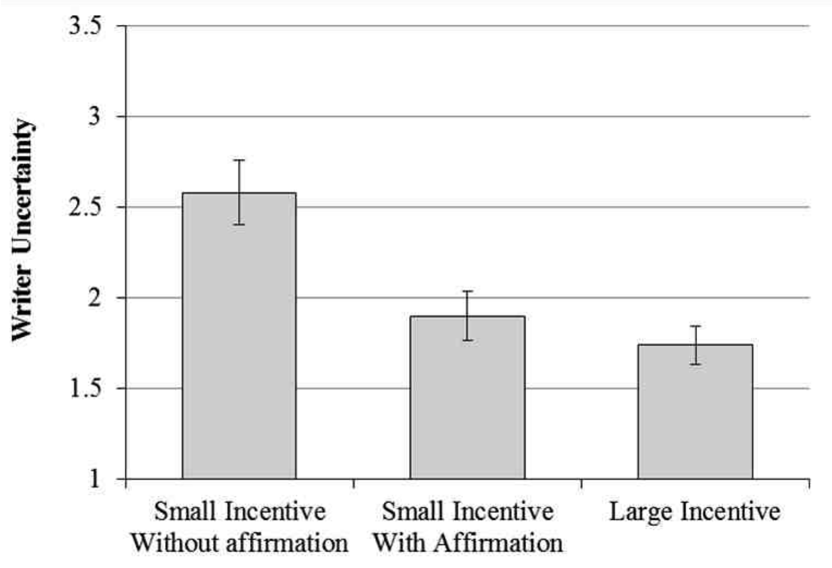
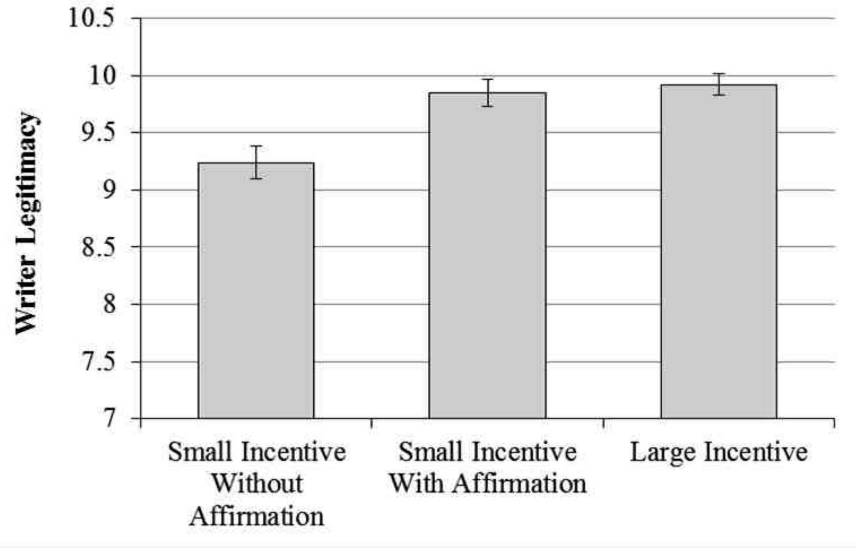


Figure 7. Effect of incentive condition on writer self-perceived legitimacy, study 2



Mediation by writer self-perceived legitimacy. To test whether self-perceived legitimacy mediated the effect of receiving a small incentive on writer uncertainty, we dummy coded our independent variable to represent the small incentive without affirmation condition versus the two other conditions (1 = small incentive without affirmation, 0 = small incentive with affirmation, 0 = large incentive) and used bias-corrected bootstrapping to generate a 95% confidence interval around the indirect effect of self-perceived legitimacy, where mediation occurs if the confidence interval excludes zero (Hayes 2013). The analysis (5,000 bootstrap samples; bias-corrected confidence intervals estimated and reported) revealed a significant indirect effect (ab = .33, SE = .11; 95% LLCI = .14, 95% ULCI = .57; see Appendix D for mediation results when the small incentive without affirmation condition is compared to the small incentive with affirmation and large incentive conditions separately). As predicted, compared to those who did affirm their legitimacy or who were paid a large incentive, writers paid a small incentive who did not affirm that they were an appropriate reviewer perceived themselves as less legitimate reviewers, and were subsequently more uncertain of their attitudes (Figure 8).

Figure 8: Mediation results, study 2

Robustness Check. To check the robustness of the findings, we reran the mediation and included positive and negative affect as statistical controls, along with review length. Results revealed similar patterns at standard levels of significance (ab = .15, SE = .07; 95% LLCI = .02, 95% ULCI = .31).

Discussion

Study 2 provides further support for H2 by showing that writers paid a small incentive (vs. a large incentive or those paid a small incentive who affirmed their legitimacy) were more uncertain of the attitudes they expressed in their reviews. In addition, this study provides further support for the proposition that small incentives increase uncertainty by changing writers' perceptions of their own legitimacy (H1 and H3). This was done on a platform routinely used by companies to solicit product reviews in exchange for monetary incentives.

HOW WRITER UNCERTAINTY INFLUENCES READER PRODUCT EVALUATIONS

We next explored the consequences of the effect of monetary incentives on writers' uncertainty for *readers*' reception of the reviews and subsequent product evaluations. Specifically, we propose that writers' uncertainty will affect readers by leading them to question the quality of the reviewed product, and, in response, lower their evaluations of the product. Indeed, past work suggests that people can detect uncertainty in a sender's message when uncertainty is salient (e.g., Dubois et al. 2011; Experiment 3, 1026). Thus, if small incentives increase writers' uncertainty, we expect uncertainty to be more easily detected by readers, and to carry over to their judgment of the reviewed product.

The certainty with which individuals communicate their attitude has been shown to affect message reception. When individuals make decisions in pairs, the sender's certainty is instrumental in shaping interpersonal dynamics during the decision-making task (Price and Stone 2004; Sniezek and Van Swol 2001; Zarnoth and Sniezek 1997). For instance, receivers are more inclined to question the reliability of the information they receive when it comes from a sender who lacks confidence (Price and Stone 2004). This heuristic – referred to as the confidence heuristic (Yates et al. 1996) – occurs because receivers are more inclined to perceive that a sender is hiding something when he or she lacks confidence (Thomas and McFadyen 1995). Based on this idea, we expect readers to be more likely to doubt the quality of the reviewed product as their perception that the writer is uncertain increases. Further, greater doubt in the quality of the reviewed product should lead readers to evaluate the product more negatively as they seek to avoid making a risky purchase.

Thus, we hypothesize that the effect of small (vs. large or no) monetary incentives on writer uncertainty will carry over to readers and lead them to infer that the writer is more uncertain. This perceived uncertainty will in turn increase reader doubt in the quality of the reviewed product, leading them to lower their evaluation of the product. Stated formally:

> **H4:** Readers will report lower evaluations of a reviewed product after reading a review written by a writer paid a small (vs. large or no) monetary incentive.

H5: This effect occurs because writers who are paid a small (vs. large or no) monetary incentive will be more uncertain of their attitudes. In turn, readers will infer greater writer uncertainty, subsequently doubt the quality of the product reviewed, and lower product evaluations.

STUDY 3

Our final study aimed to provide convergent evidence for the effect of monetary incentives on writers' uncertainty (H2), and to explore its consequences on readers' product evaluations (H4 and H5). To test these hypotheses, we conducted the study in two phases. In phase 1, participants were randomly assigned to a small, large or no incentive condition (as in study 1) in exchange for reviewing a yogurt. This first sample of participants acted as writers. In phase 2, the reviews generated in phase 1 were given to a new sample of participants who acted as readers and were blind to incentive conditions. This second sample of participants read reviews generated in phase 1 and evaluated the yogurt based on these reviews.

Phase 1: Review Generation

Participants and Design. Two-hundred and seventy-six students from a Dutch university (39.86% female, M_{age} = 20.31 years) participated in a study for course credit. Participants first read a description of a yogurt, tried a sample, and then wrote a review of it. Before reading the product description, participants were randomly assigned to one of three incentive conditions as in study 1.

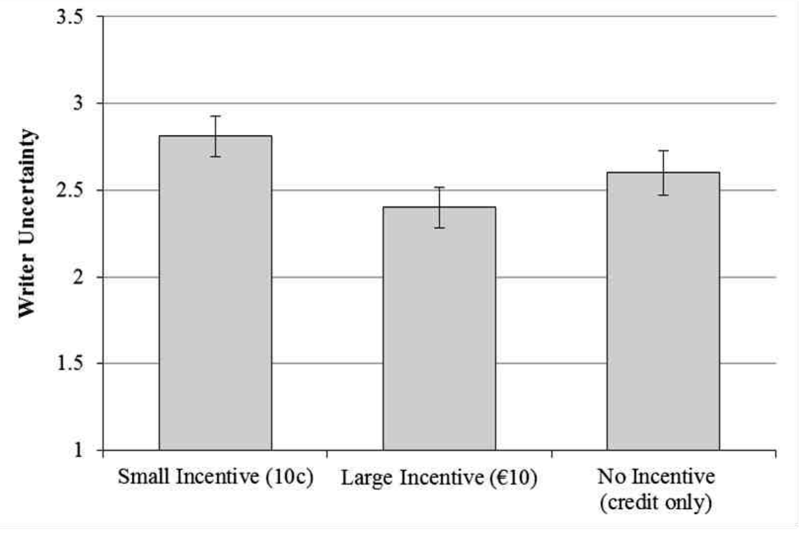
After reading a short description of the yogurt, the experimenter gave each participant a sample to try in a small cup (Figure 9). Upon finishing their sample, participants wrote a review of the yogurt. Next, they rated how uncertain they were of their attitude about the yogurt using three seven-point scale items (adapted from Fazio and Zanna 1978; Tormala and Petty 2002; α = .71; e.g., "I was uncertain of my opinion of the yogurt" anchored at 1 = "strongly disagree" to 7 = "strongly agree", see Appendix C). Finally, participants indicated if they had tried the yogurt prior to the study and provided their age and gender.

Figure 9: Product trial, study 3

After excluding participants who were familiar with the product (n = 39), who failed to follow the instructions (n = 0), or who provided uncertainty scores that were more than 3 standardized residuals from the mean (n = 1), our final sample consisted of 236 participants (36.02% female, M_{age} = 20.24 years).

Review generation results. To test whether receiving a small (vs. large or no) incentive increased writers' uncertainty, we ran an ANCOVA with review length included as a covariate. Replicating study 1, participants felt *more* uncertain in the small incentive condition (M = 2.81, SD = 1.02) than in the large incentive (M = 2.40, SD = 1.01) and no incentive conditions (M = 2.60, SD = 1.18, $F(1, 232) = 4.76$, $p < .05$; Figure 10). Uncertainty did not differ between the large and no incentive conditions ($F(1, 232) = .32$, $p = .57$, see Appendix D for all planned contrast results).

Figure 10: Effect of incentive condition on writer uncertainty, study 3



Discussion. Overall, phase 1 replicated the main effect of monetary incentives on writers' uncertainty observed in studies 1 and 2, providing further support for H2. Next, we turn our attention to the consequences of this effect for readers.

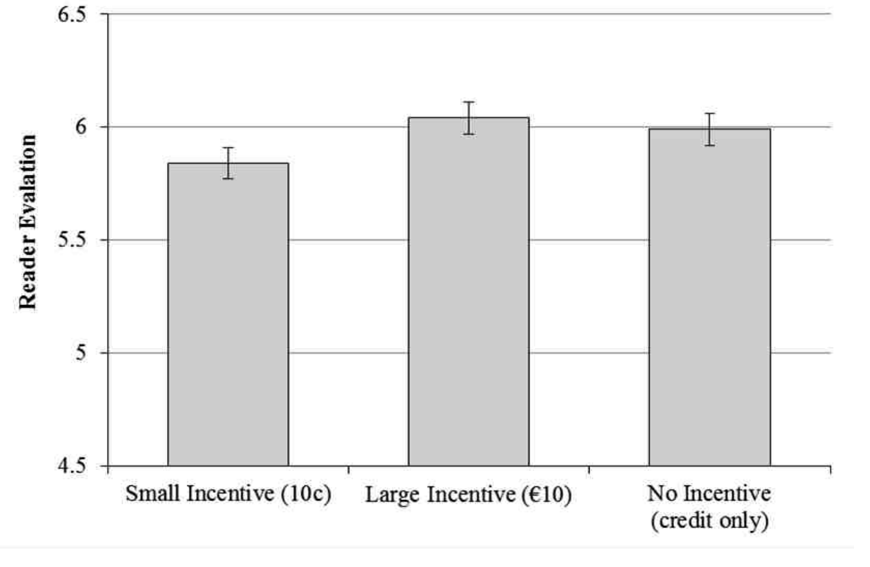
Phase 2: Review Reception

Participants and Design. Two-hundred and sixty-eight students from the same Dutch university (49.74% female, M_{age} = 19.15), blind to the incentive condition writers were in, read an average of 10.89 reviews (*SD* = .64; min = 8; max = 13). The reviews were randomly selected from the 236 reviews generated in phase 1. For each review, participants first rated how uncertain they perceived the writer was of their attitude based on the review. This perceived writer uncertainty was measured using three 7-point scales adapted from Dubois and colleagues (2011; α = .91; e.g., "The person who wrote this review was uncertain of their opinion of the yogurt," see Appendix C). Next, participants indicated how doubtful they were of the quality of the yogurt based on the review they just read, measured using three 7-point scales adapted from Tormala, Clarkson, and Petty (2006; α = 92; e.g., "Based on the review I am doubtful of the quality of the yogurt," see Appendix C). Participants also reported their evaluation of the yogurt on an 11-point star (anchored at: 1 star = bad – 11 stars = excellent) based on the review they read.

In addition to collecting these variables, we also aimed to assess potential differences in the content of the reviews generated. Thus, after reporting their evaluation of the product, participants content-analyzed the review they had just read before proceeding to the next review. Specifically, they counted the number of sentences, spelling mistakes and grammatical errors in the review. In addition, they also rated the review positivity (*r* = .85; e.g., "The review is positive"), descriptiveness (α = .87; e.g., "The review describes the yogurt in detail"), objectivity ("The review is objective"), extremity ("The attitude expressed in this review is extreme"), incomprehensibility (α = .74; "The review is hard to understand") and impartiality (α = .83; e.g., "This review presents a two-sided argument") using seven-point scales (1 = 'strongly disagree" to 7 = "strongly agree", see Appendix C). The order of presentation of these items was counterbalanced. Next, participants repeated the same procedure for the next review. After participants read and evaluated all reviews assigned to them, they proceeded to the next study in the experimental session.

Incentive size and reader product evaluations. To test the effect of small (vs. large or no) monetary incentives on readers' product evaluations, we regressed product evaluations on a variable reflecting monetary incentive conditions (coded as small = -1, no = 0 and large = 1). We also included a review random effect since each review was assessed multiple times. As in phase 1, review length (i.e., number of words) was included as a control variable. A planned contrast revealed that readers reported lower evaluations of the yogurt after reading reviews from writers paid a small incentive ($M = 5.84$, $SE = .07$) than writers paid a large ($M = 6.04$, $SE = .07$) and writers paid no monetary incentive ($M = 5.99$, $SE = .07$, $t(232) = -2.10$, $p < .05$; Figure 11). There was no difference in evaluations between the large and no monetary incentive conditions ($t(232) = -.50$, $p = .62$, see Appendix D for all planned contrast results).

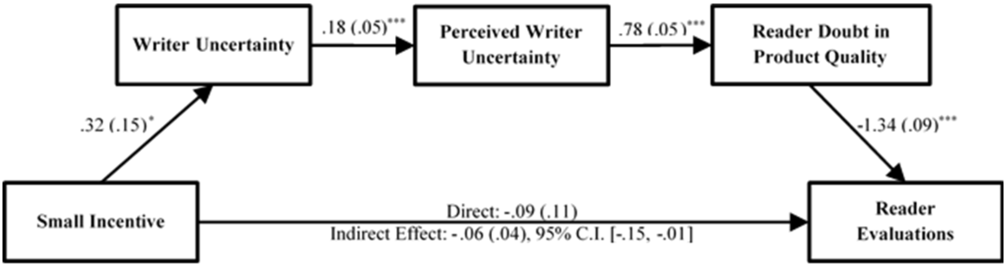
Figure 11: Effect of incentive condition on reader evaluations, study 3



Serial mediation results. We next aimed to test the prediction that readers' lower evaluation of the yogurt in the small incentive condition stemmed from the carry-over of writers' uncertainty (H5). Specifically, we predict that because small monetary incentives increase writer uncertainty, which is likely to be detected by readers, readers will doubt the quality of the product and hence lower their product evaluations. To test this causal chain, we used a serial mediation model (Hayes 2013, 143). However, because there was only one score for writer uncertainty for each review (each writer in phase 1 rated their own uncertainty), but multiple ratings of perceived writer uncertainty, doubt in product quality and product evaluations from different readers (each review from phase 1 was assessed by multiple readers in phase 2) we constructed new variables for each of the multilevel variables collected in phase 2. These variables represented the average score given by all readers of the review. As such, the serial mediation analyses were conducted at the review level across all variables in the model.

We used a biased-corrected bootstrapping procedure to generate a 95% confidence interval around the serial indirect effect of small incentives (1 = small incentive, 0 = large incentive, 0 = no incentive) on reader product evaluation through writer uncertainty (measured in phase 1), reader perception of writer uncertainty (measured in phase 2) and reader doubt in product quality (measured in phase 2), where mediation occurs if the confidence interval excludes zero (Hayes 2013). The analysis (5,000 bootstrap samples; bias-corrected confidence intervals estimated and reported) revealed a significant serial indirect effect through writer uncertainty, reader perception of writer uncertainty, and reader doubt in product quality (ab = -.06, SE = .04; 95% LLCI = -.15, 95% ULCI = -.01, see Appendix D for mediation results when the small incentive condition is compared to the large and no incentive conditions separately). No other indirect effects were significant. Thus, receiving a small (vs. a large or no) incentive increased writer uncertainty, which in turn led readers to perceive the writer as more uncertain, increase their doubt in the quality of the reviewed product, and ultimately lower their product evaluations (Figure 12). Thus, H5 was supported.

Figure 12: Serial mediation results, study 3



Robustness check of serial mediation. To check the robustness of the findings, we re-ran the analyses and included the average number of sentences, grammatical errors, and spelling mistakes in each review, as well as average reader ratings of review positivity, descriptiveness, objectivity, extremity, incomprehensibility, and impartiality as statistical controls in addition to review length. The hypothesized indirect effect remained significant (ab = -.01, SE = .004; 95% LLCI = -.02, 95% ULCI = -.001).

What facets of review content underlie the effect of incentives on reader evaluations? To examine what specific facets of review content might underlie the effect of our incentive condition on reader evaluations, we first reran the random effects regression in which we regressed reader evaluations on a variable reflecting the incentive conditions (1 = small; 0 = no incentive; 1 = large), with review length and all content characteristics (i.e., review descriptiveness, positivity, objectivity, impartiality, readability, extremity, number of sentences, grammatical errors, and spelling mistakes) as control variables. Including these content characteristics as statistical controls yielded a non-significant main effect of receiving a small incentive on reader evaluations (b = -.03, t(232) = -.37, p = .71). However, when review descriptiveness and review positivity were removed as controls, the main effect remained significant (b = -.18, t(232) = -2.00, p < .05), indicating that a change in one or both of these content characteristics might mediate the effect of writers receiving a small incentive on reader evaluations.

To test which of these characteristics underlie the effect, we ran two additional serial mediation models in which the average rating of each characteristic for each review was included as a potential serial mediator. When review positivity was included as a potential serial mediator (small incentive → writer uncertainty → review positivity → reader perception of writer uncertainty → reader doubt in product quality → reader product evaluations) only the hypothesized indirect effect was significant (ab = -.01, SE = .005; 95% LLCI = -.02, 95% ULCI = -.001). Therefore, review positivity did not seem to alter the proposed model. However, including review descriptiveness as a serial mediator (small incentive → writer uncertainty → review descriptiveness → reader perception of writer uncertainty → reader doubt in product quality → reader evaluations) lead to a significant indirect effect (ab = -.001, SE = .001; 95% LLCI = -.003, 95% ULCI = -.0001). Only two other indirect effects were significant: (1) the indirect effect through descriptiveness in the absence of reader perception of writer uncertainty (small incentive → writer uncertainty → review descriptiveness → reader doubt in product quality → reader evaluations: ab = -.003, SE = .002; 95% LLCI = -.01, 95% ULCI = -.0002) and (2) the hypothesized indirect effect (small incentive → writer uncertainty → reader perception of writer uncertainty → reader doubt in product quality → reader evaluations: ab = -.01,

SE = .005; 95% LLCI = -.02, 95% ULCI = -.001). No other indirect effects were significant. Overall, receiving a small incentive increased writer uncertainty, and this led to lower descriptiveness in the review, from which readers inferred greater uncertainty, ultimately increasing their doubt in the product quality and lowering their evaluations.

Discussion. Phase 1 of study 3 replicated earlier findings (H2), while phase 2 demonstrated the effect of monetary incentives on readers' evaluations of the reviewed product (H5). Specifically, readers gave the yogurt lower evaluations after reading reviews written by writers paid a small (vs. large or no) incentive. Serial mediation analyses showed that writers paid a small (vs. large or no) incentive felt more uncertain of their attitude, and that readers picked up on this uncertainty when reading their reviews. In response, readers doubted the quality of the product and decreased their evaluation of it. Overall, the effect of monetary incentives on readers' uncertainty is consequential because uncertainty carries over from writers to readers and affects readers' judgments of the product featured in the review.

A secondary finding of this study lies in our preliminary analysis of how writer uncertainty affected review content. Specifically, writers paid a small incentive wrote less descriptive reviews because they were uncertain of their attitude. Importantly, readers inferred uncertainty from lower descriptiveness, which increased their doubt in the product quality and decreased their product evaluations.

GENERAL DISCUSSION

Companies increasingly offer small monetary incentives to encourage consumers to write product reviews and post them online. The present research investigated how such incentives influence the psychology of writers, the generation of online reviews, and their subsequent reception by readers. Building on the idea that monetary incentives convey a social signal of the value ascribed to the role of reviewer, we show that writers infer their own legitimacy as reviewers from the size of the incentive provided. Hence a small (vs. large or no) monetary incentive reduces their perception of being a legitimate reviewer. This in turn increases their uncertainty, which carries over to readers, raising doubt in the quality of the product reviewed and subsequently decreasing readers' product evaluations.

These findings extend previous work on incentivization which primarily focused on the effect of incentives on productivity (see Camerer and Hogarth 1999; Jenkins et al. 1998, and Deci, Koestner, and Ryan 1999 for reviews). Whereas prior research focused on the positive effect of incentivized product reviews (e.g., Burtch et al. 2006; Chevalier and Mayzlin 2006), this research documents the potentially detrimental effect of small incentives, which can foster attitude uncertainty and reduce review persuasiveness, compared to large or no incentives. Specifically, we have shown how monetary incentives not only increase the effort that the recipient puts into the review (Burtch et al. 2016), but also influence the way the incentive recipient processes information and forms attitudes. Of note, we controlled for review length in all our studies, suggesting that monetary incentives influence attitude formation independently of the amount of effort writers put in their reviews. In addition, we consistently maintained some level of extrinsic motivation since all participants were participating for extrinsic rewards (i.e., course credit and money in studies 1 and 3; money only in study 2). Overall, our findings suggest an additional route through which monetary incentives affect the psychology of writers, above and beyond triggering effort.

This research contributes to the attitude formation and persuasion literatures in three important ways: First, we provide empirical support for the prediction that the feelings of legitimacy underlying an attitude can influence the certainty with which this attitude is held. Although past research has alluded to the idea that legitimacy and attitude (un)certainty are related (Rucker et al. 2014), we are the first to provide empirical support for this link.

Second, our findings suggest that attitude uncertainty may stem from both the appraisal of information *external* to the individual and of the individual him/herself. While Rucker et al. (2014) proposed that individuals are more uncertain of their attitude when they see the information upon which it is based as less legitimate, our results show that individuals' appraisal of their *own* legitimacy (i.e., their self-perceived legitimacy) can also influence the (un)certainty with which they hold and express their attitudes.

Third, this research contributes to past work on the sharing of attitude uncertainty in interpersonal contexts (e.g., Dubois and et al 2011). We shed light on a novel factor – monetary incentives – that may shape the transmission of uncertainty. Our research opens the door to the possibility that monetary incentives might similarly shape the sharing of information in other contexts in which uncertainty matters. For instance, sales people paid a smaller salary may be less persuasive than those paid larger salaries (because small salaries may impact self-perceived legitimacy, and hence attitude uncertainty).

The current work also documents a novel content characteristic individuals use to infer (un)certainty: the extent to which a message is descriptive (study 3). Specifically, participants wrote less descriptive reviews when they were uncertain of their attitude (when they were paid a small incentive) than when certain of their attitude (when they were paid a large incentive or no incentive). Although preliminary, this result contributes to past efforts aiming to unpack the different antecedents of uncertainty (Rucker et al. 2014). Thus, an interesting implication of our finding is that people who are certain of their attitudes might also be more likely to provide in-depth details regarding their experience.

Finally, this research contributes to the WOM literature by showing that content characteristics other than review valence may influence WOM effectiveness. Past efforts focused on the valence of WOM and showed that positive reviews tend to increase sales (e.g., Chevalier and Mayzlin 2006). However, more recent work started investigating other aspects of communications, such as an incentivized advocate's perceived sincerity (Barasch, Berman and Small 2016). Our research contributes to this emerging line of work by highlighting another important content dimension (i.e., uncertainty) that may shape the ability of individuals to influence others.

Practical Implications

The present work illustrates that offering monetary incentives to stimulate online reviews may sometimes backfire by altering content in unexpected ways. This is important because companies are increasingly incentivizing consumers to generate online reviews, for example by relying on marketing seeding campaigns whereby they encourage selected consumers (i.e., "seeds") to experience a product and write reviews about it (Chae et al. 2016). This practice is often accompanied by monetary incentives. Our findings provide initial evidence that the practice of providing *small* monetary incentives to consumers to write product reviews may backfire by decreasing reader product evaluations.

We found that writers tended to be more certain of their attitudes when they were paid a large monetary incentive or when they were unpaid. Though one may be tempted to infer that avoiding incentivization altogether may be the best approach, we suggest that decisions regarding incentive provision and size may also depend on brand factors affecting the importance of uncertainty – communicated through product descriptiveness. For example, a well-known brand may not value the extent to which reviewers describe its products (since people likely already know the product well) and merely view the number of reviews generated as a desired outcome. In contrast, a novel brand or a brand launching a new product might benefit from long and detailed reviews from writers, and as such should carefully calibrate monetary incentives to limit uncertainty.

Finally, managers have traditionally focused on the positivity of product reviews for assessing the marketplace's response to products and services, and ultimately use it as a predictor of success. The current findings suggest that in markets where reviews are abundant (e.g., service industries such as hospitality) managers should keep track of other aspects of online review content, such as uncertainty, to assess brand health.

Limitations and Future Research

The present research has several limitations that provide exciting avenues for future research. First, in all studies, we used 10c as a small incentive and $10 (or €10) as a large incentive. Although a strong rationale underlies our operationalization of incentive size through these amount (see page 15), future research could explore whether and how different incentive levels might modulate the effect documented in the current paper. For example, it is possible that *very large* incentives (e.g., $100 or more) could also lead reviewers to feel less legitimate if they feel that such incentives do not reflect their perceived value (i.e., they are not "worth it"), and communicate their attitudes with less certainty compared to the large incentive used in the present research (i.e., $10).

Future research might also investigate how writer uncertainty, induced by small monetary incentives, might interact with incentive disclosure to influence readers' product evaluations. Incentive disclosure is now required in many countries such as the USA and Australia (e.g., Australian Competition and Consumer Commission 2016; United States Federal Trade Commission 2013). Existing research suggests that consumers may evaluate reviewed products more negatively when it is disclosed that the writer was incentivized (du Plessis et al. 2016). One possibility is that disclosing monetary incentives may merely increase uncertainty among readers by making them suspicious that the review overemphasizes the positive aspects of the reviewed product. A second possibility is that incentive disclosure may differentially affect readers' response to the review depending on the size of the incentive provided. On the one hand, disclosure may exacerbate the detrimental effects of small incentives on readers' product evaluations by making readers question the quality of a product promoted by incentivized writers coming across as uncertain of their attitudes. On the other hand, disclosure may have little impact when the incentive is large: though disclosure may lead consumers to lower their product evaluations, the effect might be weaker for reviews written in exchange for large (vs. small) incentives when readers infer that the size of the incentive motivated the writer to spend more time experiencing the product and yielded a more accurate and trustworthy review.

CONCLUSION

In a world where companies are increasingly relying on online product reviews to win consumers and drive sales, providing monetary incentives to consumers to write reviews and post them online appears to be smart business. Soliciting such reviews can also be cheap; almost 70% of consumers on MTurk indicate that they write product reviews for small incentives ($1 or less per review). The present research shows that this practice of "paying peanuts" to consumers to write product reviews may, however, do more harm than good since monetary incentives alter the way in which writers process information, form their attitudes and ultimately express their opinions in product reviews.

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APPENDIX A: SUPPLEMENTARY MATERIALS FOR STUDY 1

Study 1 Incentive Manipulation

Small Incentive Condition

In this study you will watch two video, using the SONY MDR-ZX310 headphones provided, and then write a review of the headphones.

We will pay you **10 cents** to write this review.

This payment will be in addition to the one course credit you will receive for participating in the experimental session.

Note: The 10 cents will be paid in cash to you at the end of this study by the experimenter.

Please do not copy and paste an existing review from the internet. Write your own review, in English, based on your experience with the headphones (e.g., how the headphones look, how comfortable they are, the sound quality etc.)

Large Incentive Condition

In this study you will watch two video, using the SONY MDR-ZX310 headphones provided, and then write a review of the headphones.

We will pay you **10 euros** to write this review.

This payment will be in addition to the one course credit you will receive for participating in the experimental session.

Note: The 10 euros will be paid in cash to you at the end of this study by the experimenter.

Please do not copy and paste an existing review from the internet. Write your own review, in English, based on your experience with the headphones (e.g., how the headphones look, how comfortable they are, the sound quality etc.)

No Incentive Condition

In this study you will watch two video, using the SONY MDR-ZX310 headphones provided, and then write a review of the headphones.

Note: Please do not copy and paste an existing review from the internet. Write your own review, in English, based on your experience with the headphones (e.g., how the headphones look, how comfortable they are, the sound quality etc.)

Measurement Items, Study 1

Review Sender Uncertainty (a =.72)

1. I am uncertain of my opinion of the headphones [1 (strongly disagree) to 7 (strongly agree)]
2. I was doubtful about my opinion of the headphones [1 (strongly disagree) to 7 (strongly agree)]
3. How confident are you in your review? [Reverse coded; 1 (not at all confident) to 7 (extremely confident)]
4. How correct are the opinions you expressed in your reviews? [Reverse coded; 1 (incorrect) to 7 (correct)]

Sender Legitimacy (a =.84)

1. I feel that my review is legitimate [1 (strongly disagree) to 7 (strongly agree)]
2. I feel that my review is fair [1 (strongly disagree) to 7 (strongly agree)]
3. The compensation for writing my review made me feel legitimate in expressing my

view about the headphones [1 (strongly disagree) to 7 (strongly agree)]

4. The compensation for writing my review was fair [1 (strongly disagree) to 7 (strongly agree)]
5. The compensation for writing my review gave me the feeling that I should put effort in my review [1 (strongly disagree) to 7 (strongly agree)]
6. The compensation for writing my review made me feel good about myself [1 (strongly disagree) to 7 (strongly agree)]
7. The compensation for writing my review made me feel respected [1 (strongly disagree) to 7 (strongly agree)]
8. The compensation for writing my review made me feel important as an individual [1 (strongly disagree) to 7 (strongly agree)]
9. The compensation for writing my review made me feel that I am a person of worth [1 (strongly disagree) to 7 (strongly agree)]

APPENDIX B: SUPPLEMENTARY MATERIALS FOR STUDY 2

Study 2 Incentive Manipulation

Small Incentive Condition

In this study you will play an online game and then write a review of it. In exchange for your review **we are paying you 10 cents.**

[Affirmation Condition]

Before writing your review, please take a moment to <u>**think about why you are an appropriate reviewer for this game**</u>. Then, please write down three reasons for why you think you are a legitimate reviewer of the game you just tried (that is, a person worth asking a review from for this product). For each reason, please describe, in detail, how this characteristic or experience makes you a good reviewer.

Important: Your response to this question will not influence your payment for this study.

Reason 1:

Reason 2:

Reason 3:

Large Incentive Condition

In this study you will play an online game and then write a review of it. In exchange for your review **we are paying you $10.**

Measurement Items, Study 2

Sender Legitimacy (a =.89)

1. I felt legitimate in expressing my view about the game [1 (strongly disagree) to 11 (strongly agree)]
2. I felt that my review is legitimate [1 (strongly disagree) to 11 (strongly agree)]
3. I felt that my opinion is valuable [1 (strongly disagree) to 11 (strongly agree)]
4. I felt good about myself [1 (strongly disagree) to 11 (strongly agree)]
5. I felt like I am a person of worth [1 (strongly disagree) to 11 (strongly agree)]
6. I felt important as an individual [1 (strongly disagree) to 11 (strongly agree)]

Review Sender Uncertainty (a =.77)

1. I am uncertain of my opinion of the game [1 (strongly disagree) to 11 (strongly agree)]
2. I was doubtful of my opinion of the game [1 (strongly disagree) to 11 (strongly agree)]
3. How confident are you in your review? [Reverse coded; 1 (not at all confident) to 11 (extremely confident)]

APPENDIX C: SUPPLEMENTARY MATERIALS FOR STUDY 3

Measurement Items, Study 3

Review Sender Uncertainty (a =.71)

1. I am uncertain of my opinion of the game [1 (strongly disagree) to 7 (strongly agree)]
2. I was doubtful about my opinion of the game [1 (strongly disagree) to 7 (strongly agree)]
3. How confident are you in your review? [Reverse coded; 1 (not at all confident) to 7 (extremely confident)]

Review Recipient Perception of Review Sender Uncertainty (a =.91)

1. The person who wrote this review was confident in their opinion of the yogurt [1 (strongly disagree) to 7 (strongly agree)]
2. The person who wrote this review was uncertain of their opinion of the yogurt [1 (strongly disagree) to 7 (strongly agree)]
3. The person who wrote this review was doubtful of their opinion of the yogurt [1 (not at all confident) to 7 (extremely confident)]

Review Recipient Doubt in Product Quality (a =.92)

1. Based on the review I am doubtful of the quality of the yogurt [1 (strongly disagree) to 7 (strongly agree)]
2. Based on the review I am confused about the quality of the yogurt [1 (strongly

disagree) to 7 (strongly agree)]

3. Based on the review I am uncertain about the quality of the yogurt [1 (not at all confident) to 7 (extremely confident)]

Review Positivity (r =.85)

1. The review was positive [1 (strongly disagree) to 7 (strongly agree)]
2. The review was favorable [1 (strongly disagree) to 7 (strongly agree)]

Review Descriptiveness (a =.87)

1. The review describes the yogurt in detail [1 (strongly disagree) to 7 (strongly agree)]
2. The review contains many details about the yogurt [1 (strongly disagree) to 7 (strongly agree)]
3. This is a high quality review [1 (strongly disagree) to 7 (strongly agree)]
4. The review is helpful [1 (strongly disagree) to 7 (strongly agree)]
5. The review helps me picture myself eating the yogurt [1 (strongly disagree) to 7) strongly agree)]

Review Incomprehensibility (a =.74)

1. The review is hard to understand [1 (strongly disagree) to 7 (strongly agree)]
2. The review is vague [1 (strongly disagree) to 7 (strongly agree)]
3. The review is easy to read [Reverse coded; 1 (strongly disagree) to 7 (strongly agree)]

Review Impartiality (a =.83)

1. The review presents a two-sided argument [1 (strongly disagree) to 7 (strongly agree)]
2. The review discusses the positive and negatives of the yogurt [1 (strongly disagree) to 7 (strongly agree)]
3. The arguments presented in this review are balanced [1 (strongly disagree) to 7 (strongly agree)]

APPENDIX D: SUPPLEMENTARY ANALYSES FOR STUDY 1 - 3

Table D1: Planned contrasts, studies 1 – 3

Study	DV	Conditions Compared	Planned Contrast Results
1	Uncertainty	Small vs. Large and No	$F(1, 222) = 4.25, p < .05$
		Small vs. Large	$F(1, 222) = 3.27, p = .07$
		Small vs. No	$F(1, 222) = 2.99, p = .09$
		Large vs. No	$F(1, 222) = .00, p = .95$
	Legitimacy	Small vs. Large and No	$F(1, 222) = 46.27, p < .001$
		Small vs. Large	$F(1, 222) = 36.03, p < .001$
		Small vs. No	$F(1, 222) = 32.13, p < .001$
		Large vs. No	$F(1, 222) = .01, p = .90$
2	Uncertainty	Small vs. Small with Affirmation and Large	$F(1, 324) = 12.72, p < .001$
		Small vs. Small with Affirmation	$F(1, 324) = 10.57, p = .001$
		Small vs. Large	$F(1, 324) = 8.41, p < .01$
		Large vs. Small with Affirmation	$F(1, 324) = .00, p = .98$
	Legitimacy	Small vs. Small with Affirmation and Large	$F(1, 324) = 13.48, p < .001$
		Small vs. Small with Affirmation	$F(1, 324) = 11.41, p < .001$
		Small vs. Large	$F(1, 324) = 8.75, p < .01$
		Large vs. Small with Affirmation	$F(1, 324) = .00, p = .97$
3	Uncertainty	Small vs. Large and No	$F(1, 232) = 4.76, p < .05$
		Small vs. Large	$F(1, 232) = 4.65, p < .05$
		Small vs. No	$F(1, 232) = 2.54, p = .11$
		Large vs. No	$F(1, 232) = .32, p = .57$
	Product Rating	Small vs. Large and No	$t(232) = -2.10, p < .05$
		Small vs. Large	$t(232) = -2.04, p < .05$
		Small vs. No	$t(232) = -1.56, p = .12$
		Large vs. No	$t(232) = -.50, p = .62$

Table D2: Indirect effects, study 1 – 3

Study	DV	Mediator	Conditions Compared	Statistical Controls	Indirect Effect
1	Uncertainty	Legitimacy	Small vs. Large and No	Review Length	*ab* = .16, *SE* = .06; 95% LLCI = .05, 95% ULCI = .30
			Small vs. Large	No incentive dummy Review length	*ab* = .15, *SE* = .06; 95% LLCI = .05, 95% ULCI = .30
			Small vs. No	Large incentive dummy Review length	*ab* = .16, *SE* = .06; 95% LLCI = .05, 95% ULCI = .31
2	Uncertainty	Legitimacy	Small vs. Small with Affirmation and Large	Review Length	*ab* = .33, *SE* = .11; 95% LLCI = .14, 95% ULCI = .57
			Small vs. Small with Affirmation	Large incentive dummy Review length	*ab* = .33, *SE* = .12; 95% LLCI = .12, 95% ULCI = .58
			Small vs. Large	Small with affirmation dummy Review length	*ab* = .33, *SE* = .11; 95% LLCI = .13, 95% ULCI = .58
3	Product Rating	Writer uncertainty (M1) Perceived Writer Uncertainty (M2) Reader Doubt in Product Quality (M3)	Small vs. Large and No	Review Length	*ab* = -.06, *SE* = .04; 95% LLCI = -.15, 95% ULCI = -.01
			Small vs. Large	No incentive dummy Review length	*ab* = -.07, *SE* = .04; 95% LLCI = -.17, 95% ULCI = -.02
			Small vs. No	Large incentive dummy Review length	*ab* = -.05, *SE* = .04; 91% LLCI = -.14, 91% ULCI = -.001

CHAPTER 3

WHEN PAYING DOES (NOT) PAY OFF: WHEN AND WHY INCENTIVE DISCLOSURE LOWERS PRODUCT EVALUATIONS

With the rise of ecommerce and social media, user-generated content (UGC) in the form of consumer-generated product ratings and reviews has become ubiquitous. Moreover, consumers' online reviews are recognized as a persuasive source of information that can influence consumers' product judgments and, ultimately, purchase decisions. Prior research shows that UGC, in general, can affect a variety of important marketing outcomes, including customer acquisition, sales, and consumption (Berger 2014; Chevalier and Mayzlin 2006; Godes and Mayzlin 2009; Lamberton and Stephen 2016; Liu 2006; Stephen 2016; Stephen and Galak 2012; Trusov, Bucklin, and Pauwels 2009; Villanueva, Yoo, and Hanssens 2008; You, Vadakkepatt, and Joshi 2015), as well as consumer decision confidence (Lamberton, Naylor, and Haws 2013) and choice (Moore 2015).

Online product reviews contain two types of information: core (the review content and/or rating itself) and auxiliary (information accompanying a review, such as details about a reviewer and how a review was generated). While extant research has primarily focused on core information, auxiliary information has received only scant attention from researchers so far, despite its prevalence and potential importance. Broadly, the lack of understanding of how auxiliary UGC information may affect consumers represents a critical gap in the literature, particularly since auxiliary information, of various kinds, could be highly influential. For example, work on source credibility, which can be signaled through status-indicating "badges" or labels next to reviewers' names (an example of auxiliary information), implies that auxiliary information may affect the extent to which a reviewer's opinion influences consumer choices (Luca 2011). Similarly, it is possible that knowing how a reviewer came to write a review and whether there might have been an ulterior motive due to, for example, receiving some incentive or compensation, could conceivably affect persuasion.

This latter example of auxiliary information is the focus of this research. We consider auxiliary information in the form of disclosures of reviewer incentives in the context of consumer-generated product reviews. This form of auxiliary information is important in practice because firms are increasingly turning to incentives, such as coupons, rebates, free samples, and even small monetary payments, as a means of encouraging customers to write reviews (WOMMA 2013). At the same time, many firms, as well as government regulators and consumer protection agencies, have established policies requiring transparent disclosures of such incentives (e.g., Australian Competition and Consumer Commission 2016; U.S. Federal Trade Commission 2009). Presumably, if a consumer reading a review learns that the reviewer received an incentive, this is likely to induce some uncertainty in her mind about the reviewer's trustworthiness. This disclosure-induced uncertainty might lead her to discount the reviewer's opinion as a means of correcting for presumed reviewer bias, even if the reviewer in fact was not biased by the incentive. This so-called "incentive backlash effect" presents a dilemma for firms: while using incentives is often necessary to encourage consumers to post reviews, incentive disclosure could negate a review's persuasiveness.

We posit, however, that the incentive backlash effect that would be expected based on prior research on attitude certainty and persuasion will not always occur. While disclosure may lead consumers to doubt the trustworthiness of the reviewer, this does not necessarily imply that the induced uncertainty will always undermine the persuasiveness of a reviewer's opinion. We argue that whether uncertainty does or does not have this effect depends on the outcome of a metacognitive process in which consumers elaborate on their disclosure-induced uncertainty about reviewer trustworthiness and question its relevance to the judgment or decision at hand.

Across three studies (a field study and two experiments), we show that when the uncertainty is deemed *integral* to judgment formation (when its relevance is questioned and thought to be relevant), the uncertainty will be treated as informative and the incentive backlash effect will occur. However, when the uncertainty is deemed *incidental* to judgment formation (when its relevance is questioned and thought to be irrelevant), the uncertainty will not be treated as informative and the incentive backlash effect will *not* occur. Thus, ironically, elaborating on *and doubting* one's uncertainty in a reviewer's trustworthiness may actually preserve the persuasive power incentivized reviewers can have over consumers' review-influenced product evaluations.

Theoretically, these findings are important because they demonstrate that whether or not a reviewer's opinion is persuasive depends on consumers' metacognitions of their disclosure-induced uncertainty (i.e., thinking about, or elaborating on, their uncertainty in the trustworthiness of the reviewer). This is contrary to and builds on prior research suggesting that thinking about one's attitude uncertainty leads one to ignore or discount the focal attitude (Petty, Briñol, and Tormala 2002). Thus, even though some uncertainty would always be induced by the disclosure of a reviewer's incentives, this will not always undermine the persuasiveness of the reviewer's opinion.

THE ROLE OF ATTITUDE (UN)CERTAINTY IN PERSUASION

Our conceptualization builds on research on attitude certainty, which is "the subjective sense of conviction a person has regarding an attitude" and refers to the extent to which a person believes their attitude is correct or valid (Gross, Holtz, and Miller 1995; Petty, Briñol, and Tormala 2007; Rucker et al. 2014; Tormala 2016). This belief is based on a person's appraisal of the validity of the attitude, done through elaborating on the accuracy, completeness, relevance, legitimacy, or importance of the information and their affective assessment of the validity of the attitude (Rucker et al. 2014). Thus, attitude certainty is a metacognitive "tag" attached to an attitude that reflects a secondary assessment of the primary cognition (Petty and Briñol 2006). For example, in the context of consumer-generated reviews and disclosure of a reviewer's incentive, for the attitude "This reviewer is trustworthy" a metacognitive assessment would be asking "Am I sure about that?" and thinking through reasons why this uncertainty is or is not relevant to the judgment or decision at hand, and therefore the extent to which this uncertainty is diagnostic.

Certainty metacognitions are important because thoughts held with greater confidence tend to be more persuasive (have a stronger impact on attitudes and behaviors) than thoughts held with less confidence. This has been shown in work on the "self-validation effect" (Petty et al. 2002), which explains why uncertainty about the validity or relevance of an attitude will render that attitude *less* persuasive in subsequent judgments and decisions. For example, Briñol, Petty, and Tormala (2004) showed that advertising messages from non-credible sources are less persuasive because consumers are less confident in their attitudes formed based on such messages. Based on these findings one may expect that, in the context of incentivized reviews accompanied by uncertainty-inducing disclosure statements, uncertainty in reviewer trustworthiness will lead a review reader to doubt the attitudes they form based on such a review, which should in turn decrease persuasion (Tormala, Briñol, and Petty 2006). Furthermore, because this process is metacognitive (Rucker et al. 2014) and metacognitive reasoning requires cognitive effort (Petty et al. 2007), this should be especially likely to occur under higher levels of elaboration (Rucker et al. 2014). To summarize the predictions from prior research, the central argument is that if a consumer is uncertain about an attitude, elaborating on this uncertainty would strengthen the impact of the uncertainty.

While this makes intuitive sense, we argue that this will not always be the case in our context. Instead, when a consumer elaborates on uncertainty they have about an incentivized reviewer's trustworthiness we suggest that it is possible that the uncertainty's impact on judgments will *not* be strengthened and instead could be weakened. In other words, thinking about one's uncertainty will not always result in that uncertainty interfering with the persuasiveness of the reviewer's opinion. This is because, ironically, although elaboration increases the likelihood that a person will assess the extent to which they are uncertain of an attitude (Briñol and Petty 2009; Briñol, Petty, and Tormala 2004; Petty et al. 2002), it can also lead them to doubt the relevance of this uncertainty to the judgment or decision that consumer is facing.

Thus, if through this metacognitive elaboration process the uncertainty in the reviewer's opinion is deemed invalid or irrelevant, then we would expect the uncertainty to not interfere with persuasion. This would imply that, for instance, if an incentivized reviewer's opinion about a product is positive, then this will contribute to a consumer also evaluating the product in a positive manner. Conversely, if this process results in a consumer deciding that their uncertainty about the reviewer's trustworthiness is indeed a relevant and legitimate concern, then, in line with prior work, that uncertainty would interfere with persuasion. That would mean, for instance, that an incentivized reviewer's positive opinion about a product would have less of an effect on a consumer's evaluation of the product. Put simply, if a consumer elaborates on the uncertainty surrounding a reviewer's trustworthiness, we suggest that things could go in one of two ways—either the uncertainty is deemed relevant and then undermines the persuasive power of the review, or it is deemed not relevant and does not undermine the persuasive power of the review.

The prediction that uncertainty related to reviewer incentivization needs not always undermine the persuasive power of a reviewer's opinion (i.e., reduction in incentive backlash effect) is contrary to prior work on attitude uncertainty and persuasion, as noted earlier. While consumers will likely experience some increase in uncertainty about a reviewer's trustworthiness when exposed to auxiliary information disclosing that a reviewer received an incentive in exchange for her review, we argue that this uncertainty *will not necessarily* carryover to interfere with the persuasiveness of the review. We show that elaborative processing—thinking about uncertainty—is critical here. This is because the probable default (i.e., when there is no, or minimal, elaboration) would be for one's judgments to be colored by the uncertainty and pushed in the direction of either prior beliefs or other information that is salient at the time. Following prior research on attitude formation (e.g., Chen and Chaiken 1999; Petty, Cacioppo, and Schumann 1983; Petty and Wegener 1999), consumers in our context will likely rely on the heuristic that "all incentivized reviewers are biased" when they spend little or no time thinking about their uncertainty. As such, under low elaboration and in the presence of no information suggesting otherwise, we would expect the incentive backlash effect to occur. To overcome that, therefore, actual thought that considers the relevance of one's uncertainty in reviewer trustworthiness is likely to be required.

When would uncertainty about an incentivized reviewer's trustworthiness be deemed relevant versus not relevant? We suggest that metacognitive elaboration on such uncertainty will render it relevant (e.g., a legitimate concern) if it is deemed to be *integral* in judgment formation. Or, conversely, it will be thought of as lacking relevance (e.g., not a legitimate concern) when it is deemed to be *incidental* in judgment formation. Importantly, the uncertainty could be deemed incidental—thus weakening the otherwise-expected incentive backlash effect—if there is additional information related to the reviewer or reviewer-generation process (i.e., other auxiliary information) that brings the relevance of the consumer's uncertainty into question. For example, if a consumer believes that it is in fact a common practice for companies to incentivize reviewers, it might not seem so egregious or illegitimate when a reviewer is found to be incentivized, thus rendering uncertainty less of a concern. Of course, additional information could also work the other way, resulting in a consumer deeming their uncertainty to be *integral* to their judgment-formation process. For example, if auxiliary information discloses not only that an incentive was paid to a reviewer, but also that the payer of the incentive had a strong desire for favorable reviews (e.g., because they are the retailer or manufacturer of the product), then this might give a consumer good reason to suspect potential reviewer bias.

In summary, our conceptualization predicts when incentive disclosure-induced uncertainty in a reviewer's trustworthiness will versus will not influence the persuasive power of that reviewer's opinion on consumers. This builds on prior work that would suggest that such uncertainty would always color a consumer's judgment, and suggests conditions under which this would in fact *not* happen. As discussed earlier, the outcome—

whether or not there is an incentive backlash effect—hinges on what happens if consumers metacognitively elaborate on their uncertainty. If this leads them to deem their uncertainty to be integral in judgment formation (i.e., relevant) then the backlash will be expected, just as prior work would predict. However, if elaboration in the presence of additional information results in them deeming their uncertainty to be incidental in judgment formation (i.e., not relevant), we would not expect a backlash. Stated formally as hypotheses:

H1: When incentive disclosure-induced uncertainty in reviewer trustworthiness is deemed incidental it will not have a negative effect on a consumer's product evaluation when more (versus less) time is spent elaborating on it.

H2: When incentive disclosure-induced uncertainty in reviewer trustworthiness is deemed integral it will have a negative effect on a consumer's product evaluation when more (versus less) time is spent elaborating on it.

We test these hypotheses in a field study and two experiments. In study 1 we analyze reviews from Amazon.com that were or were not incentivized to see whether, using real-world data, there is evidence of the aforementioned incentive backlash effect and if it is moderated by elaboration. The results provide initial support for hypothesis 1 since we find that the incentive backlash effect is attenuated when product category involvement is high. Next, we systematically test our hypotheses in two controlled experiments. In study 2 we find preliminary support for hypothesis 2 and show that elaboration facilitates the incentive backlash effect when uncertainty in reviewer trustworthiness is deemed integral to judgment formation. Finally, in study 3 we manipulate whether or not uncertainty is integral or incidental to judgment formation and find support for both hypothesis 1 and hypothesis 2.

STUDY 1

Study 1 provides initial, real-world support for our conceptual framework, particularly hypothesis 1, by showing that elaboration may moderate the incentive backlash effect. To do this, we collected data on publicly available product reviews on the U.S. version of Amazon.com. These reviews were accompanied by auxiliary information that could induce uncertainty in reviewer trustworthiness: a statement disclosing whether or not the reviewer was incentivized as part of a program called "Amazon Vine." Under this scheme, selected customers are sent free products (i.e., receive an incentive) and then post reviews for the received products on Amazon. If a product review is written by a customer who received that product for free under the Vine program, auxiliary information indicating that the reviewer was incentivized is displayed with a label stating "Vine Customer Review of Free Product" (see Figure 1).

To ensure that we could obtain this auxiliary information for all the products in our sample, we considered only products that were part of the Vine program at the time of the study. Naturally, not all reviews for a product in the Vine program will be incentivized, however, which allows us to compare—for the same products—incentivized versus not incentivized reviews (i.e., the presence or absence of incentive-disclosing auxiliary information for the same product is used as a quasi-experimental factor). Our expectation was that Vine reviews could make consumers more uncertain of reviewer trustworthiness, which would result in lower review persuasiveness compared to non-Vine reviews. Review persuasiveness was measured using the helpfulness rating for each review ("X of Y people found the following review helpful"), which is a proxy for persuasiveness since a review with a higher helpfulness score is likely to have had greater influence on consumers' opinions. Thus, the incentive backlash effect should manifest in lower helpfulness scores for Vine reviews than for non-Vine reviews.

Figure 1: Amazon vine program review and disclosure statement

Importantly, in addition to this, we attempted to find initial support for our prediction that elaboration moderates the incentive backlash effect. Since the data here are non-experimental and limited to what can be observed publicly on Amazon.com, we used product category as a proxy for elaboration. Based on past research showing that consumers elaborate more on higher-involvement products (Petty et al. 1983; Petty and Wegener 1999), we considered certain products—higher involvement ones—to be likely to engender higher elaboration because more involvement is required when considering or evaluating them. Thus, we expected the negative effect of Vine reviews on review helpfulness to be moderated by the level of product involvement. Based on past literature we expected to observe the incentive backlash effect for low involvement products since responses to these products are likely to be driven by the heuristic that "incentivized reviews are biased". Further, based on our theory, if incentive-related uncertainty in reviewer trustworthiness is incidental then the incentive backlash effect should be weaker for higher involvement products (i.e., hypothesis 1). However, if uncertainty is integral to judgment formation then the incentive backlash effect will be stronger for higher involvement products (i.e., hypothesis 2). Since this is a preliminary study using real-world, non-experimental data, we are agnostic to the nature of this interaction since it was unclear a priori whether disclosure-induced uncertainty would be incidental or integral when it is elaborated upon. Our purpose, simply, is to demonstrate that such an interaction exists, which we then explore in detail in study 2.

Sample and Data Collection

We compiled a diverse set of 10 products available on the U.S. Amazon store in April and May 2014 and sampled 300 reviews across these 10 products. Table 1 lists the products, which ranged in price from \$12.81 to \$499.99 at the time of data collection (*M* = \$111.06, *SD* = \$161.26). Thirty reviews were collected for each product, divided equally into three types: (1) incentivized/Vine, (2) verified purchase, and (3) not verified purchase. Incentivized reviews were part of the Amazon Vine program and carried the disclosure statement "Vine Customer Review of Free Product." Verified purchase reviews were not incentivized but carried the statement "Verified Purchase" to indicate that the reviewer had purchased the product from Amazon. Not verified purchase reviews were not incentivized and carried no additional information. See appendix A for additional details on product selection.

Table 1: List of products for which reviews were collected in study 1

Product	**Price**	**Product Category**
BPI Sports B4 Once-Daily Fat Burner	$21.80	Vitamin B Supplements
Huggies Little Movers Diapers	$31.98	Disposable Diapers
Innocent Blood: The Order of Sanguines Series	$16.78	Books Supernatural
iRobot Roomba 770 Vacuum	$499.99	Robotic Vacuums
KIND Nuts and Spices, Dark Chocolate Bars	$14.24	Sports Nutrition Food Bars
Krups XP100050 Espresso Machine	$59.99	Small Appliances
Labrada Nutrition Garcina Extract Capsules	$14.00	Appetite Suppressants
Tide High Efficiency Detergent	$12.81	Liquid Detergent
Toshiba Chromebook	$269.99	Chromebook Laptops
Weber Q1000 Grill	$169.00	Freestanding Grills

For each review, we collected the helpfulness score displayed at the top of each review (i.e., "X of Y people found the following review helpful"), which was our dependent variable. Four control variables we also collected: (i) the order in which the review was presented on Amazon under default newest-to-oldest sorting, (ii) the product rating (1 to 5 stars) given by the reviewer, (iii) the number of reviews written by the reviewer, and (iv) the proportion of people who found all the reviews written up to the time of data collection by the reviewer to be helpful (including the current review). The majority of reviews in our sample were favorable: the mean rating (1-5) given by reviewers was 3.48 (SD = 1.54, median = 4), only 89 out of 300 reviews (29.67%) had ratings below 3, and the mode was, in fact, 5 (116 out of 300 reviews, 38.67%).

Results

Impact of incentive disclosure on review helpfulness. We expected incentivized (Vine) reviews to have lower helpfulness scores than non-incentivized (Verified, Not verified) reviews. This would suggest that lower review helpfulness is associated with incentive disclosure inducing uncertainty in the trustworthiness of reviewers, and be evidence of the expected incentive backlash effect. We estimated a binomial model (since our dependent variable was a proportion of people who said that the review was helpful) in which review helpfulness was regressed on dummy variables for Verified and Not Verified review types (i.e., incentivized Vine reviews were the baseline for these contrasts). We also included the control variables to account for observed reviewer- and product-level heterogeneity, and a product random effect to control for unobserved product-level heterogeneity.

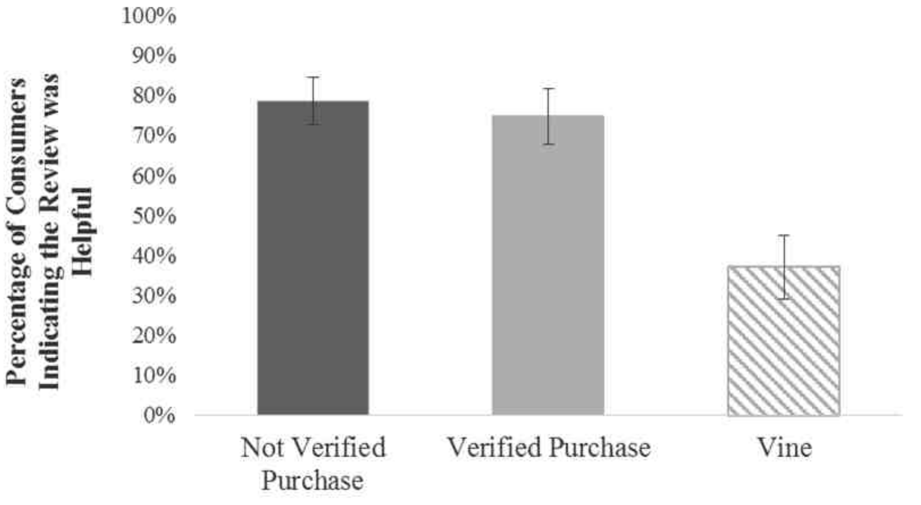
Regression results are reported in table 2 and the average aggregate helpfulness scores based on this model are shown in figure 2. As predicted, the main effect of review type on helpfulness was significant ($F(2, 284) = 9.42$, $p < .001$). Analysis of the simple effects revealed that Verified Purchase reviews ($b = 1.63$, $t(284) = 3.58$, $p < .001$) and Not Verified reviews ($b = 1.85$, $t(284) = 4.16$, $p < .001$) were significantly more helpful than incentivized Vine reviews. There was no difference in helpfulness between Verified and Not Verified reviews ($b = .22$, $t(284) = .39$, $p = .58$). On average, based on the model, 36.65% of Vine reviews were deemed helpful by Amazon users, compared to 78.58% of Not Verified reviews and 74.72% of Verified reviews (see Figure 2).

Table 2: Binomial regression results for review type main effect

Parameter	**Estimate (std. err.)**
Intercept	-4.96 (.87) ***
Review type = not verified	1.85 (.44) ***
Review type = verified	1.63 (.46) ***
Review order	.01 (.02)
Reviewer's product rating	.06 (.11)
Number of reviews by reviewer	.001 (.001)
Reviewer's helpfulness on other reviews	5.26 (.84) ***
Random effect variance	.43 (.31)

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

Figure 2: Review helpfulness scores by review type



Moderation by product category (involvement). Next, we tested whether elaboration—using product involvement level as a proxy—moderates the effect of incentive disclosure on perceived review helpfulness. Two independent coders classified the 10 products into high involvement (Chromebook computer, robot vacuum cleaner, vitamin supplements, weight-loss tablets, propane grill, and espresso machine) and low involvement (nut bars, book, diapers, and laundry detergent) products based on (i) price, (ii) the product's potential for inflicting harm (social, economic, and/or physical) if an ill-informed decision is made, (iii) how difficult the product's characteristics are to comprehend, and (iv) whether or not the brand is well known. Inter-rater reliability was high ($\kappa = .7$) indicating significant agreement among coders regarding the classification. Differences were resolved through discussion.

We estimated a binomial model in which review helpfulness was regressed on dummy variables for Verified and Not Verified review types, a dummy variable for high involvement products (i.e., low involvement products were the baseline), and the interactions between involvement and each of the review type dummy variables. As in the previous model, we included a product random effect to control for unobserved product-level heterogeneity, and the control variables to account for observed reviewer- and product-level heterogeneity.

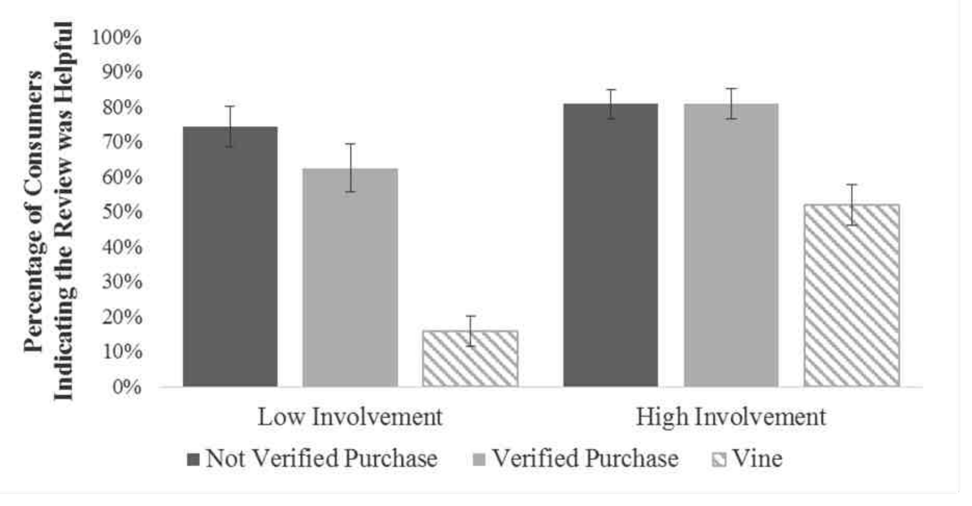
Results are reported in table 3 and the average aggregate helpfulness scores based on this model are shown in figure 3. The main effect of review type ($F(2, 16) = 20.01$, $p < .001$) and product category involvement ($F(1, 8) = 21.38$, $p < .01$) were both significant and their interaction was marginally significant ($F(2, 16) = 3.14$, $p = .07$). We next explored the nature of this interaction by investigating the simple effects. Simple effects showed that Vine reviews were rated significantly less helpful than non-Vine reviews when product category involvement was low ($b = -1.67$, $t(16) = -5.10$, $p < .001$), but not when it was high ($b = .07$, $t(16) = .31$, $p = .76$).

Table 3: Binomial regression results showing moderation by product category involvement

Parameter	**Estimate (std. err)**
Intercept	-4.45 (.63) ***
Review type = not verified	1.36 (.37) **
Review type = verified	1.36 (.40) **
Involvement = low	-1.74 (.37) **
Involvement*Review type = low and not verified	1.36 (.55) *
Involvement*Review type = low and verified	.81 (.54)
Review order	.01 (.01)
Reviewer's product rating	.11 (.07)
Number of reviews by reviewer	.00 (.00)
Reviewer's helpfulness on other reviews	5.24 (.60) ***
Random effect variance	.52 (.04)

* $p < .05$, ** $p < .01$, *** $p < .0001$

Figure 3: Moderation by product category involvement



On average, based on the model, 15.9% of Vine reviews of low involvement products were deemed helpful by Amazon users, compared to 74.15% of Not Verified reviews and 62.45% of Verified reviews. In contrast, when product category involvement was high, on average, based on the model, 51.82% of Vine reviews were rated helpful, compared to 80.71% of Not Verified reviews and 80.80% of Verified reviews. Contrasts showed no difference in helpfulness ratings for non-incentivized (Verified and Not Verified) reviews across the two levels of involvement (b = .55, t(16) = 1.02, p = .32).

Is lower helpfulness due to incentive provision or incentive disclosure? An alternative explanation for the lower helpfulness ratings observed for Vine reviews could be that incentive provision may alter the content or the quality of the reviews (instead of the auxiliary information increasing doubt in reviewer trustworthiness). To rule this out we ran a study in which the reviews were evaluated in the absence of incentive disclosure information (i.e., just the review text) to demonstrate that the effect was driven by the incentive disclosure and not the content itself. For this we recruited 5,394 members of Amazon Mechanical Turk, but dropped 40 because they were not U.S. consumers (and would therefore may not be familiar with the products) or native English speakers, resulting in a final sample size of 5,354. Each participant was randomly assigned one of the 300 reviews and asked to read and evaluate it. Before reading the review, participants were also shown the manufacturer's product description displayed on Amazon. Each review was assessed by an average of 17.85 participants (SD = 1.30, min. = 13, max. = 21). Importantly, participants *did not* see any auxiliary information and were therefore blind to review type.

After reading their assigned product description and review, participants rated the product on a five-point "star" scale (1 to 5) to measure their product evaluation. They also indicated how helpful they perceived the review to be on two seven-point Likert-scaled items that were averaged to form a single indicator of perceived review helpfulness ("This review was helpful" and "This review was useful"; 1 = "strongly disagree" to 7 = "strongly agree;" r = .85). Participants also rated the reviews on a number of other characteristics including how positive, objective, and trustworthy the review(er) is (see Appendix B for details).

We regressed perceived review helpfulness on a variable indicating whether the review was incentivized under the Vine program (1) or not incentivized (-1). Since each review was assessed multiple times, and reviews were nested within multiple products, we included random effects for both review and product. We also included the same control variables described earlier. Interestingly, the results showed that in the *absence* of disclosure, Vine reviews were in fact rated *more helpful* than non-Vine reviews ($b = .54$, $t(5054) = 7.00$, $p < .001$). Given that this effect is in the opposite direction to the effect reported earlier, it suggests that it is likely the presence of auxiliary information disclosing the reviewer was incentivized that is triggering the negative effect of Vine reviews on helpfulness, and not something to do with the content of the reviews. Additionally, with these participants we also found that perceived review helpfulness carries over to positively affect consumers' product evaluations (i.e., evidence supporting our above-stated assumption that helpfulness is a reasonable proxy for the persuasiveness of a reviewer's opinion). Participants evaluated the reviewed product on a 1-5 rating scale, and this was found to be positively correlated with perceived review helpfulness ($r = .11$, $p < .001$).

Discussion

Using 300 reviews from Amazon.com covering 10 diverse products, and exploiting a natural quasi-experiment created by Amazon's Vine program and review incentive disclosure policy, we found preliminary support for our prediction that elaborating on one's uncertainty about a reviewer's trustworthiness can moderate the incentive backlash effect. Specifically, our findings are suggestive of, and provide initial support for, hypothesis 1, given that we found an attenuation of the incentive backlash effect for higher involvement products that, presumably, engender higher levels of elaboration. This suggests that in the context of Amazon reviews at least, consumers tend to deem incentive disclosure-induced uncertainty in reviewer trustworthiness as being incidental to judgments when they give this some thought.

In summary, this study demonstrated both the existence of the incentive backlash effect (in line with prior research) and, critically, that the size of this effect depends on factors related to consumer elaboration. Moreover, these findings are based on real-world data. Of course, this can only be interpreted as preliminary support given the constraints and limitations of such data. In study 2 and 3 we use experiments to test hypothesis 1 and 2 directly.

STUDY 2

The purpose of study 2 is to provide preliminary support for hypothesis 2 by showing that elaborating on uncertainty about reviewer trustworthiness, induced by incentive disclosure, will render the uncertainty integral to judgment formation when elaboration leads consumers to not question the relevance of their uncertainty. In addition, this study also measures uncertainty, thus allowing us to directly show that incentive disclosure induces uncertainty.

Here we used a different type of auxiliary information that would confirm the relevance of uncertainty in reviewer trustworthiness when it is elaborated upon (thus making participants *not* question their uncertainty) by disclosing both the provision of an incentive *and* its source. The logic was that if the incentive source had a vested interest in receiving positive reviews, such as a manufacturer or retailer who would stand to benefit from higher sales associated with positive reviews, then it would be more likely that uncertainty related to this would be deemed integral to judgment formation than in the converse, where the source did not have a vested interest. Put simply, if the incentive is disclosed and it comes from a source who would like reviewers to be positively biased, then the uncertainty is likely to be integral to judgment formation when it is elaborated on, in line with hypothesis 2.

Also in this study we manipulated the review valence, since previously only positive reviews were considered. Our theory most likely applies in the case of non-negative reviews, since the purchase-related risk of trusting a biased positive review is greater than the risk of trusting a biased negative review (e.g., if a consumer trusts a biased positive review they might purchase a product with heightened expectations that could be unmet, leading to dissatisfaction; but if a consumer trusts a biased negative review they may just avoid making a purchase). As such, the predicted effects should occur when reviews are not negative, which we demonstrate here as a boundary condition.

Design and Procedure

Four-hundred and one participants from Amazon Mechanical Turk were recruited for this study. In this and the subsequent study we decided *a priori* to exclude participants if English was not their first language and/or if they attempted to participate in the study again after their first time (only their first time was included). Based on these criteria, eight participants were excluded, resulting in a final sample size of 393. Subjects were randomly assigned to a condition in a 2(valence: positive review vs. negative review) x 4(disclosure: silent, paid by non-profit, paid by game maker, paid by game seller) between-subjects design. Participants were asked to read a review of a product and then answer some questions about it.

The product in this study was an online game called "Bloons". In this game, players control an animated monkey who fires darts at colorful balloons. The goal is to pop as many balloons as possible in each round.

The procedure was as follows. First, participants read a brief description of the game, and the review corresponding to their condition. At this stage both valence and disclosure were manipulated by varying (i) whether the purported source of the incentive has a vested interest in receiving a positive review (i.e., when the incentive provider is the game maker or game seller) or not (i.e., when the incentive provider is a third-party non-profit or when no information about incentive disclosure is provided); and (ii) the valence of the review (i.e., positive or negative; see Appendix C for the stimuli and pretest results confirming the validity of the disclosure manipulation). After reading the review, participants were asked to rate the game on a five-point star rating scale (1-5). Finally, we measured uncertainty in the reviewer's trustworthiness with five seven-point Likert-scaled items ($\alpha = .88$; 1 = "strongly disagree" to 7 = "strongly agree;" e.g., "I am doubtful about the reviewer's intentions"; see Appendix C). Here, we also measured review positivity as a check of the valence manipulation, using two seven-point Likert-scaled items ($r = .98$; 1 = "strongly disagree" to 7 = "strongly agree;" "The review was positive" and "The review was favorable"). To measure elaboration, we also recorded the amount of time participants spent reading the review before rating the product.

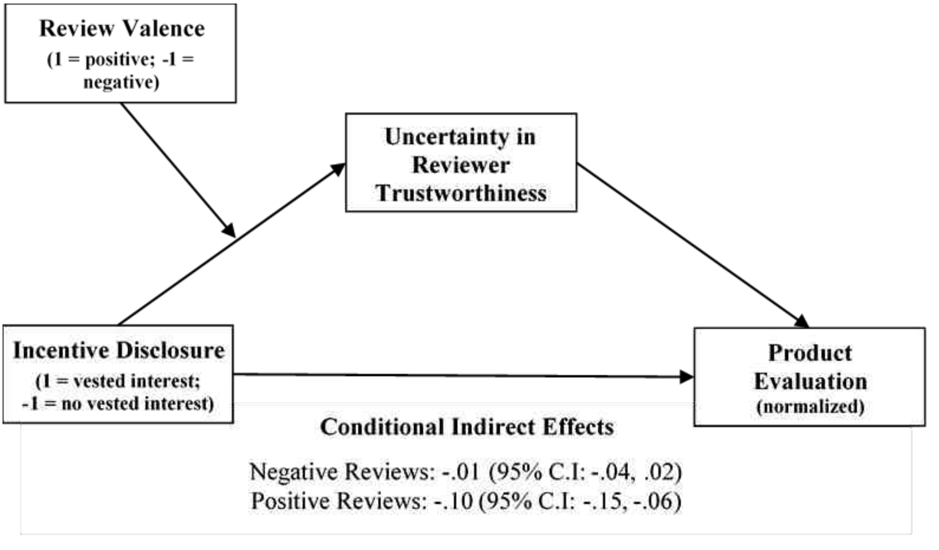
Results and Discussion

Manipulation checks. A pretest confirmed that our disclosure manipulation operated as intended. Participants reported that the game maker ($M_{\text{game maker}} = 5.98$, $SD = 1.39$) and game seller ($M_{\text{game seller}} = 5.95$, $SD = 1.30$) were both more likely to "benefit" from a positive review than the third party non-profit ($M_{\text{non-profit}} = 4.53$, $SD = 1.69$; both ps $< .001$; see Appendix C for a complete description of the pretest). The difference in perceived benefit between the game seller and game maker was not significant ($F(1,138) = .01$, $p = .91$). For convenience in the data analysis we collapsed the four-level disclosure factor into two levels representing a vested interest (game maker, game seller; pooled disclosure = 1) and no vested interest (silent, non-profit; pooled disclosure = -1). Our valence manipulation also operated as intended. The positive review was rated as significantly more positive ($M_{\text{positive}} = 6.46$, $SD = .65$) than the negative review ($M_{\text{negative}} = 1.63$, $SD = .76$; $F(1, 392) = 4523.61$, $p < .001$).

Main results. We first tested whether the effect of disclosure on product evaluations, through uncertainty, was moderated by valence. Since we considered positive and negative reviews, for the dependent variable we created a valence-invariant measure of product evaluation by subtracting the mid-point value of the star rating scale (3) from the measured rating and taking the absolute value of the difference. This resulted in a measure that captured how far the product evaluation was from the neutral scale mid-point (i.e., how positive it was in the positive valence conditions or how negative it was in the negative valence conditions). We refer to this measure as normalized product evaluation.

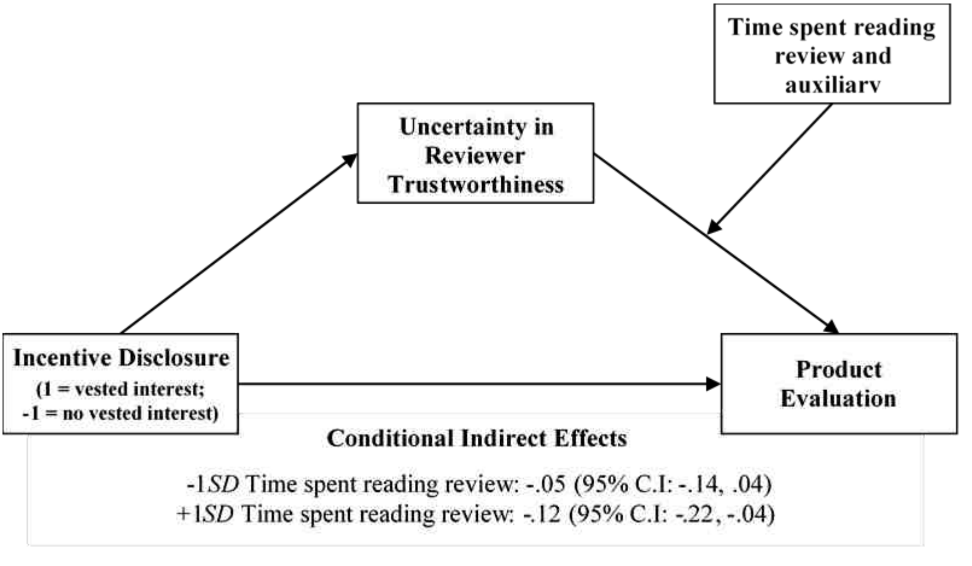
Using a conditional indirect effects analysis (Hayes 2013, model 7) we found a significant negative indirect effect of pooled disclosure (vested interest) on normalized product evaluation through uncertainty when valence was positive (indirect effect = -.10, 95% C.I. = [-.15, -.06]). The effect was not significant, however, when the review was negative (indirect effect = -.01, 95% C.I. = [-.04, .02]; see Figure 4). Thus, our disclosure manipulation for inducing uncertainty in reviewer trustworthiness operated as intended. When the review was positive, disclosing incentive payment by a source who had a vested interest in a positive review increased uncertainty in the trustworthiness of the reviewer which negatively affected product evaluations. However, this was not the case when the review was negative.

Figure 4: Conditional indirect effect of disclosure on normalized product evaluations as a function of review valence



Next, for positive reviews only, we tested our prediction that relevant uncertainty in a reviewer's trustworthiness—in this case, when the incentive source has a vested interest in positive reviews—will only carry over to negatively affect product evaluations when it is elaborated upon. This means that, for positive reviews, the negative indirect effect of disclosure on product evaluations, through uncertainty, should only be significant when participants spend more time reading the review and accompanying auxiliary information. This is expected to occur since elaboration should enable participants to consider the relevance of their uncertainty in the trustworthiness of the reviewer for judging product quality. Since participants will become more confident that their uncertainty is relevant when they spend more time elaborating on it, they should rate the product lower when elaboration time is higher (the incentive backlash effect will occur). This will not occur, however, when participants spend less time elaborating.

Figure 5: Conditional indirect effect of disclosure on product evaluations among positive reviews



As expected, a conditional indirect effects analysis (Hayes 2013, model 14) found a significant negative indirect effect of incentive disclosure when the source had a vested invested on product evaluation through uncertainty when evaluation time was higher (+1 *SD*; indirect effect = -.12, 95% C.I. = [-.22, -.04]), but not when evaluation time was lower (-1 *SD*; indirect effect = -.05, 95% C.I. = [-.14, .04]; Figure 5).

Discussion

In this study, we tested the prediction that elaboration can render disclosure-induced uncertainty integral to judgment formation. Based on hypothesis 2, we predicted and found that when an incentivized review is positive and it is disclosed that the incentive source has a vested interest in receiving positive reviews, participants who spent more (vs. less) time ruminating on this lowered their product evaluations based on their disclosure-induced uncertainty. Our predictions therefore hold when elaboration is higher. However, in contrast to study, we do not observe heuristic processing in this study under lower elaboration (though the coefficient of the indirect effect under lower elaboration is, as expected, negative). There may be a number of reasons for this, including noisier data, participants spending less time reading the review and auxiliary information, or our specific operationalization of elaboration in this study. Importantly, consistent with hypothesis 2, however, the incentive backlash effect is facilitated (the coefficient becomes larger and significant) as elaboration time increases. Taken together, studies 1 and 2 demonstrate that elaboration can render uncertainty in reviewer trustworthiness either incidental or integral to judgment formation, thus moderating the impact of incentive disclosure on product evaluations or, more generally, the incentive backlash effect with respect to review persuasiveness. In the next study we expand on these findings by providing further support for hypothesis 1 and 2 by testing both hypotheses in a single study.

STUDY 3

The purpose of this final study is to test both hypothesis 1 and 2 simultaneously by manipulating whether or not disclosure-induced uncertainty is relevant. In this study we created a context in which incentive disclosure would raise doubts in consumers' minds, but where, upon elaboration, the relevance of the resulting uncertainty may versus may not be confirmed.

This was achieved by leading participants to believe that it is either common or uncommon for firms to pay consumers to write product reviews. We based this on the following logic. Participants who believe that incentivizing reviews is common practice should be more inclined to question the relevance of their uncertainty when they think about it, because incentivization no longer signals something potentially improper about the exchange between the reviewer and the incentive source. Thus, when reviewer incentives are common, uncertainty will be appraised as illegitimate and not relevant when it is elaborated upon, and thus be deemed as incidental in judgment formation. The incentive backlash effect should therefore not occur. When it is not elaborated on, however, the incentive backlash effect should be observed since participants rely on the heuristic that incentivized reviewers are biased. On the other hand, participants who believe that it is an abnormal, uncommon practice to incentivize reviewers will be less inclined to question the relevance of their uncertainty to their product judgment when they spend time thinking about it. As such, participants who elaborate on this will likely deem their uncertainty as relevant, thus making it integral to judgment formation.

Consequentially, the incentive backlash effect should occur. Similar to the predictions made in study 2 we also expect to observe the incentive backlash effect under lower elaboration, due to heuristic processing.

Design and Procedure

One hundred and seventy members of Amazon Mechanical Turk participated in this study. Two participants were excluded because English was not their first language or because they attempted to participate in the study more than once, which resulted in a final sample size of 168. Participants were randomly assigned to conditions in a 2(disclosure: paid vs. not paid) x 3(norm: payment-common vs. payment-uncommon vs. control) between-subjects design. The reviewed product was a Samsung Chromebook computer and in all conditions the review was moderately positive (see Appendix D).

The procedure was as follows. First, participants were exposed to the norm manipulation (described below). Second, participants read the product review and an accompanying disclosure statement (auxiliary information). The review was the same in all conditions, and all that varied was the auxiliary information. In the paid conditions, the auxiliary information stated, “This reviewer was paid by Samsung to write this review.” In the not-paid conditions the disclosure was, “This reviewer was not paid to write this review.” Third, participants were asked to rate the product on a five-point “star” rating scale to provide a measure of their product evaluation, which was used as the dependent variable. Finally, participants completed Likert-scaled items designed to measure uncertainty in reviewer trustworthiness, which was the mediating variable ($\alpha = .95$, 1 = “strongly disagree” to 7 = “strongly agree;” e.g., “I am doubtful about the reviewer’s intentions; see Appendix D). Elaboration was again measured unobtrusively using response time, in this case the time spent evaluating the product.

The manipulation for payment norm (common vs. uncommon vs. control) was as follows. Participants were given information about product reviews framed as “something that they may be interested to know about.” See appendix D for the complete text. The information reported results from a (fictitious) study that was purportedly conducted by the Pew Research Center (which often produces reports about Internet consumption and user behavior). In the payment-uncommon condition the “results” were that 78% of companies said in a survey that they “do not attempt to solicit online reviews…by offering people incentives or payment of some kind” and that 12% of consumers said in a survey that they think companies “on a regular basis pay or incentivize people to post product reviews on websites.” In the payment-common condition identical wording was used except that we stated that 78% of companies “do attempt to solicit online reviews” and that 88% of consumers believe that companies pay for reviews. In the control condition participants were not provided with any of the fictitious Pew Research Center study results.

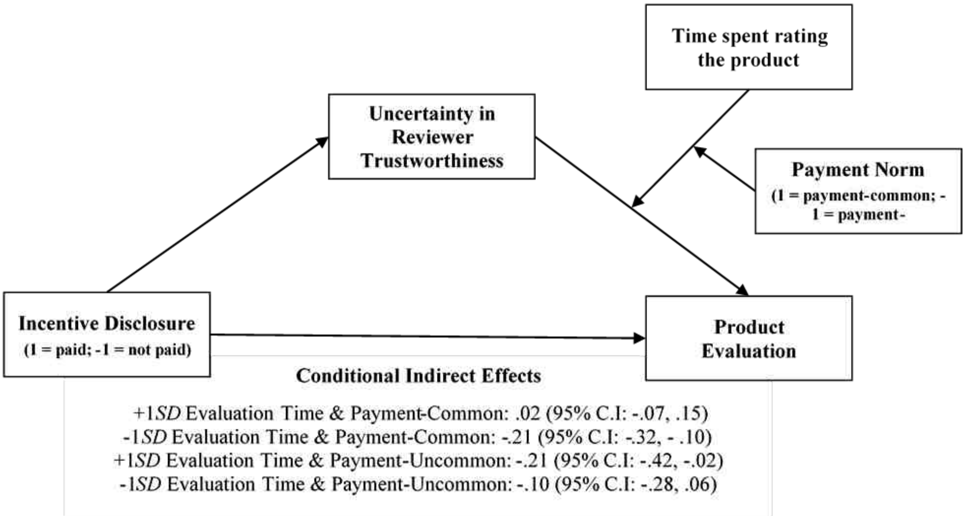
This manipulation was checked using three seven-point Likert-scaled items (1 = “strongly disagree” to 7 = “strongly agree”) at the end of the study intended to measure the extent to which participants believed that online reviews were typically incentivized (α = .85; e.g., “I think that online reviewers are paid to write reviews,” and “I think that paying for online reviews happens more often than one would think”; see Appendix D). Participants in the payment-common condition ($M_{\text{payment-common}} = 5.14$, $SD = 1.00$) and the control condition ($M_{\text{control}} = 4.59$, $SD = 1.35$) believed that payment was more typical than participants in the payment-uncommon condition ($M_{\text{payment-uncommon}} = 4.12$, $SD = 1.35$; $F(1, 167) = 18.87$, $p < .001$ and $F(1, 167) = 3.97$, $p < .05$ respectively). Since participants in the control and payment-common conditions believed that payment was more common than participants in the payment-uncommon condition, we pooled these conditions for the subsequent analysis.

Results

Our objective was to test our hypotheses by estimating the moderated-mediation model shown in figure 6. We expected incentive disclosure to negatively affect product evaluation through increased uncertainty. However, in line with hypothesis 1 and hypothesis 2, the relationship between uncertainty and product evaluation would be moderated by how common participants believed review incentivization practices to be and the time they spent elaborating on uncertainty. Participants who were led to believe that reviewer incentives are common (uncommon) were expected to be more (less) likely to question the relevance of their uncertainty in reviewer trustworthiness, but only when they spend time elaborating.

In summary, following hypothesis 1, participants who elaborated longer and who thought that incentives were common should deem their uncertainty to be incidental and we should not see the incentive backlash effect (i.e., no negative effect of disclosure on product evaluation). However, following hypothesis 2, participants who also elaborated longer but who were told incentive payment is uncommon should deem their uncertainty to be integral and we should see the incentive backlash effect (i.e., a negative effect of disclosure on product evaluation).

Figure 6: Conditional indirect effect of disclosure on product evaluations through uncertainty as a function of payment norm and evaluation time



We tested these predictions using a conditional indirect effects analysis (Hayes 2013, model 18). As predicted, when reviewer incentive payment was common, the indirect effect of disclosure on product evaluation (through uncertainty) was not significant when evaluation time was higher (+1 *SD*; indirect effect = .02, 95% C.I. = [-.07, .15]). It was, however, significant, when evaluation time was lower (-1 *SD*; indirect effect = -.21, 95% C.I. = [-.32, -.10]). Thus, consistent with hypothesis 1, when participants spend more time elaborating on their uncertainty and their uncertainty could be deemed incidental, there was no backlash to disclosure. This was not the case among participants who spent less time elaborating on their uncertainty. Here, heuristic processing dominated and the incentive backlash effect was observed. When reviewer incentive payment was uncommon, the indirect effect of disclosure on product evaluation through uncertainty was negative and significant when evaluation time was higher (+1 *SD*; indirect effect = -.21, 95% C.I. = [-.42, -.02]). It was not, however, significant when evaluation time was lower (-

1 *SD*; indirect effect = -.10, 95% C.I. = [-.28, .06]). Thus, consistent with hypothesis 2, when participants spend more time elaborating on their uncertainty and their uncertainty could be deemed integral, there was a backlash to disclosure. Our predictions therefore hold under higher elaboration. Though the coefficient of the indirect effect under lower elaboration is negative as expected, the indirect effect is not significant. However, consistent with hypothesis 2 this coefficient is larger under higher elaboration than lower elaboration and becomes significant as elaboration increases.

Is this due to a reduction in uncertainty? A key claim of our theory is that elaborating on one's uncertainty in a reviewer's trustworthiness in light of auxiliary information will not change one's level of uncertainty but, through elaboration, can change the extent to which this uncertainty impacts product evaluation judgments. This is an important point, since prior research has focused on attenuating the impact of uncertainty on judgement formation either by resolving (Castano et al. 2008) or reducing (Wichman et al. 2008) it. We instead posit that elaborating on incentive disclosure-induced uncertainty may change the extent to which it impacts judgments and does not necessarily have to resolve or reduce it. This prediction implies that the time participants spend elaborating on their uncertainty and the disclosed payment norm should moderate the uncertainty to product evaluation path and not the disclosure to uncertainty path in the mediation model. We tested this using Hayes (2013, model 11). The results showed a significant main effect of incentive disclosure (b =.64, $t(160)$ = 8.23, p < .001) and evaluation time (b = -.15, $t(160)$ = -1.95, p = .05) on uncertainty in reviewer trustworthiness. Importantly, in support of our theory, no other main or interaction effects were significant (all *ps* > .05). Thus, regardless of the information, incentive disclosure seems to increase uncertainty in reviewer trustworthiness. How this uncertainty is processed—deemed incidental or integral—appears to be what moderates its impact on judgment here, not whether the uncertainty itself can be increased or decreased.

Discussion

In this study we provided support for hypothesis 1 and hypothesis 2 jointly. Specifically, consistent with hypothesis 1, participants who were told that incentivizing reviewers is common practice did not evaluate the reviewed product more negatively when they spent more time evaluating the product. In contrast, consistent with the heuristic processing, those who spent less time evaluating the product did lower their evaluation of the reviewed product. Furthermore, in support of hypothesis 2, participants who were told that incentivizing reviews is uncommon and who spent more time elaborating on this, lowered their product evaluations in response to their uncertainty in the reviewer's trustworthiness that was induced by the incentive-disclosing auxiliary information.

GENERAL DISCUSSION

Although consumer-generated product reviews can play an important role in consumers' product judgments and purchasing decisions, and have been extensively investigated in various ways over the last 10-15 years, research has not considered the influence of review-related auxiliary information on reviewer persuasiveness and, more generally, how consumers process information that can potentially undermine the credibility and persuasiveness of a reviewer. This is surprising given that auxiliary information is common on popular sites (e.g., Amazon) and mobile applications (e.g., Instagram).

To address this gap, we examined the impact of one type of auxiliary information, reviewer incentive disclosure statements, on the persuasiveness of reviews. In contrast to the commonly held belief that incentive disclosure will always have a negative impact on the persuasive power of reviews because it induces uncertainty in reviewer trustworthiness, we showed that this incentive backlash effect depends on whether disclosure-induced uncertainty is deemed integral or incidental to judgment formation when it is elaborated upon.

When elaborating on uncertainty in reviewer trustworthiness makes consumers question its relevance to the judgment at hand, uncertainty becomes incidental to judgment formation and the backlash effect is less likely to occur. We observed this for incentivized reviews of high involvement products (study 1), and when consumers elaborated on the fact that incentive provision is common and normal in practice (study 3). Instead, when elaborating on uncertainty in reviewer trustworthiness reinforces a consumer's uncertainty, it is deemed integral to judgment formation and the incentive backlash effect is more likely to occur. We saw this when consumers elaborated on the fact that the disclosed source of the incentive has a vested interest in a positive review (study 2) and when incentive provision is uncommon and not normal in practice (study 3).

This research makes important contributions to both the attitude certainty and the WOM marketing literatures. First, in contrast to prior work showing that doubting the credibility of a source decreases its persuasiveness, we demonstrate that this does not always need to be the case. Instead, we argue that uncertainty in reviewer trustworthiness can be appraised as either incidental or integral to judgment formation, which determines whether it will influence consumers' judgments. This method for rendering uncertainty irrelevant to decision making is unique, since prior research has focused more on how to resolve or reduce uncertainty (e.g., Castano et al. 2008; Wichman et al. 2008). In none of our studies do we necessarily *resolve* uncertainty in reviewer trustworthiness. We instead simply identify situations under which it is more or less likely to be used as an influential input into consumers' judgments about products.

The second important contribution to the literature is based on the context: incentivized consumer-generated product reviews. This is important in practice since reviewer incentives can generate much debate and ethical concern. The right thing to do, of course, is to be transparent about incentive provisions to reviewers but, unsurprisingly, this can cause a backlash that undermines the power of the reviewers' opinions. As we saw in study 1, this could be particularly problematic since when consumers evaluated Amazon Vine reviews without knowing they were incentivized, they thought those reviews were actually more helpful. Indeed, more generally, offering incentives could improve the quality of reviews. However, consumers' tendency to discount or ignore reviews when incentives are disclosed (i.e., the incentive backlash effect), can result in consumers ignoring these reviews when in fact they should not. Thus, the current research helps us understand why this can occur (uncertainty is integral to judgment formation) and, more importantly, when this may not occur (uncertainty is incidental to judgment formation).

Limitations and Future Research

A key limitation of our studies is that we do not directly measure the extent to which participants perceive their uncertainty in reviewer trustworthiness to be valid or relevant. In order to address this, we used a "moderation-of-process" approach (Spencer, Zanna and Fong 2005) to test our mechanism. We did this by not only illustrating when uncertainty will be deemed incidental (studies 1 and 3), but also by illustrating when it will be deemed integral to judgment formation (studies 2 and 3). Jointly these studies provide a solid test of our theory while simultaneously being high in external validity.

In the present research we have investigated the impact of uncertainty in one's doubt on reviewer persuasion in one specific context: incentivized product reviews. While we have demonstrated the importance of this effect by showing that it, for example, attenuates discounting of incentivized reviews of high involvement products, it is likely that there could be other managerially relevant conditions that may lead to differences in elaboration. For example, future research could consider the channel in which the review is presented. Perhaps consumers may engage in higher elaboration when reading a review than when watching a video review since voice has been shown to facilitate emotion regulation and social bonding (Berger 2014). Furthermore, reviews presented on mobile devices may further lower elaboration since individuals may be more emotionally connected to their phone (Lurie, Ransbotham, and Liu 2013).

Beyond consumer-generated reviews, the effect of elaborating on the relevance of one's uncertainty about an information source could extend to elaboration on uncertainty appraisals of other attitudes. It would be interesting, therefore, to see if our conceptual framework fits in other contexts where people face uncertainty in attitude formation.

In summary, this research extends prior work investigating the impact of UGC on persuasion by considering how auxiliary information—in this case disclosure statements about reviewer incentives—affects consumers' product evaluations. By considering how auxiliary information may influence product review processing, we show that, in contrast to the common belief that uncertainty regarding the trustworthiness of a reviewer will always decrease persuasion, the role of uncertainty in decision-making is more nuanced and depends on whether uncertainty is deemed *integral* or *incidental* to judgment formation.

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APPENDIX A: AMAZON FIELD STUDY REVIEW SELECTION PROCEDURE

To find an equal number of reviews for each review type, we employed a two stage procedure:

Stage 1: Identification of Initial Products

Three product categories were selected a priori that varied on multiple dimensions, including price, usage frequency, purchasing patterns and others. These categories were: Chromebook laptops, robot vacuum cleaners and nutrition bars. Each product was entered into the Amazon search engine and all products with more than 100 reviews were considered. Next, we determined if Vine reviews were available for products with more than 100 reviews. If a review, identified as a "Vine Customer Review of Free Product," was found, we continued to search for 10 reviews of each type. Only if 10 reviews of each type with a helpfulness proportion at the top of the review (our dependent variable) were found was a product included in the sample. Reviews that contained videos were not included in the sample.

Stage 2: Snowball Sampling Using Initial Vine Reviewers

Next, we navigated to the profile of each of the 10 Vine reviewers who wrote a review for the Toshiba Chromebook. The personal page of reviewers contains a history of the products they have reviewed. By examining each product reviewed by the initial 10 reviewers, we were able to identify other products that had at least 10 Vine reviews. We continued searching for reviews until we had a total of 10 different product categories. The final sample of reviews (n = 300) covered 10 product categories (e.g., disposable diapers, small appliances, vitamin supplements, fiction novels, liquid detergents).

APPENDIX B: CONTENT ANALYSIS OF AMAZON REVIEWS, STUDY 1

In addition to rating the helpfulness and usefulness of the reviews, participants also rated the reviews on the characteristics outlined in table B1. For each item, participants rated the extent to which they agreed with the statement (1 = "Strongly Disagree" to 7 = "Strongly Agree").

Table B1: Descriptive statistics from content analysis of Amazon reviews

Item	Review type	N	Mean	(SD)
The review was objective	Not Verified Reviews	1791	4.43	(1.49)
	Verified Purchase Reviews	1779	4.37	(1.48)
	Vine Review	1784	4.84	(1.38)
The review was biased (reversed)	Not Verified Reviews	1791	4.31	(1.59)
	Verified Purchase Reviews	1779	4.43	(1.57)
	Vine Review	1784	4.6	(1.58)
The reviewer had an ulterior motive when writing this review (reversed)	Not Verified Reviews	1791	5.06	(1.48)
	Verified Purchase Reviews	1779	5.15	(1.44)
	Vine Review	1784	5.27	(1.47)
The review was truthful	Not Verified Reviews	1791	5.20	(1.18)
	Verified Purchase Reviews	1779	5.21	(1.12)
	Vine Review	1784	5.48	(1.07)
The review was trustworthy	Not Verified Reviews	1791	4.84	(1.30)
	Verified Purchase Reviews	1779	4.74	(1.34)
	Vine Review	1784	5.22	(1.17)
I am doubtful about the review	Not Verified Reviews	1791	3.00	(1.56)
	Verified Purchase Reviews	1779	2.93	(1.48)
	Vine Review	1784	2.73	(1.48)

Item	Review type	N	Mean	(SD)
The review was positive	Not Verified Reviews	1791	4.11	(2.1)
	Verified Purchase Reviews	1779	4.10	(2.08)
	Vine Review	1784	5.07	(1.69)
The review was favorable	Not Verified Reviews	1791	4.13	(2.05)
	Verified Purchase Reviews	1779	4.12	(2.02)
	Vine Review	1784	5.08	(1.64)
The review expressed a clear opinion	Not Verified Reviews	1791	5.76	(1.17)
	Verified Purchase Reviews	1779	5.45	(1.41)
	Vine Review	1784	5.78	(1.15)
The review was useful	Not Verified Reviews	1791	5.17	(1.44)
	Verified Purchase Reviews	1779	4.85	(1.57)
	Vine Review	1784	5.52	(1.25)
The review was informative	Not Verified Reviews	1791	5.17	(1.42)
	Verified Purchase Reviews	1779	4.66	(1.64)
	Vine Review	1784	5.54	(1.23)
The review was well written	Not Verified Reviews	1791	4.65	(1.63)
	Verified Purchase Reviews	1779	4.11	(1.78)
	Vine Review	1784	5.31	(1.35)
The review was persuasive	Not Verified Reviews	1791	4.78	(1.50)
	Verified Purchase Reviews	1779	4.32	(1.64)
	Vine Review	1784	4.97	(1.39)

APPENDIX C: SUPPLEMENTARY MATERIALS FOR STUDY 2

Product Reviews, Study 2

Positive review

This game is a lot of fun! It is a game of strategy and finesse. Every level is a new challenge to find the proper way to beat the puzzle. Even when you figure out a portion of the puzzle you still have to get the angle and power right every time, which can prove challenging in and of itself. The game is a lot of fun and very addictive. You may tell yourself you're only going to play one more level, but five levels later you're still going strong.

Negative review

This game is boring and confusing. I didn't really get it. I thought the point was to pop balloons, but I didn't get any points for popping them! I played several times and still have no idea how the scoring works, or how to advance levels. Seeing as the instructions were confusing, the game wasn't fun to play. I suppose if I had some rules or instructions that might help.

Disclosure Manipulation, Study 2

Silent: nothing was said about incentive payment

Paid by non-profit: For full disclosure you should be aware that the reviewer was paid by a non-profit organization to write this review. This non-profit organization encourages consumers to write reviews and post them online in order to make it easier for consumers to find products that they will like.

Paid by game maker: For full disclosure you should be aware that the reviewer was paid by the game's software developer to write this review.

Paid by game seller: For full disclosure you should be aware that the reviewer was paid by a major online store, where the game can be purchased as an app, to write this review.

Disclosure Manipulation Pre-test

One-hundred and forty-five participants from Amazon Mechanical Turk were recruited to determine if our disclosure manipulation operated as intended. Specifically, we wanted to confirm that participants would infer that the game seller and game manufacturer of an online game have a vested interest in receiving positive reviews. Based on our a prior exclusion criteria, six participants were excluded from all analyses. The remaining 139 participants were randomly allocated to conditions in a 2(review valence: positive, negative) x 3(incentive source: paid by non-profit, paid by game maker, paid by game seller) between-subjects design. We measured the degree to which the payment source was perceived to benefit from a positive review (our manipulation check) with five seven-point Likert-scaled items ($\alpha = .98$; 1 = "strongly disagree" to 7 = "strongly agree;" e.g., "benefit from a positive review" and "profit from a positive review"). Analyses confirmed that the disclosure manipulation operated as intended. Participants reported that the game maker ($M_{\text{game maker}}$ = 5.98, SD = 1.39) and game seller ($M_{\text{game seller}}$ = 5.95, SD = 1.30) were both more likely to "benefit" from a positive review than the third party non-profit ($M_{\text{non-profit}}$ = 4.53, SD = 1.69). Planned contrasts confirmed that participants believe that, compared to the third party non-profit, the game maker ($F(1, 138) = 21.82$, $p < .001$) and game seller ($F(1, 138) = 21.08$, $p < .001$) were more likely to benefit from a positive review. The difference in perceived benefit between the game seller and game maker was not significant ($F(1,138) = .01$, $p = .91$).

Measurement Items, Study 2

Doubt in reviewer trustworthiness (a = .88)

1. I am doubtful about the reviewer's intentions
2. I am uncertain of the reviewer's intentions
3. I am confused about the reviewer's intentions
4. The review was biased
5. The reviewer had an ulterior motive

Each item was measured on a 7-point Likert scale ranging from 1 (strongly disagree) to 7 (strongly agree).

APPENDIX D: STIMULI AND SUPPLEMENTARY ANALYSES FOR STUDY 3

Product Description, Study 3

The product you will consider in this research is the current model Samsung Chromebook (3G, 11.6-inch screen) that runs Google Chrome OS. Here's the description of this product provided by Samsung and Google:

The Chromebook is for everyone. The Samsung Chromebook is a new computer that helps you get everyday things done faster and easier. It starts in seconds, has virus protection built-in, and runs your favorite Google apps plus thousands more. The Chromebook comes with leading Google products, like Search, Gmail, YouTube and Hangouts, so you can work, play, and do whatever you want, right out of the box. And it's simple to use. There's no setup, and your files are automatically backed up in the cloud. At just 2.4 pounds, 0.7 inches thin, and with over 6.5 hours of battery life, the Samsung Chromebook can go anywhere you go. It's built to stay cool, so it doesn't need a fan and runs silently. It also includes 100GB of free Google Drive storage (for 2 years), a built-in webcam, dual band Wi-Fi, and 3G mobile internet to make it easy to connect to the cloud anywhere you go.

Product Review and Manipulations of Market Norms and Disclosure

Norm manipulation: payment common

Before reading the review, we would like to inform you about something you may be interested to know about. According to a recent study conducted by the Pew Research

Center, 78% of companies do attempt to solicit online reviews by offering people incentives of some kind. The study also found that 88% of consumers believe that companies “on a regular basis pay or incentivize people to post product reviews on websites”.

Norm manipulation: payment uncommon

Before reading the review, we would like to inform you about something you may be interested to know about. According to a recent study conducted by the Pew Research Center, 78% of companies do not attempt to solicit online reviews by offering people incentives of some kind. The study also found that 12% of consumers believe that companies “on a regular basis pay or incentivize people to post product reviews on websites”.

Product Review

The Chromebook is a great little laptop. I use it a lot for playing online games, watching movies on Netflix, and checking stuff out on YouTube. Not to mention Facebook :) It does everything I need in a basic laptop. It isn't super powerful but you don't expect something like this to replace a more powerful machine (if that's what you need). Rather, this is something you take with you when you're on the go, like traveling. That said, it does well with video streaming (netflix, hulu, etc) and even things like Google hangouts (video chats). It suits me better than a tablet because it has a keyboard. The Chrome operating system is also pretty easy to use. The one thing to know is that it works best if you already live inside the Google “ecosystem” meaning that you use gmail, etc regularly. If you're a google person this is great. Otherwise it isn't so awesome.

Disclosure manipulation

Disclosure: the reviewer was [paid by Samsung] [not paid] to write this review.

Measurement Items, Study 3

Perceived prevalence of reviewer incentivization (a = .85)

1. I think that online reviewers are often paid to write reviews
2. I think that it is common practice for companies to pay people to write reviews and post them online
3. I think that paying for online reviews happens more often than one would think
4. I think that an online review being paid for is more likely the norm than an exception
5. I think that many online reviews on major websites are paid for

Doubt in reviewer trustworthiness (a = .95)

1. The review made me doubtful of the reviewer's intentions
2. The review was written by a reviewer who had an ulterior motive
3. The reviewer was telling the truth about this product (R)
4. The review was biased
5. The review was objective (R)
6. The review was truthful (R)
7. The review was trustworthy (R)

Each item was measured on a 7-point Likert scale ranging from 1 (strongly disagree) to 7 (strongly agree).

CHAPTER 4

PREOCCUPIED WITH THE POWERFUL:

A QUANTITATIVE REVIEW OF EXPERIMENTAL DESIGNS, ATTRIBUTION OF RESULTS, AND EFFECT SIZES IN SOCIAL POWER RESEACH

Over the past fifteen years, a considerable interest in understanding the consequences of social power, defined as individuals' asymmetric control over valuable resources including time, money, or rights (Emerson 1962; Fiske 2010; Keltner, Gruenfeld, and Anderson 2003; Magee and Smith 2013), has developed. Among other things, studies have found that being powerful leads individuals to form superficial social perceptions (Fiske 1993; Galinsky et al. 2006), engage in approach-related behavior (Anderson and Berdahl 2002; Galinsky, Gruenfeld, and Magee 2003), objectify themselves and others (Gruenfeld et al. 2008; Inesi, Lee, and Rios 2014), perform well on complex decision problems (Smith, Dijksterhuis, and Wigboldus 2008), overestimate their own height and underestimate others' height (Duguid and Goncalo 2012; Yap, Mason, and Ames 2013), and report greater well-being (Kifer et al. 2013).

Despite the large number of published studies and the myriad of theories that resulted from these efforts, we argue that the way social power has been studied, has introduced a bias in the literature. Specifically, we highlight the pervasive convention that power*fulness* is generally assumed to be the driving causal force behind power's far-reaching effects. This convention has historical, philosophical, and psychological roots. For example, it was Lord Acton who, in a critical letter to Mandell Creighton, first noted that "power tends to corrupt." Acton's assertion has inspired some of the most prolific social psychological experiments (e.g., Kipnis 1972; Zimbardo 1973) and since sparked a great deal of scientific interest in how those in positions of power tend to behave (DeCelles et al. 2012; Inesi, Gruenfeld, and Galinsky 2012; Malhotra and Gino 2012; Overbeck and Park 2001; Pitesa and Thau 2013). In addition, the actions and decisions of those in a position of power often have considerable consequences for others who are dependent on them (Emerson 1962; Kelley and Thibaut 1978), and so perhaps understanding powerfulness deserves greater scientific emphasis. A final factor that may contribute to a focus on powerfulness is that those in positions of power are often more visible and people hold great expectations for the few at the top (Rucker, Hu, and Galinsky 2014). Thus, a finding that specifically confirms, qualifies, or challenges these expectations is likely to spark the interest of internal and external audiences.

Absent in this research, however, seems to be the role of powerlessness. As a consequence, theoretical inferences for the powerless are often only *inferred* from what we know about powerfulness, which can be appreciated by the fact that many, if not all, popular theories of social power offer dichotomous (and opposing) predictions for high and low power (e.g., Guinote 2007; Keltner et al. 2003; Magee and Smith 2013). If the powerful exhibit heightened (reduced) levels of a certain behavior, the powerless must exhibit lower (higher) levels of that behavior. Yet, systematically studying the psychology of powerlessness is important for many reasons. First, most social hierarchies take the shape of a pyramid where there are relatively few powerful at the apex and a large number of powerless at the base (Magee and Galinsky 2008). For example, the Catholic Church has one pope, few bishops, but many laities. Similarly, an organization generally has one CEO, but many employees who do not have a subordinate. Even chimpanzees exhibit pyramid-shaped social structures where lower-ranking members subordinate themselves to a single dominant alpha male (de Waal 2007). Focusing on powerfulness alone facilitates the development of theories that explain the behavior of a few but remain silent about the majority of individuals in a social hierarchy.

Second, the experience of power tends to be socially embedded. Power and dependence go hand in hand. For example, actor A has power over an actor B when actor B is dependent on actor A (Emerson 1962). Similarly, having control over resources only leads to power when these resources are valued by others (Magee and Galinsky 2008). Thus, it makes intuitive sense that powerfulness can only be understood in a meaningful way when it is jointly considered with powerlessness. Third, even if one accepts the premise that those at the top of an organizational hierarchy are relatively more theoretically relevant than those at the bottom, understanding the effects of powerlessness still has its merits. The psychological experience of power has a diverse set of conscious and nonconscious antecedents and can be triggered by structural, experiential, and physical forces alike (French and Raven 1959; Galinsky, Rucker, and Magee 2015). These different bases of power often operate independently of each other, leading to powerfulness being experienced in some contexts and powerlessness being experienced in others. For example, although individuals may be high in structural power, they could still experience a subjective sense of powerlessness, and vice versa (Anderson and Galinsky 2006; Galinsky et al. 2015; Tost 2015).

Although there are clear reasons for studying powerfulness, we believe that better understanding the psychology of powerlessness is fundamental to building more thorough and meaningful theories explaining the psychology of power. However, at present it is unclear if we can draw conclusions regarding the antecedents, correlates, and consequences of powerlessness from the extant research since we do not know how treating powerfulness as the driving causal force influences the way power has been operationalized in the past and the theoretical inferences that can be drawn as a result. To gain insight into this question, we examine quantitatively how experimental research on power is designed in psychological, organizational, and marketing research. The goal is to illuminate whether the cumulative body of power research allows for nuanced conclusions about whether and how psychological states of powerfulness *and* powerlessness impact cognition, emotion, motivation, and behavior. We do this by examining which comparison groups powerfulness has been contrasted against (part I). Some studies use 3-cell designs in which both low power and control groups are contrasted to high power. More frequently, though, high power is studied in 2-cell designs where it is compared to either a low power *or* a control group – both of which can lead to a theoretical void for the psychology of powerlessness. Next, we explore the implications of only comparing high to low power (omitting the control condition; part II) and of comparing high power to a control condition (omitting the low power condition; part III) for substantive inferences across the continuum of social power. We discuss the advantages of 3-cell designs and how 2-cell designs tend to limit the inferences that can be drawn—particularly about individuals who are powerless—and can potentially lead to an overestimation of the high power effect. All materials, data, and syntax reported in this paper can be accessed at https://osf.io/fbmwz/?view_only=483af598ef4f4080a6e60eed4218da44.

By conducting a quantitative survey, a content analysis, and a meta-analysis, we contribute to the power literature by highlighting how the recent scientific tradition to interpret powerfulness as the driving causal force limits our knowledge about the psychology of the powerless. Our results suggest that social power could benefit from a more thorough examination of powerlessness and we discuss innovative methodological approaches that could facilitate more nuanced theoretical inferences in future research.

Second, our systematic examination of study designs in a well-established literature extends the current discussion on how to increase methodological rigor in the behavioral sciences (Ioannidis 2008; Medin 2012; OSF 2015; Simmons, Nelson, and Simonsohn 2011) by illuminating how the use of 3-cell study designs can strengthen construct validity and lead to more conservative effect sizes. Finally, we provide a blueprint for how to quantitatively review theoretical and methodological conventions and the accompanying challenges commonly encountered in experimental research. Our approach of using experimental designs as an independent variable in a meta-analysis not only allows researchers to systematically take stock of what is known, but also to identify theoretical gaps and exciting avenues for future research in other areas of psychology, organizational behavior, and marketing.

I. HETEROGENEITY OF STUDY DESIGNS IN SOCIAL POWER RESEARCH

We start our investigation by distinguishing between two common types of study designs in social power research: 3-cell and 2-cell designs. Studies with a *3-cell design* generally include a high power, low power, and control condition. Studies that exclude any one of these conditions use a *2-cell design.* There are a number of important advantages that come with the use of 3-cell study designs. First, the presence of a control condition, in which researchers either manipulate an average level of power, an experience unrelated to power, or use no manipulation at all (Bailey 2008), is important to interpret the direction of effects. Social power research often makes pointed inferences about the directionality of an effect and attributes changes in the level of a dependent variable to powerfulness (e.g., "The powerful size others down […]", Yap, Mason, and Ames 2013, 591) or sometimes to powerlessness (e.g. "[…] low power increased consumers' willingness to pay […]", Rucker and Galinsky 2008, 257). For example, imagine a study that tests the impact of power on bullying. If this study were to show that bullying is higher in a high power condition than a low power condition (i.e., high power > low power), one could not reliably determine whether high power increased bullying, low power decreased bullying, or both. Including a control condition, on the other hand, provides more confidence in whether powerfulness (i.e., high power > control = low power), or powerlessness (i.e., high power = control > low power) is leading to an observed movement of a dependent variable. In the absence of a control condition, however, such inferences are difficult because it is unclear which treatment condition is the driving force behind an effect.

Second, including both a high and low power condition in addition to a control condition allows for more nuanced insights into the exact shape of a causal relationship between social power and a dependent measure. Not including low power, for example, limits inferences to powerfulness and makes conclusions about powerlessness challenging. Although popular theories of social power often dichotomize predictions into high and low power (e.g., Guinote 2007; Keltner et al. 2003), it is not always clear that the low power condition is a linear extension of inferences made based on a comparison of a high power and control condition (i.e., high power > control > low power; see Magee and Smith 2013). It is possible that other patterns may be observed (e.g., high power = low power > control) that would lead to more nuanced, or different, conclusions about the causal relationship between power and a particular dependent variable. Research designs with three cells also provide more confidence in whether an effect is driven by power as opposed to related, but conceptually distinct, constructs. Power is a multifaceted phenomenon (French and Raven 1959; Galinsky, Rucker, and Magee 2015) and, although power is generally defined as control over valued resources, it can manifest in many different forms. It is possible, for example, that researchers unintentionally manipulate other constructs that bolster the power effect (see Tost 2015). It is more likely that this would become apparent if experimental designs with three cells were tested.

Studying powerfulness: What is the comparison group?

To gauge the relative frequency of study designs in the power literature and to examine what comparison groups high power is usually contrasted against, we conducted an extensive literature search to retrieve relevant published studies in which social power served as the independent variable. First, we searched major academic databases (e.g., PsycINFO, Google Scholar) for articles published in a pre-determined list of 18 leading journals in psychology (e.g., *Journal of Personality of Social Psychology, Psychological Science*), organizational behavior (e.g., *Academy of Management Journal, Organizational Behavior and Human Decision Processes*), and marketing (e.g., *Journal of Consumer Research*; see appendix A for a list). We used the following search term: *power* OR dependence OR status OR hierarch* OR control*. The retrieval was conducted on February 2, 2015 and included all articles available on or prior to that date. Second, we hand-searched the reference sections of ten literature reviews on social power (e.g., Anderson and Brion 2014; Hirsh, Galinsky, and Zhong 2011; Magee and Galinsky, 2008, see appendix B for a comprehensive list) to identify additional articles. Third, we searched the abstracts of the considered publication outlets to make sure we did not miss any relevant papers. Fourth, we searched the websites and publication lists of all authors in our sample to identify “in press” manuscripts.

Eligible studies were selected based on pre-determined criteria. First, we included all studies that experimentally manipulated social power using random assignment. Not considered were studies in which power was measured or was not the primary independent variable. Second, we included all manipulations of power as long as a) they allowed researchers to manipulate high power, low power, and include a control condition (e.g. there is no low power equivalent to a high power manipulation involving making a fist; see Schubert and Koole 2009), and b) each prime type manipulated high power, low power, or a control condition at least once in our sample. Third, the different power conditions had to be manipulated independently of each other (e.g., not within-dyad). Finally, the articles had to be written in English and contain sufficient statistics to calculate effect sizes for our meta-analysis (see part III). This procedure yielded 293 studies published in 113 articles (see appendix C for an overview and appendix I for a complete list of references).

Observation #1: Powerfulness tends is studied using 2-cell designs with a low power or a control conditions serving as the comparison group

Table 1 summarizes the frequency with which the different power conditions occur. While 99% of studies included a high power condition, fewer included a low power condition (84%), and only a third included a control condition (34%). This pattern suggests greater scientific focus on the powerful.

Importantly, only 17% of studies had an experimental design with three cells (high power, low power, *and* control conditions). The majority of studies (83%) had experimental designs with two cells. Sixty-six percent of studies included a high and low power condition, 16% included a high power and control condition, and 1% compared a low power to a control condition.

One could argue that the use of 2-cell designs is less problematic when they are combined with other studies using 3-cell designs in the same paper. Thus, we also analyzed the relative frequency of study combinations at the paper level. When the studies in our sample were grouped into their respective papers, only 26% of the 113 papers included at least one study with a 3-cell design. The majority (69%) of papers used 2-cell designs. Only 5% of papers used 3-cell designs exclusively.

Table 1: Relative frequency of power conditions, study designs, and study combinations across our sample.

	Count	*Percentage*
Total studies in sample	293	100%
Condition frequency		
High power condition	290	99%
Low power condition	246	84%
Control condition	101	34%
Study design frequency		
3-cell study designs	51	17%
2-cell study designs	242	83%
High power and low power	192	66%
High power and control	47	16%
Control and low power	3	1%
Total papers in sample	113	100%
Study combination frequency		
Only 2-cell designs	78	69%
At least one study with a 3-cell design	29	26%
All studies with 3-cell designs	6	5%

Consequently, in four out of five studies and two out of three papers published in peer-reviewed journals, theoretical inferences that could be made about powerlessness may be limited due to the pervasiveness of 2-cell designs. Next, we discuss the theoretical and methodological challenges that can arise from omitting control conditions (part II) and omitting a treatment condition (e.g., low power; part III).

II. POWERFULNESS OR POWERLESSNESS: WHICH IS THE DOMINANT CAUSAL FORCE?

The use of appropriate control conditions is both theoretically and empirically important. Control groups are needed to generalize across the continuum of power. A *control group* refers to an experimental group that receives a neutral treatment or no treatment at all (Shadish, Cook, and Campbell 2002). Although comparing two treatment groups (e.g., high power to low power) allows researchers to conclude that *an* effect exists, it remains unclear which treatment was responsible for the observed effect. Only a control group allows for a counterfactual inference about what would have happened in the absence of any treatment.

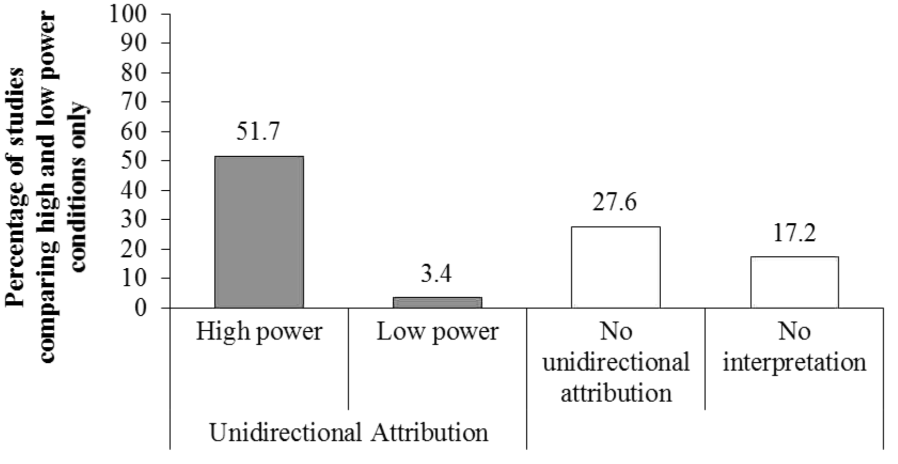
Consider a study by Yap and colleagues (2013), which concluded that "[power manipulated through] expansive posture […] can lead to dishonest behavior" (Yap et al. 2013, 2285). This research was based on a series of studies comparing the relative levels of dishonest behavior between high and low power conditions. Study 1 found that participants assuming an expansive posture (high power) were more likely to steal money than participants assuming a contractive pose (low power). Although the powerful showed more dishonest behavior, it remains unclear whether powerlessness *reduced* unethical behavior, or whether both high *and* low power affected behavior. Thus, examining the effects of power without appropriate control conditions can limit the interpretive value of findings.

Observation #2: Effects lacking a control condition tend to be attributed to high power

Although two thirds of the studies did compare high against low power, we do not know whether researchers draw unidirectional inferences about the power effect (i.e., attributing the significant difference in between-group means to either the high or low power group) when no such conclusion is possible. To examine how the results of studies comparing high and low power are interpreted, we content-analyzed the study discussion sections of a randomly selected subset of studies in our sample (30%, 58 studies) that lacked a control condition. Two of the authors categorized how the effects were interpreted. For each study, the coders decided whether the effect of power on the dependent variable was primarily attributed to high power (e.g., "[…] elevated power facilitates goal-directed motor performance") or low power (e.g., "[…] powerless individuals were unable to ignore information that was irrelevant to the task at hand"), or whether no unidirectional attribution was made (e.g., "Participants primed with high power were more inclined to negotiate the value of a consumer good […] than were participants primed with low power"), or whether the study discussion did not involve an explicit interpretation of the results. Reliability between the two coders was high (κ=.92) and discrepancies were resolved through discussion.

Figure 1 reveals that of the 58 studies content-analyzed, thirty-two studies (55%) made unidirectional attributions; in 30 of these studies (52%), it was concluded that the effect of power was primarily driven by high power. In other words, powerfulness tends to be favored as the dominant causal driver when interpreting study results. In two of the studies (3%) it was concluded that the power effect was driven by low power. Only 28% (16 out of 58) of all studies made no explicit, unidirectional attribution (i.e., did not attribute the effect to high or low power alone). The remaining 17% (10 out of 58) of the study discussions did not interpret the results (e.g., the next study was introduced).

Figure 1: Relative attribution of effects produced by studies comparing high and low power



To rule out coder expectancy effects, the 58 study discussions were also each coded by 10 individuals on MTurk (see appendix D for details). The same results emerged. On average, more than half of the discussions were categorized as making unidirectional attributions, the majority of which were in favor of the powerful (42% to high power; 13% to low power). In addition, coders also indicated how the results were attributed using a continuous scale (1=*primarily to being powerless*; 4=*equal / unclear attribution*; 7=*primarily to being powerful*; ICC = .86). Again, studies tended to be interpreted in favor of the powerful, with a mean rating (M = 4.71, SD = 1.08) significantly above the midpoint of the scale, $t(57) = 5.00$, $p < .001$, $d = .96$.

Thus, although it is widely accepted that not including a control condition makes interpretation of effect directionality difficult, we found that a) more than half of the study conclusions made unidirectional statements about the effects of power when in fact no such conclusion was possible, and b) the vast majority of the directional attributions focused on the implications of powerfulness. Such inference practices have the potential to flood the field with apparently novel, but disjointed findings on powerfulness since some of the effects attributed to the high power condition may in fact be driven by the low power condition. In addition, these practices may create a theoretical void regarding the effects of low power since effects are, more often than not, attributed to powerfulness.

III. HIGH POWER VERSUS CONTROL DESIGNS

While some studies contrast powerfulness with powerlessness in the absence of a control condition, other studies include a control condition but lack a low power condition. Indeed, 49 of the studies in our sample did not include both power conditions (see table 1). Of those, an overwhelming 96% omitted low power. This further reinforces the idea that powerfulness tends to be seen as the causal force and the literature has thus focused primarily on understanding the psychology of powerfulness at the expense of powerlessness. One consequence of this is that theoretical inferences about the powerless are often only *inferred* from knowledge of powerfulness (e.g. Guinote 2007; Keltner et al. 2003; Magee and Smith 2013): For example, one study hypothesized that accountability moderates the effect of power on selfishness (Pitesa and Thau 2013). A conclusion of this research was that “power leads to more self-serving decisions under moral hazard” (Pietsa and Thau 2013, 555). This statement suggests that those individuals who are powerless must be making *fewer* self-serving decisions than others. However, such a conclusion is premature given it is based on studies contrasting a high power and control condition.

Sometimes even explicit conclusions about the linearity of power's effects are made. Consider the studies by Mourali and Yang (2013), which examined the role of power in consumers' resistance to influence (studies 2–4). Here, half of the participants were asked to recall a situation in which they had power over someone else (high power group) and the other half wrote about their previous day (control group). Although these studies did not include a low power condition, explicit conclusions for the powerless were drawn (e.g. "Consumers with low power still conformed to the opinions of their peers"; Mourali and Yang 2013, 551). However, assuming that the effects of power follow a linear pattern from high to low power is not always appropriate and may be problematic. Indeed, there are instances in which the linearity assumption breaks down and the outcomes for the powerless cannot be readily implied from studying those with relatively more power (e.g. Handgraaf et al. 2008; Schaerer, Swaab, and Galinsky 2015).

An illustrative example

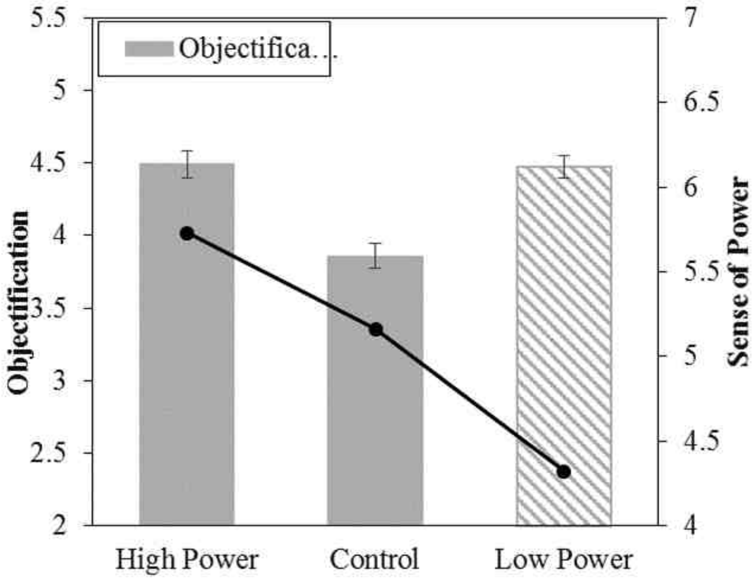
To illustrate the benefits of 3-cell designs and demonstrate how an empirical focus on high power using a 2-cell design can lead to different theoretical conclusions, we conducted an illustrative experiment examining the effect of social power on objectification (seeing others as a means to an end; see Fredrickson and Roberts 1997). Prior research has investigated this effect and found that being powerful is associated with increased levels of objectification of social targets (Bargh et al. 1995; Gruenfeld et al. 2008; Inesi et al. 2012; Kipnis 1972; Zimbardo 1973). We ran an experiment (N = 259; see appendix E for exact sample, procedure, and results) using an established power manipulation and an established measure of objectification (Gruenfeld et al. 2008, study 1a). Our experiment involved participants describing a professional relationship either with a subordinate (high power condition), a peer (control condition), or a supervisor (low power condition), the subsequent completion of an eight-item scale measuring objectification tendencies (e.g., "This relationship is important to me because it helps me accomplish my goal"), and a manipulation check gauging participants' sense of power (Anderson, John, and Keltner 2012). As expected, participants in the high power condition felt more powerful ($M = 5.73$, $SD = 1.01$) than participants in the control condition ($M = 5.16$, $SD = 0.76$), $F(1,258) = 12.98$, $p < .001$, $d = .65$. Accordingly, participants in the high power condition were more likely to objectify their work partner ($M = 4.49$, $SD = .86$) than participants in the control condition ($M = 3.86$, $SD = .88$), $F(1,258) = 25.10$, $p < .001$, $d = .72$.

Given these results, a 2-cell study contrasting high power and a control condition would likely conclude that objectification increases with power and assume that the powerless should thus be *less* likely to objectify others. To test whether a 3-cell study could lead to a different conclusion, we compared the high power and control conditions to the low power condition. Strikingly, we found that the powerless exhibited the same behavioral tendencies as those in the powerful condition (see figure 2).

Participants in the low power condition ($M = 4.47$, $SD = .70$) were significantly *more* likely to objectify than those in the control condition, $F(1,258) = 23.70$, $p < .001$, $d = .76$, and equally likely to objectify when compared to those in the high power condition, $F(1,258) = .02$, $p = .88$, $d = -.03$. This occurred despite the fact that participants in the low power condition felt the *least powerful* of all conditions ($M = 4.32$, $SD = 1.36$; all Fs >

28.08; *p*s < .001; all *d*s > -.70). The current experiment suggests that objectification is especially likely to occur in hierarchical relationships (e.g., between a supervisor and a subordinate). But it is important to note that power has also been shown to affect generalized objectification; Gruenfeld and colleagues (2008; study 1b) found that participants who wrote about a time they had power over someone else reported a higher tendency to objectify an unrelated social target relative to those who wrote about a time someone else had power over them.

Figure 2: Illustration demonstrating that the inclusion of a low power condition can lead to different theoretical inferences



Why theoretical inferences for powerlessness do not necessarily follow from powerfulness

This experiment illustrates that focusing research efforts exclusively on high power may lead to substantively different theoretical inferences and thus can pose challenges to the construct validity of power. Construct validity refers to the extent to which an operationalization represents a concept it is supposed to represent and is a fundamental aspect of methodological thinking in psychology (Campbell and Cook 1979; Cronbach and Meehl 1955). There are at least three reasons why only comparing high power to a control condition alone may limit our understanding of powerlessness (and thus power more generally). We discuss two theoretical considerations (i.e., curvilinear effects, construct complexity) and one methodological consideration (i.e., confounded manipulations).

The effects of power may be curvilinear. One possibility is that the effects of power are curvilinear. For example, a plethora of research in social psychology suggests that powerfulness tends to lead to approach-related behavior such as moving an annoying fan, taking a risk, or making the first offer in negotiations (e.g., Anderson and Galinsky 2006; Fast et al. 2011; Galinsky et al. 2003; Magee, Galinsky, and Gruenfeld 2007). Yet, studies in consumer research report similar tendencies for the opposite side of the continuum, where powerless individuals tend to engage in proactive attempts to return to a desired status quo (Rucker and Galinsky 2008). Related patterns have also been documented in the status literature where curvilinear effects have been found for conformity (Phillips and Zuckerman 2001) and creativity (Duguid and Goncalo 2015).

Given that status and power are often inextricably intertwined phenomena (Fiske 2010; Magee and Galinsky 2008), it would be reasonable to expect similar patterns for power.

The power construct may be more complex than assumed. While our results may suggest that powerlessness can also lead to objectification, is it possible that the mechanism is different from the impact of powerfulness on objectification. This would suggest something theoretically unique about the psychology of powerlessness, that it is not simply the opposite of powerfulness. This could be the case when a treatment is described as a simple construct when it in fact reflects a more complex one or consists of multiple constructs. In reality, power has many different sources, such as financial resources, structural positions of authority, respect from others, and knowledge (French and Raven 1959). These facets often interact to influence individuals' actions. For example, an individual can feel subjectively powerless despite having a great deal of structural power (Anderson and Galinsky 2006; Galinsky et al. 2015; Tost 2015). In addition, individuals tend to be part of complex dependence relations in which they simultaneously control others and are controlled by others (Anderson and Brion 2014). Thus, the psychology of powerlessness may not simply be the opposite of powerfulness, and could be thought of as a distinct psychological construct with its effects occurring orthogonally to powerfulness.

The power manipulation may be confounded. A third reason one would observe unexpected effects for low power could be confounds residing within a power manipulation. Experimental manipulations in psychology are rarely pure representations of the constructs they intend to represent, leading researchers to sometimes unintentionally manipulate a related but distinct additional construct (Shadish et al. 2002). It is therefore possible that writing about a situation of powerfulness could simultaneously activate constructs that would also be activated by recalling situations of powerlessness. For example, encouraging participants to think about their relationship with their superior or with their subordinate may activate an exchange-oriented mindset (Clark and Mills 1979), while thinking about a peer may activate a communal orientation. In addition, the exposure to power-related words (e.g., power, authority, executive) has been shown to subliminally activate power (Bargh et al. 1995; Smith and Trope 2006). Such words are often also part of low power manipulations (e.g., "Recall a particular incident in which someone else had *power* over you"). In sum, only examining a subset of the high power–control–low power continuum may reduce detection of more complex causal relationships and confounding variables.

Implications for the effect size of social power

In addition to imprecise theoretical inferences, we also argue that omitting a treatment condition (e.g., low power) can lead to an overestimation of the power effect (e.g., high power). This could occur for several reasons. It is possible that conditions (e.g., low power) that do not support a researcher's hypothesis (e.g., the powerful cheat) are less likely to be reported in the first place. This argument has been extensively discussed elsewhere (Simmons et al. 2011). However, we argue that even when researchers engage in rigorous scientific analysis, the a priori omission of low power conditions can lead to effect size inflation. Consider again our example of objectification: including a low power condition a priori may have led to the realization that the actual net effect of power is smaller than based on comparing the high power and control condition (d = .72). Indeed, Ioannidis (2008) notes that effect size inflation can occur at the interpretation stage of the research process. Confounds, for example, can bolster an observed effect of a manipulation by contributing to the observed variance in the dependent variable (Fern and Monroe 1996; Kriebel et al. 2004). As a result, the estimated effect size based on the comparison of a high power to control condition would be inflated. Another reason for why not including a low power condition could lead to an overestimation of the high power effect is because patterns in which *powerlessness* is the generative force and the high power effect naturally small (e.g., HP = C < LP) would go undetected. In the absence of a low power condition, a null effect of high power (e.g., HP = C) would naturally lead to an abandonment of the high-power hypothesis.

To examine whether such effect size patterns can be observed in the published literature, we conducted a meta-analysis to test whether the effect sizes produced by the 293 studies in our sample would vary as a function of the included experimental conditions. This allows us to test whether particular combinations of power manipulations result in smaller or larger effect sizes.

The independent variable in our meta-analysis model was whether a particular study included a high power condition, a low power condition, and/or a control condition. Because we were agnostic to the theoretical valence of the power consequences, the absolute effect size value was used. In addition, we were interested in the effects of power independent of other contextual influences and calculated the average main effect of power when study designs included other factors (see appendix F for detailed meta-analysis procedures and appendix G for robustness checks). We computed the standardized difference between two means (Cohen's *d*) based on means and standard deviations (or standard errors), F-tests, t-tests, chi-square tests, proportions, correlations, or *p*-values (Lipsey and Wilson 2001). For each study, we considered the effects of all key dependent variables for which predictions were made, excluding mediators or exploratory dependent measures.

Table 2: Descriptive statistics of distribution and effect sizes by control variable.

	k	*m*	*d*	*95% CI*	T^2
Manipulation technique					
Structural manipulation	270	94	0.49***	[0.43; 0.55]	0.19
Experiential manipulation	394	164	0.43***	[0.39; 0.48]	0.12
Conceptual manipulation	64	22	0.51***	[0.36; 0.65]	0.51
Physical manipulation	42	19	0.57***	[0.42; 0.72]	0.05
Dependent measure					
Cognition	91	28	0.44***	[0.32; 0.55]	0.10
Self-perception	63	35	0.55***	[0.45; 0.65]	0.12
Social perception	177	66	0.48***	[0.40; 0.55]	0.10
Resistance to influence	50	24	0.51***	[0.33; 0.70]	0.44
Performance and behavior	219	88	0.47***	[0.42; 0.53]	0.09
Motivation and emotion	151	62	0.43***	[0.37; 0.50]	0.10
Physiological	11	7	0.48**	[0.19; 0.77]	0.05
Experimental design					
Manipulation check included	339	134	0.46***	[0.41; 0.50]	0.08
Manipulation check not included	431	159	0.47***	[0.42; 0.52]	0.22
Moderator included	418	143	0.32***	[0.27; 0.36]	0.21
Moderator not included	352	150	0.59***	[0.55; 0.63]	0.03
Mediator included	91	46	0.45***	[0.39; 0.51]	0.07
Mediator not included	679	249	0.47***	[0.43; 0.51]	0.14
Experimental setting					
US sample	342	133	0.47***	[0.41; 0.52]	0.21
Non-US sample	242	86	0.45***	[0.39; 0.51]	0.08
Geography not specified	186	74	0.49***	[0.42; 0.56]	0.15
Lab	606	221	0.48***	[0.44; 0.53]	0.14
Online	115	55	0.38***	[0.31; 0.44]	0.10
Other (e.g., field)	49	17	0.54***	[0.41; 0.66]	0.08

Note: CI = confidence interval, k = number of independent effect sizes, m = number of studies.
*** $p < .001$, ** $p < .01$, * $p < .05$

We included a number of control variables in our meta-analysis to ensure that any effects we find are not driven by other design choices available to researchers. The included control variables were: manipulation type, dependent measure type, experimental design (manipulation checks, mediators, moderators), and experimental setting (subject nationality, location, collection method; see appendix F for exact variable descriptions). Coding decisions were made by two authors (κs > .89) and discrepancies were resolved through discussion. Table 2 provides a descriptive summary of the control variables.

Observation #3: Effect size of "high power versus control" designs are likely inflated

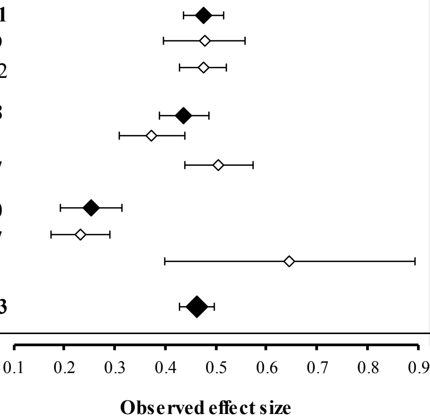
We tested whether the effects of power may be larger in the absence of a power condition. Indeed, a meta-regression revealed that the effect size of studies with 2-cell designs (i.e., those comparing either high *or* low power to a control condition) was significantly larger (d = .52, SE = .03, m = 50, k = 99) than the effect size of studies using 3-cell designs (d = .32, SE = .02, m = 51, k = 161), β = -.19, SE = .04, 95% CI [-.27;-.11], p<.001. Adding control variables did not change the results β = -.23, SE = .03, 95% CI [-.29;-.16], p < .001 (see appendix G for robustness tests).

This effect was primarily driven by studies focusing on high power. We found identical results when we conducted the analyses only for studies comparing high power to a control condition (see table 3). The effect size of these studies was significantly larger when no low power condition was included (d = .51, SE = .03, m = 47, k = 93) than when one was present (d = .37, SE = .03, m = 51, k = 83), β = -.13, SE = .05, 95% CI [-.22;-.03], p = .009. This pattern did not change when the control variables were added, β = -.17, SE = .05, 95% CI [-.26;-.08], p < .001 (see appendix G for robustness tests). Because only three

2-cell studies compared low power to a control condition, we were unable to estimate a model that would produce a trustworthy *p*–value (Tipton 2015). However, the means in table 3 suggest that, analogous to not including low power, omitting high power may also lead to higher effect sizes.

Table 3: Observed effect size as a function of experimental design.

	k	*m*	*d*	*95% CI*	T^2
High power vs. low power	**510**	**241**	**0.47*****	**[.44; .52]**	**0.14**
Control included	80	49	0.48***	[.40; .56]	0.11
Control not included	430	192	0.47***	[.43; .52]	0.14
High power vs. control	**176**	**98**	**0.44*****	**[.39; .49]**	**0.05**
Low power included	83	51	0.37***	[.31; .44]	0.03
Low power not included	93	47	0.51***	[.44; .57]	0.06
Control vs. low power	**84**	**50**	**0.25*****	**[.19; .32]**	**0.02**
High power included	78	47	0.23***	[.17; .29]	0.01
High power not included[a]	6	3	0.65***	[.40; .89]	0.00
Mean effect	**770**	**293**	**0.47*****	**[.43; .50]**	**0.13**



Note: CI = confidence interval, k = number of independent effect sizes, m = number of studies, *** $p < .001$.

[a] Effect of “Control vs. low power (high power not included)” could not be estimated as RVE requires at least 10 observations at the lowest hierarchy level (Hedges et al. 2010). This effect size was instead calculated using the procedure recommended by Borenstein et al. (2005) that ignores the multilevel structure of the data and is suitable to smaller samples.

Alternative interpretations. A critical reader may argue that instead of effect sizes being overestimated in studies employing 2-cell designs, effect sizes may be underestimated in studies with 3-cell designs. To address this concern we conducted a *p*-curve analysis to assess the strength of the evidential value of each type of study design (see appendix H for details). A *p*-curve indicates whether a published set of studies contains evidential value for an effect by analyzing the distribution of significant p-values (Simonsohn, Nelson, and Simmons 2014). Studies rejecting the null hypothesis that there is no true effect should exhibit low p-values and a right-skewed p-curve. In contrast, studies containing little evidential value should produce a uniform p-curve. The p-curve analysis suggests that studies using 3-cell designs ($Z = -2.83$, $p = .002$) and studies using 2-cell designs in which high and low power are compared ($Z = -4.58$, $p < .001$), were right-skewed and therefore contain evidential value. In contrast, the test for evidential value was not significant ($Z = -.84$, $p = .20$) for studies that merely compared high power to a control condition in the absence of a low power condition (see appendix H for robustness tests). Thus, the effect size of the high power effect is likely more robust when a low power condition is also present.

GENERAL DISCUSSION

Over the past fifteen years, researchers have shown a tremendous interest in the effects of social power and have concluded that the powerful make selfish choices (Pitesa and Thau 2013), take risks (Anderson and Galinsky 2006), are dishonest (Yap et al. 2013), and objectify those who are dependent on them (Gruenfeld et al. 2008). Either implicit or explicit in these inferences is the assumption that power effects are linear and that the powerless would therefore be less selfish, less risk-taking, less dishonest, or less objectifying. The present research examined whether the way in which power has been studied allows for such inferences. We uncovered a striking pattern in the published literature that is telling of the theoretical focus of social power research; a majority of studies are based on 2-cell designs that focus on studying the effects of powerfulness (part

1). One consequence of this preoccupation with the powerful is that effects tend to be attributed to high power when study designs do not allow for reliable inferences about the relative contribution of powerfulness and powerlessness to an observed effect (i.e., when no control condition is present; part II). Secondly, some studies merely examine powerfulness (as compared to a control condition) in the absence of a low power condition (part III). Although doing so may allow for inferences regarding effect directionality, a different set of challenges arises. Omitting the low power condition by design may result in different theoretical inferences when, for example, the effect of power is not linear. Overestimation of the high power effect may also arise when confounds bolster the effect or when the effect is actually driven by low power but goes undetected due to the use of a 2-cell design.

Identifying the causal force behind power's far-reaching effects

The results of this quantitative review challenge the assumption that powerfulness is the dominant causal force behind power's pervasive effect. Our findings beg the question of how the relative contribution of high and low power can be teased apart when a 3-cell design is not a possibility. One approach is to compare subjective and objective outcome measures. For example, Duguid and Goncalo (2012) examined whether the experience of power influences individuals' perception of their own height. Because study participants' estimates of their own height could be objectively compared to participants' actual height, the study was not only able to conclude that height estimates were higher for high power participants than for low power participants but also, that it was only in the high power condition where participants' estimates deviated from their objective height.

The conclusion that “the powerful overestimate their own height” (Duguid and Goncalo 2012, 36) is thus a valid one. Another approach could be to manipulate power using within-subjects designs (Greenwald 1976) in which the same group of participants is exposed to more than one treatment. Similar to 3-cell designs, within-subject designs allow for the identification of the causal force but may require fewer subjects since each subject provides multiple data points. Future research could consider other creative ways to draw more definite conclusions about powerfulness and powerlessness.

Re-examining the full spectrum of power

Our present research offers considerable evidence to question whether inferences about the psychology of powerlessness can be reliably made from observing the powerful. We noted earlier that most popular theories of power are based on a dichotomous framework where having power and lacking power are expected to lead to opposing outcomes (Keltner et al. 2003). Experimental designs often follow theories and so one implication for theory development is to take into account more complex effects of power. One way to effectively test whether the effects of power follow a linear pattern, a U-shaped pattern, or some other pattern is to use manipulations of power that allow for a more fine-grained differentiation between high and low levels of power. For example, Handgraaf and colleagues (2008) manipulated power by varying the delta in an ultimatum game where the offer sender makes an initial offer that can then be accepted or rejected by the offer recipient. The delta is an exogenously determined discount factor ($0 \leq \delta \leq 1$) that is used to calculate the final payoff of the players involved in an ultimatum game. In case the proposal of the offer sender is rejected by the offer recipient, the offer is multiplied by the delta. The appealing feature of this manipulation is that it covers the entire spectrum from high power (e.g. δ = 0; the recipient has substantive power) to low power (e.g. δ = .10; the recipient has some but little power) to no power (δ = 0; the recipient has no power at all). Other power manipulations are capable of achieving the same goal. For example, varying the decision weights in percent (Sachdev and Bourhis 1985) or changing the payoff structure in matrix games (Tjosvold and Sagaria 1978).

Another direction for future research is to systematically consider whether, when and why the powerless may behave in opposition to or in accordance with the powerful. For example, there is reason to believe that low power and absolute powerlessness (i.e., no power) can be associated with qualitatively different psychological experiences. Handgraaf and colleagues (2008) argue that "if even the smallest decrease in power means one of the parties becomes completely dependent on another, this may have strong effects on the way the situation is interpreted […]" (page 1146). Similar observations have been made in negotiations. Schaerer and colleagues (2015) found that although negotiators without any alternatives (no power) felt less powerful than those with weak alternatives (low power), the powerless achieved better outcomes than those with some power and at times negotiated as aggressively as those with high power. Without systematically considering the effect of powerlessness on individuals, instances in which those without power exhibit power-like behavior may be overlooked.

What is the ideal control condition?

Our findings raise the question of what an ideal control condition would look like. Social power research has relied on a variety of control conditions, including relational conditions (e.g., thinking about a peer; Gruenfeld et al. 2008; Inesi et al. 2012), non-relational conditions (e.g., thinking about one's last meal; Kraus, Chen, and Keltner 2011), neutral semantic primes (e.g., word completion task with power-neutral words; Anderson and Galinsky 2006), or no instructions at all (e.g., Blader and Chen 2012; Smith and Trope 2006). While there is no one-size-fits-all solution, factors such as the study context can affect the feasibility of one type of control condition over another. For example, although Galinsky and colleagues initially used a control prime asking participants to write about their previous day (Galinsky et al. 2003), they later replaced it with a prime asking participants to recall their last trip to the supermarket (e.g., Dubois, Rucker, and Galinsky 2015; Rucker and Galinsky 2008), because the former can be subject to unwanted contextual influences (e.g., the weather the day before or the loss of the local college basketball team on the previous day). The ideal control condition may also vary as a function of the research question. For example, when the goal is to invoke a generalized feeling of power, or a "power mindset," using a non-relational manipulation may be appropriate (e.g., Anderson and Galinsky 2006; Inesi 2010). In contrast, when a study aims to examine the effects of power on social distance, using an independent relationship as comparison may be more feasible (Magee and Smith 2013).

One way to alleviate concerns over selecting the ideal control condition may be to use different power manipulations across a series of studies as each manipulation tends to come with its own control condition. However, such an approach may come at the expense of internal replication. Alternatively, researchers can *control* for anticipated shortcomings of a manipulation through two primary means (see also Becker 2005). The first option is control by experimental design. To better disentangle the effects of power from potentially unwanted factors, one could include more than one control condition in the same study.

For example, Stevens and Fiske (2000) compared the effects of power (i.e., asymmetric dependence) to both a symmetric dependence condition and a neutral control condition. The second option is statistical control. Researchers could measure potential confounds and include them as covariates to test the robustness of their findings. For example, to rule out that mood (instead of the hypothesized illusory control) was driving the effects of power on approach-related tendencies, Fast and colleagues (2009) showed that their predictions remained robust when controlling for mood.

Mastering the tradeoff between design specificity and statistical validity

Although the present research suggest that 3-cell designs allow for a more rigorous test of a causal relationship, researchers may still find ways to draw valid conclusions when using 2-cell designs. One obvious approach to increasing the level of confidence in whether high and/or low power drive an effect, is to complement studies with two cells with at least one study with a 3-cell design (like 26% of the papers included our sample did). There are also other ways to deal with the lack of a 3-cell design, for example, by including process measures, testing boundary conditions, and successively addressing alternative explanations in a series of studies (Shadish et al. 2002). At the very least, researchers should explicitly disclose when their study designs do not allow for definitive conclusions about a certain aspect of power. For example, although DeCelles and colleagues (2012) compared high power to a control condition, they explicitly noted that "our theoretical focus is on the influence of powerfulness and moral identity, and so we manipulated a neutral control condition rather than low power or powerlessness in our experiments. It could prove interesting to develop and test theory related to how a perceived lack of power might interact with moral identity" (DeCelles et al. 2012, 687). Failing to explicate such limitations may prevent other researchers from investigating powerlessness or lead naïve readers to make premature judgments about the effects of power.

We also appreciate that due to the ever increasing need for larger sample sizes researchers may need to be more focused and economical when designing studies and that 2-cell study designs may be a consequence of that. In practice, decisions about the number of experimental cells and number of observations per cell are often interrelated and at times can be a tradeoff. Increasing the number of cells may come at the cost of smaller cell sizes. We believe that both dimensions are important to conduct more rigorous research; increasing cell size provides more confidence in *statistical* conclusions and increasing the number of cells strengthens *theoretical* conclusions. With the emergence of affordable and innovative ways to collect experimental data, such as large online participant pools (Buhrmester, Kwang, and Gosling 2011) and crowdsourced research (Silberzahn and Uhlmann 2015), such tradeoffs may become less of a challenge in the future.

Relevance to other fields

Finally, while we illustrated the prevalence and consequences of 2-cell designs in the power literature, we believe such conventions are potentially more widespread. For example, diversity researchers recently questioned whether homogeneity is an appropriate comparison group when studying the effects of diversity. They document that – similar to power – outcomes are often interpreted as the effect of diversity alone despite the fact that homogeneity can have independent effects on its own (Apfelbaum, Phillips, and Richeson

2014). Another example comes from evolutionary psychology; some studies found that women tend to prefer dominant men over nondominant men as romantic partners – but when compared to a control condition that describes a male without dominance-related traits, both dominant and nondominant males were relatively unattractive (Burger and Cosby 1999). Similar assumptions and traditions may also be present in other fields where high treatments, low treatments, and control conditions are possible, such as status (Sauder, Lynn, and Podolny 2012), regulatory focus (Higgins 1997), and accountability (Lerner and Tetlock 1999).

We end with the conclusion that more meta-science examining the prevalence and implications of study designs is needed to take stock of our ability to make theoretical inferences in other research domains. We hope this article inspires such research.

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APPENDIX A: LIST OF JOURNALS CONSIDERED

Below are the journals from which studies were selected for inclusion in our sample. Note that not all of the journals considered produced studies that were included in our final sample (e.g., Organization Science, Management Science).

Psychology

- British Journal of Social Psychology
- European Journal of Social Psychology
- Journal of Applied Psychology
- Journal of Experimental Social Psychology
- Journal of Personality and Social Psychology
- Personality and Social Psychology Bulletin
- Psychological Science
- Social Psychological and Personality Science

Organizational Behavior

- Academy of Management Journal
- Administrative Science Quarterly
- Journal of Management
- Management Science
- Organization Science
- Organizational Behavior and Human Decision Processes
- Research in Organizational Behavior

Marketing

- Journal of Consumer Psychology
- Journal of Consumer Research
- Journal of Marketing Research

APPENDIX B: LIST OF REVIEWS CONSIDERED

Anderson, Cameron and Sebastien Brion (2014), "Perspectives on Power in Organizations," *Annual Review of Organizational Psychology and Organizational Behavior,* 1(1), 67 – 97.

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APPENDIX C: OVERVIEW OF STUDIES INCLUDED IN META-ANALYSES AND P-CURVE

			Conditions			Analysis				
Authors	Year	Study	High power	Control	Low power	Meta-analysis	Strict primary p-curve	Strict robustness p-curve	Inclusive primary p-curve	Inclusive robustness p-curve
Anderson and Galinsky	2006	2	•		•	•	•	•	•	•
		3	•	•	•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Berdahl and Martorana	2006	1	•		•	•	•	•	•	•
Bian, Haque, and Smith	2012	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Blader and Chen	2012	1	•	•		•	•	•	•	•
		2	•	•		•	•		•	
		3	•	•		•	•		•	
		4	•		•	•	•		•	
		5	•		•	•	•	•	•	•
Bohns and Wiltermuth	2012	1	•	•	•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Brescoll	2011	2	•		•	•	•	•	•	•
Brinol, Petty, Valle, and Rucker	2007	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Brion and Anderson	2013	2	•	•	•	•				
Burgmer and Englich	2013	1	•	•		•	•	•	•	•
		2	•		•	•	•	•	•	•
Carney, Cuddy, and Yap	2010	1	•		•	•	•	•	•	•
Caza, Tiedens, and Lee	2011	1	•		•	•	•	•	•	•
	2011	2	•		•	•	•	•	•	•
Cho and Fast	2012	1	•		•	•	•	•	•	•
Cuddy, Wilmuth, Yap, and	2015	1	•		•	•	•	•	•	•
De Cremer and Van Dijk	2005	1	•		•	•	•	•	•	•
De Dreu and Van Kleef	2004	1	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
de Lemus, Spears, and Moya	2012	2	•		•			•		•
DeCelles, DeRue, Margolis, and Ceranic	2012	1	•	•		•				
		2	•	•		•				
Depret and Fiske	1999	1	•		•	•	•	•	•	•
DeWall, Baumeister, Mead, and Vohs	2011	1a	•	•	•	•	•	•	•	•
		1b	•	•	•	•	•	•	•	•
		2	•	•	•	•			•	•
		3	•	•		•	•	•	•	•
		4	•	•		•	•	•	•	•
		5	•	•	•	•			•	•
Dubois, Rucker, and Galinsky	2015	5	•	•	•	•			•	•
		6	•		•	•	•	•	•	•
Dubois, Rucker, and Galinsky	2010	1	•	•	•	•	•	•	•	•
		2	•		•	•			•	•
		3	•		•	•			•	•
		4	•		•	•	•	•	•	•
Duguid and Goncalo	2012	1	•	•	•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Fast and Chen	2009	2	•	•		•	•	•	•	•

			Conditions			Analysis				
Authors	Year	Study	High power	Control	Low power	Meta-analysis	Strict primary p-curve	Strict robustness p-curve	Inclusive primary p-curve	Inclusive robustness p-curve
Fast, Gruenfeld, Sivanathan, and Galinsky	2009	1	•	•	•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•	•		•	•	•	•	•
		4	•	•		•	•	•	•	•
Fast, Halevy, and Galinsky	2012	1	•		•		•	•	•	•
Fast, Sivanathan, Mayer, and Galinsky	2012	1	•		•	•	•	•	•	•
		2	•	•	•	•	•	•	•	•
		3	•	•	•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Ferguson, Ormiston, and Moon	2010	1	•	•	•	•	•	•	•	•
Fischer, Fischer, Englich, Aydin,	2011	2	•		•	•	•	•	•	•
Galinsky, Gruenfeld, and Magee	2003	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•	•	•	•	•	•	•	•
Galinsky, Magee, Gruenfeld, Whitson, and Liljenquist	2008	1	•	•		•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•	•	•	•	•	•	•	•
		4	•	•		•			•	•
		5	•		•	•	•	•	•	•
Galinsky, Magee, Inesi, and Gruenfeld	2006	1	•		•	•				
		2a	•		•	•	•	•	•	•
		2b	•		•	•	•	•	•	•
		3	•	•		•	•	•	•	•
Galinsky, Magee, Rus, Rothman, and Todd	2014	1	•		•	•			•	•
		2	•		•	•			•	•
Galinsky, Wang, Whitson, Anicich, Hugenberg, and Bodenhausen	2014	1	•		•	•	•	•	•	•
Garbinsky, Klesse, and Aaker	2014	1	•	•	•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Goldstein and Hays	2011	1	•		•	•	•	•	•	•
		2	•	•	•	•			•	•
		3	•	•	•	•			•	•
Gruenfeld, Inesi, Magee, and Galinsky	2013	1a	•	•		•	•	•	•	•
		1b	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•	•		•	•	•	•	•
		5	•	•		•	•	•	•	•
Guinote	2008	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
		6	•		•	•	•	•	•	•
Guinote	2007a	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Guinote	2007b	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Guinote	2007c	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Guinote, Weick, and Cai	2012	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•

			Conditions			Analysis				
Authors	Year	Study	High power	Control	Low power	Meta-analysis	Strict primary p-curve	Strict robustness p-curve	Inclusive primary p-curve	Inclusive robustness p-curve
Guinote, Willis, and Martellotta	2010	3	•		•	•		•		•
Hays and Goldstein	2015	2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Huang and Galinsky	2011	3	•		•	•	•	•	•	•
Huang, Galinsky, Gruenfeld, and Guillory	2011	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Inesi	2010	1	•	•		•	•	•	•	•
		2	•	•	•	•			•	•
		3	•		•	•	•	•	•	•
		4	•	•		•	•	•	•	•
Inesi, Botti, Dubois, Rucker, and Galinsky	2011	1a	•		•	•	•	•	•	•
		1b	•		•	•	•	•	•	•
		3a	•		•	•	•	•	•	•
		3b	•	•		•	•	•	•	•
Inesi, Gruenfeld, and Galinsky	2012	1	•	•		•	•	•	•	•
		2	•	•		•	•	•	•	•
		3	•	•		•	•	•	•	•
		5	•	•		•	•	•	•	•
Inesi, Lee, and Rios	2014	1	•	•		•	•	•	•	•
		2	•	•		•	•	•	•	•
		3	•	•		•	•	•	•	•
		4	•	•		•				
Inesi and Rios	2013	1	•	•		•		•		•
		2	•	•			•	•	•	•
		3	•	•			•	•	•	•
Jiang, Zhan, and Rucker	2014	1a	•		•	•	•	•	•	•
		1b	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•		•	•	•	•
		5	•		•	•	•	•	•	•
Jin, He, and Zhang	2014	1	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•	•	•	•	•		•	
Johnson and Lammers	2012	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•	•	•	•			•	•
Jordan, Sivanathan, and Galinsky	2011	1	•		•	•			•	•
		2	•		•	•				
		3	•		•	•			•	•
		5	•		•		•	•	•	•
Joshi and Fast	2013a	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•	•	•		•	•	•	•
Joshi and Fast	2013b	1	•		•	•	•	•	•	•
		2	•	•		•	•	•	•	•
		3	•	•	•	•			•	•
Karremans and Smith	2010	2	•		•	•	•	•	•	•
Kifer, Heller, Perunovic, and Galinsky	2013	2a	•		•	•	•	•	•	•
Kilduff and Galinsky	2013	2	•	•	•	•			•	•
Kim, Smith, and Brigham	1998	1	•		•	•			•	•
Kipnis	1972	1	•	•		•	•	•	•	•

			Conditions			Analysis				
Authors	Year	Study	High power	Control	Low power	Meta-analysis	Strict primary p-curve	Strict robustness p-curve	Inclusive primary p-curve	Inclusive robustness p-curve
Koning, Steinel, van Beest, and van Dijk	2011	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Kraus, Chen, and Keltner	2011	3	•	•	•				•	•
Kteily, Saguy, Sidanius, and Taylor	2013	2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Kunstman and Maner	2011	1	•	•		•	•	•	•	•
		2	•	•		•	•	•	•	•
		3	•	•		•	•	•	•	•
Lammers, Dubois, Rucker, and Galinsky	2013	1	•		•	•	•	•	•	•
		2	•	•	•	•			•	•
Lammers, Galinsky, Gordijn, and Otten	2008	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Lammers, Galinsky, Gordijn, and Otten	2012	1	•	•	•	•	•	•	•	•
		2	•	•	•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•	•	•	•	•	•	•	•
		5	•	•	•	•	•	•	•	•
Lammers, Gordijn, and Otten	2008	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•	•	•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Lammers and Stapel	2009	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3a	•		•	•	•	•	•	•
		3b	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Lammers, Stapel, and Galinsky	2010	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Lammers, Stoker, and Stapel	2009	1	•	•		•	•	•	•	•
Lisjak, Molden, and Lee	2012	4	•	•	•	•			•	•
Magee, Galinsky, and Gruenfeld	2007	1a	•		•	•	•	•	•	•
		1b	•		•	•	•	•	•	•
		2	•	•		•	•	•	•	•
		4	•		•	•	•	•	•	•
Maner, Gailliot, Menzel, and Kunstman	2012	1	•	•		•				
Miyamoto and Ji	2011	1	•	•	•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Moon and Chen	2014	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•	•	•	•	•	•	•	•
Mourali and Yang	2013	1	•		•	•	•	•	•	•
		2	•	•			•	•	•	•
		3	•	•			•	•	•	•
		4	•	•		•	•	•	•	•

			Conditions			Analysis				
Authors	Year	Study	High power	Control	Low power	Meta-analysis	Strict primary p-curve	Strict robustness p-curve	Inclusive primary p-curve	Inclusive robustness p-curve
Narayanan, Tai, and Kinias	2013	1	•	•	•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Overbeck and Droutman	2013	1	•		•		•	•	•	•
		2	•		•		•	•	•	•
		3	•	•	•		•	•	•	•
Overbeck and Park	2006	2	•		•	•	•	•	•	•
Overbeck and Park	2001	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Park, Streamer, Huang, and Galinsky	2013	1	•		•	•				
		2a	•		•	•	•	•	•	•
		2b	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Pitesa and Thau	2013a	1	•	•	•		•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•		•	•	•	•
Pitesa and Thau	2013b	1	•	•		•	•	•	•	•
		3	•	•		•	•	•	•	•
Ranehill, Dreber, Johannesson, Leiberg, Sul, and Weber	2015	1	•		•	•	•	•	•	•
Rodriguez-Bailon, Moya, and Yzerbyt	2000	1	•	•	•	•			•	•
Rucker, Dubois, and Galinsky	2011	1	•		•	•	•		•	
		2	•	•	•	•	•		•	
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Rucker and Galinsky	2009	2	•	•	•	•			•	•
		3	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Rucker and Galinsky	2008	1	•	•	•	•			•	•
		2	•	•	•	•			•	•
		3		•	•	•	•	•	•	•
Rucker, Hu, and Galinsky	2014	1a	•		•	•	•	•	•	•
		1b	•		•	•	•	•	•	•
		2a	•		•	•	•	•	•	•
		2b	•		•		•	•	•	•
		3	•		•	•	•	•	•	•
Rus, van Knippenberg, and Wisse	2010	1	•		•	•	•	•	•	•
Scheepers, de Wit, Ellemers, and Sassenberg	2012	1	•		•	•	•	•	•	•
Scheepers, Ellemers, and Sassenberg	2013	1	•		•		•	•	•	•
Schmid and Mast	2013	1	•	•		•	•	•	•	•
		2	•	•		•	•	•	•	•
Scholl and Sassenberg	2015	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•			•	•
Scholl and Sassenberg	2014	2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•	•	•	•				
See, Morrison, Rothman, and Soll	2011	3	•	•	•	•		•		•

			Conditions			Analysis				
Authors	Year	Study	High power	Control	Low power	Meta-analysis	Strict primary p-curve	Strict robustness p-curve	Inclusive primary p-curve	Inclusive robustness p-curve
Slabu and Guinote	2010	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Sligte, de Dreu, and Nijstad	2011	1	•		•	•				
		2	•		•		•	•	•	•
		3	•		•	•	•	•	•	•
Smith, Dijksterhuis, and Wigboldus	2008	1	•		•		•	•	•	•
		2	•		•		•	•	•	•
Smith, Jostmann, Galinsky, and van Diik	2008	1	•		•	•	•	•	•	•
		2	•	•	•	•			•	•
		3	•	•	•	•			•	•
		4	•	•	•	•			•	•
Smith and Trope	2006	1	•	•	•	•	•	•	•	•
		2	•	•	•	•	•	•	•	•
		3	•		•	•	•	•	•	•
		4	•	•	•	•			•	•
		5	•	•	•	•	•	•	•	•
		6	•	•	•	•	•	•	•	•
		7	•	•	•	•	•	•	•	•
Stillman, Baumeister, and DeWall	2007	1		•	•	•	•	•	•	•
		2	•	•	•	•	•	•	•	•
Tiedens and Fragale	2003	2	•		•	•			•	•
Tost, Gino, and Larrick	2013	1	•	•		•	•	•	•	•
		2	•	•		•	•	•	•	•
		3	•	•		•				
Tost, Gino, and Larrick	2012	2	•		•	•	•	•	•	•
van Dijk and De Cremer	2006	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Van Kleef, De Dreu, Pietroni, and Manstead	2006	4	•		•	•	•	•	•	•
		5	•		•	•	•	•	•	•
Van Kleef, Oveis, Homan, van der Löwe, and Keltner	2015	4	•		•	•	•	•	•	•
van Prooijen, Coffeng, and Vermeer	2014	1	•		•	•	•	•	•	•
		4	•		•	•	•	•	•	•
Wade-Benzoni, Hernandez, Medvec, and Messick	2008	3	•	•		•	•	•	•	•
Weick and Guinote	2010	2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Weick and Guinote	2008	1a	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•
Whitson, Liljenquist, Galinsky, Magee, Gruenfeld, and Cadena	2013	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Willis, Guinote, and Rodriguez-Bailon	2010	2		•	•	•			•	•
Wiltermuth and Flynn	2013	1	•	•		•	•	•	•	•
		4	•		•	•	•	•	•	•
Yap, Mason, and Ames	2013	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
Yap, Wazlawek, Lucas, Cuddy, and Carney	2013	1	•		•	•	•	•	•	•
		2	•		•	•	•	•	•	•
		3	•		•	•	•	•	•	•

Note: The actual number of p-values included in the p-curve analysis may vary because a) for some studies (e.g. reversing interactions) two p-values are required, and b) because the p-value re-calculates p-values based on the test statistics some tests may have been fallen outside the p<.05 criteria post recaluation.

APPENDIX D: CODING OF EFFECT DIRECTIONALITY ON MECHANICAL TURK

Method

To assess the robustness of our coding, we had each of the randomly selected study discussions coded by 10 individuals on Mechanical Turk. We recruited 58 individuals (mean age = 32.55, *SD* = 8.51; 30.0% female) who served as coders in exchange for $2.00.

Coders were informed that the purpose of the study was to examine how researchers interpret the results of their experiments in published scientific articles. Coders were trained by guiding them through four illustrative study discussions; one where the effect was clearly attributed to high power, one where the effect was attributed to low power, one where no unidirectional interpretation was made, and one where the interpretation was unclear. Upon completion of this practice round, each of the 58 coders was then randomly assigned to 10 of the 58 study discussions.

Each of the 10 study discussions was presented on a separate page together with the coding scales. On each page, participants were told to carefully read the respective paragraph and think about how the effect was interpreted. After reading the paragraph, participants were first asked to indicate whether the effects were primarily attributed to high or low power using a 7-point Likert scale (1 = *primarily to being powerless*; 4 = *equal / unclear attribution*; 7 = *primarily to being powerful*). Observer agreement was “almost perfect” (ICC =

.86; Landis and Koch 1977). Then, participants were asked to assign the study interpretation into one of four non-overlapping groups:

- High power: The effects are primarily attributed to high power (i.e., being powerful)
- Low power: The effects are primarily attributed to low power (i.e., being powerless)
- No directional interpretation: No unidirectional interpretation was made
- Unclear: The interpretation of the effect is unclear

Observer agreement was “substantial” for high power (ICC = .79; Landis and Koch 1977) and low power (ICC = .77), and “moderate” for the remaining two categories (ICCs = .50 and .57, respectively). Upon completion of the coding, all participants filled out an attention check (all participants succeeded) and reported their demographics.

Results

We first analyzed participants’ responses on the continuous scale. On average, studies tended to be interpreted in favor of the powerful. The mean rating (M = 4.71, SD = 1.08) was significantly above the midpoint of the scale, $t(57) = 5.00$, $p < .001$, $d = .96$.

We next analyzed the categorical coding. On average, 56.7 percent of all studies were coded as making directional interpretations. Of those, 42.2 percent were attributed to high power and 13.4 percent to low power. Only a third of all studies (32.9 percent) included a non-directional interpretation of the effect. In 11.3 percent of the cases, it was unclear or the discussion section did not discuss the actual effect (e.g., the section introduced the next study instead).

References

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APPENDIX E: ILLUSTRATIVE EXPERIMENT DEMONSTRATING THE IMPORTANCE OF 3-CELL DESIGNS

The purpose of this experiment was to show the merits of studying powerfulness in combination with powerlessness in a domain where high power is generally assumed to be the driver behind power's effects. Specifically, we ran an experiment examining the effect of social power on objectification. Since prior research has shown that powerfulness is associated with increased levels of objectification and aggressive behavior (e.g., Bargh et al. 1995; Gruenfeld et al. 2008; Kipnis 1972; Zimbardo 1973), it is reasonable to expect that we could replicate these effects. As such, we expected that participants would be more likely to report that they have objectified a work partner over whom they had power (i.e., a subordinate) than an individual who was their peer (the control condition used in Gruenfeld et al. 2008). According to this logic, one would conclude that power and objectification would be positively correlated, i.e. the more power one has, the more one will objectify others.

Design and Procedure

Participants. Three hundred and one participants were recruited from Amazon Mechanical Turk to participate in this study. Sample size was determined in advance based on a pretest. Prior to launching the study, we decided to exclude participants with duplicate IP addresses or those who failed an attention check (Oppenheimer, Meyvis, and Davidenko 2009). In addition, participants who were not currently employed or who were not a superior, subordinate and a peer in their current job were not eligible to participate in the study and were "screened out" prior to starting the survey. This was done to maximize the likelihood that each participant would be able to describe a relationship with either a subordinate, superior or a peer. However, at the end of the study, some participants still indicated that they were not currently employed or that they did not have experience as a supervisor, superior and peer. These participants were also excluded. Based on these a priori determined exclusion criteria, 42 subjects were dropped leaving a final sample of 259 participants (mean age = 32.61, *SD* = 9.42; 42.47% female). Results are identical when all 301 participants were analyzed instead (all ps < .001).

Design. Participants were randomly assigned to one of three conditions (low power, control, or high power) where they encountered a short writing task developed by Gruenfeld et al. (2008, study 1a). In the *high power condition*, participants were asked to describe a hierarchical, professional relationship in which their work partner reports directly to them or in which they have power or control over their work partner:

> Please think of a professional relationship you have, or have had in the past, that is hierarchical. The relationship should be one in which your work partner either reports directly to you or in which you have disproportionate power or control (or both) over him/her. Briefly describe your partner, and the nature of your relationship, in the space below.

In the *control condition,* participants were asked to describe a non–hierarchical, professional relationship in which neither they nor their work partner has comparably more power or control:

> Please think of a professional relationship you have, or have had in the past, that is not hierarchical. The relationship should be one in which you and your work partner do not report directly to one another, nor does one of you have disproportionate power or control over the other. Briefly describe your partner, and the nature of your relationship, in the space below.

In the *low power condition,* participants described a professional relationship in which they report directly to their work partner or one in which their work partner has power or control over them:

> Please think of a professional relationship you have, or have had in the past, that is hierarchical. The relationship should be one in which you report either directly to your work partner or in which your work partner has disproportionate power or control (or both) over you. Briefly describe your partner, and the nature of your relationship, in the space below.

Next, participants rated how likely they are to objectify the work partner they described. We used the established 10 item objectification scale previously used by Gruenfeld and colleagues (α = .69; e.g., “I think more about what this person can do for me than what I can do for him/her”, “I tend to contact this person when I need something from him/her” and “This relationship is important to me because it helps me accomplish my goals”). Next, participants were asked to think back to the relationship they described and rate how powerful they felt in this relationship. We used the sense of power scale developed by Anderson, John, and Keltner (2012; $\alpha = .91$; e.g., “In the relationship with this person I can get him/ her to do what I want”, “In the relationship with this person I think I have a great deal of power” and “In the relationship with this person I can get him/her to listen to what I say”). Last, participants completed an attention check, provided their demographic information, and were debriefed.

Results

Manipulation Check. As expected, participants in the high power condition reported feeling more powerful ($M = 5.73$, $SD = 1.01$) than participants in the control condition ($M = 5.16$, $SD = 0.76$), $F(1,258) = 12.98$, $p < .001$, $d = .65$. Participants in the low power condition reported feeling less powerful ($M = 4.32$, $SD = 1.36$) than participants in the control condition, $F(1, 258) = 28.08$, $p < .001$, $d = -.78$, and the high power condition, $F(1, 258) = 72.11$, $p < .001$, $d = -1.18$. Thus, the power manipulation was successful.

Objectification. Based on prior research we expected that participants would report being more likely to objectify another person when the person in the described professional relationship was a subordinate rather than a peer. This was what we found. Participants in the high power condition were more likely to objectify their work partner ($M = 4.49$, $SD = .86$) than participants in the control condition ($M = 3.86$, $SD = .88$), $F(1,258) = 25.10$, $p < .001$, $d = .72$.

Strikingly, participants who were asked to describe a professional relationship in which their work partner had power over them (low power condition) also reported being more likely to objectify this work partner (M = 4.47, SD = .70) than participants in the control condition, $F(1,258) = 23.70$, $p < .001$, $d = .76$. The difference in objectification between participants in the low power and high power conditions was not significant, $F(1,258) = .02$, $p = .88$, $d = -.03$, suggesting that the powerful and powerless exhibit similar tendencies to objectify others.

Discussion

This study shows that comparing the effects of high power to a control condition, could lead to different conclusions about the effects of power depending on whether powerlessness is simultaneously tested as well. In this study, a researcher who only included a high power and control condition in their design would conclude that being in a position of power increases the objectification of social targets. In contrast, the surprising effect of the low power condition could suggest, for example, that the effects of power on objectification are curvilinear or that any type of vertical hierarchical thinking could increase objectification as compared to non-hierarchical thinking. In the manuscript, we discuss a number of likely explanations for such an effect.

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APPENDIX F: META-ANALYSIS METHODOLOGY

Variable coding

Power manipulations. All eligible manipulations of social power were separated into a total of four manipulation types (see table 2; for the coding scheme used see Galinsky et al. 2015). *Structural manipulations* included scenarios in which participants take on a hierarchical role (e.g. a supervisor, peer, or subordinate role), are given control over resources (e.g. money to be distributed), or are associated with another powerful entity (e.g. a colleague or group). *Experiential manipulations* involved episodic recall primes (e.g. participants recall having power over someone else) and imagined hierarchical role manipulations (e.g. participants imagine being in a hypothetical scenario). *Conceptual manipulations* included semantic primes (e.g. word search puzzle), and *physical manipulations* included body posture manipulations (e.g. expanded or contracted pose).

Dependent measure. The scheme for the dependent measures consisted of seven categories (Galinsky et al. 2015), namely *cognition* (e.g. abstraction), *self-perception* (e.g. optimism), *social perception* (e.g. perspective taking), *resistance to influence* (e.g. conformity), *performance and behavior* (e.g. risk-taking), *motivation and evaluation* (e.g. goal pursuit), and *physiological measures* (e.g. heart rate).

Experimental design. Next, we coded whether a study included a manipulation check, a mediating variable, and/or a moderating variable. The moderating variable had to be manipulated orthogonally to power (e.g. in a 2x2 between-subjects factorial design) and was subject to the same criteria of independence and random assignment as the power manipulation.

Experimental setting. It is possible that variations in effect size stem from factors related to the study setting. Thus, we coded the location where the study was conducted (United States, outside the United States, unknown), and whether a study was conducted in a laboratory, online (e.g. Mechanical Turk), or in other settings (e.g. field).

Additional controls. We also controlled for the number of observations that was the basis for a particular effect size calculation and a study's publication year.

Coding of factorial study designs

When studies manipulated other constructs orthogonally to power, we calculated the average effect size of power. For example, when a reversal in directionality was predicted (e.g. in a 2x2 between-subjects design with a categorical moderator), the larger effect size was coded as a positive value (at level 1 of the moderator) and the smaller effect size as negative (at level 2 of the moderator), leading to a positive main effect for that study. We took a number of different precautions to make sure that any effects we found would not result from penalizing interactive designs (e.g. where an attenuation is predicted) over main effect studies. First, in the meta-regression we controlled for whether a study included a moderator variable that was manipulated orthogonally to power. Second, we also conducted separate robustness analyses (see appendix G) where we excluded any studies with moderating variables.

Results

Note that the mean effect of power observed across all studies (d = .47, SE = .02, m = 293, k = 770) is somewhat larger than the average effect size typically observed in psychological research (see Bakker, van Dijk and Wicherts 2012). This could reflect the fact that the power manipulations in our sample tend to be more heavy-handed than typical manipulations used in other psychological research. It could also reflect that there may be publication bias present in the literature. Indeed, several scholars have made the argument that it is almost certain that any large body of research suffers from some degree of publication bias (Simonsohn 2012) and inflated effect sizes (Ioannidis 2008).

While this is an important and interesting finding in itself that deserves further attention, the focus of the present meta-analysis is different from a typical meta-analysis. While a typical meta-analysis is concerned with the estimation of the *true* effect in the *universe* of all effects (e.g. Borenstein et al. 2005), the present research was primarily interested in comparing and contrasting *relative* effect sizes in the *published* literature. Future research could identify unpublished studies to derive a better estimate for the true effect size of social power and/or use effect size estimation approaches that are free of publication bias “(e.g., Tuk, Zhang, and Sweldens 2015).

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Tuk, Mirjam A., Kuangjie Zhang and Steven Sweldens (2015), “The Propagation of Self-Control: Self-Control in One Domain Simultaneously Improves Self-Control in Other Domains,” *Journal of Experimental Psychology: General*, 144(3), 639 – 654.

APPENDIX G: META-ANALYSIS ROBUSTNESS CHECKS

Analysis based only on three most popular primes

We replicated our main analyses using only the three most popular power manipulations (i.e. hierarchical role manipulation, recall prime, and semantic primes). The effect size of studies with 2-cell designs comparing either high *or* low power to a control condition was significantly larger (d = .51, SE = .04, m = 42, k = 80) than the respective effect size of studies using 3-cell designs (d = .33, SE = .02, m = 45, k = 144), β = -.17, SE = .04, 95% CI [-.25; -.09], p < .001. This pattern did not change when the control variables were added, β = -.20, SE = .04, 95% CI [-.27; -.13], p < .001.

We found similar results when we conducted the analyses only for studies comparing high power to a control condition. The effect size of high power relative to a control condition was significantly larger when no low power condition was present (d = .51, SE = .04, m = 40, k = 75) than when one was present (d = .39, SE = .03, m = 45, k = 74), β = -.11, SE = .05, 95% CI [-.21; -.01], p = .032. This pattern did not change when the control variables were added, β = -.13, SE = .05, 95% CI [-.24; -.02], p = .021.

Analysis excluding studies with factorial designs

To make sure that any effects we found were not driven by whether some studies are more likely to include moderators (e.g. predicting attenuating or reversing interactions) than other studies, we repeated the analyses using only the portion of the studies that were based on main effects. Excluding studies with moderating predictions did not change our results.

Studies with 2-cell designs comparing either high *or* low power to a control condition exhibited larger effect sizes (d = .62, SE = .03, m = 28, k = 47) than the respective studies using 3-cell designs (d = .34, SE = .02, m = 34, k = 93), β = -.28, SE = .03, 95% CI [-.35; -.21], $p < .001$. This pattern did not change when the control variables were added, β = -.22, SE = .03, 95% CI [-.28; -.16], $p < .001$.

We found similar results when we conducted the analyses only for studies comparing high power to a control condition. The effect size of high power relative to a control condition was significantly larger when no low power condition was present (d = .61, SE = .03, m = 25, k = 41) than when one was present (d = .40, SE = .03, m = 34, k = 49), β = -.22, SE = .04, 95% CI [-.30; -.13], $p < .001$. This pattern did not change when the control variables were added, β = -.14, SE = .05, 95% CI [-.24; -.04], p = .007.

APPENDIX H: P-CURVE ANALYSIS AND RESULTS

Although the meta-analysis indicates that the effect sizes of high power might be overestimated when no low power condition was included, it is possible that instead of effect sizes being inflated in studies employing 2-cell designs it is the case that effect sizes are underestimated in studies with a 3-cell design. To address this concern we conducted a *p*-curve analysis to assess the evidential value of each type of study design. The *p*-curve tool can be used to indicate whether a published set of studies contains evidential value for a true effect by analyzing the distribution of significant *p*-values (Simonsohn, Nelson, and Simmons 2014). Studies rejecting the null hypothesis that there is no true effect should exhibit low *p*-values and thus produce a right-skewed *p*-curve. In contrast, studies containing little evidential value would produce a uniform *p*-curve.

Method

Article selection. As the purpose of the *p*-curve is to analyze the distribution of significant *p*-values, only significant effects ($p < .05$) were considered. Of the 293 studies included in the meta-analysis, a maximum of 282 studies could be included in the p-curve after eliminating studies that reported non-significant results or lacked the statistics needed for the p-curve tool. In addition, 20 studies reported insufficient statistics to calculate effect sizes but could be included in the *p*-curve, leading to a total of 302 eligible studies (see appendix C for a comprehensive list).

P-value selection. For each study, we identified the relevant *p*-value(s) which need to be independent of each other. In most cases, this was a single *p*-value (e.g., main effect). However, in some cases (e.g., reversing interaction), two *p*-values were required (e.g., two simple effects). We adhered to the guidelines laid out in the *p*-curve manual, but also took several steps to account for any remaining ambiguity. First, we decided a priori to always select the first reported relevant test when multiple eligible tests were reported (e.g., multiple dependent variables). To make sure that our findings were not an artifact of this decision, we also conducted a robustness curve for which we always selected the last reported test. For studies that only included a single test, the *p*-values entered in the primary and robustness *p*-curves were identical. Second, it is possible that hypotheses were tested with statistical tests other than the one specified in Simonsohn et al. (2014). For example, Tiedens and Fragale (2003) predicted a reversing interaction (which would call for two simple effects) but used a planned contrast to test their hypotheses. Taking this into consideration, we also conducted a more inclusive *p*-curve (and the respective robustness curve) where we also included alternative tests that were reported in lieu of the ones specified by the *p*-curve architects. Third, we provide an exhaustive list of the selected *p*-values, including the rationale of our decision when the selection of the relevant *p*-value was not unambiguous (see https://osf.io/fbmwz/?view_only=483af598ef4f4080a6e60eed4218da44).

P-curve calculation. We used the *p*-curve web application (http://www.p-curve.com) to calculate each of the *p*-curves. The application recalculates all *p*-values from the test statistics entered and then tests the skew of the resulting curve. For each *p*-value, the tool calculates the probability of observing a significant *p*-value as extreme or more extreme assuming the null is true. These probabilities are then tested against a uniform *p*-curve. If the null of a uniform *p*-curve is rejected, it can be concluded that the underlying set of studies contain evidential value. The *p*-curve application also tests whether an observed curve is flatter than one produced by powered studies powered at 33% (suggesting a lack of evidential value) and whether an observed effect is left-skewed (suggesting the use of QRPs, Simmons, Nelson, and Simonsohn 2011).

Results

We first analyzed the *p*-curves for the set of studies that contained high power, low power, and control conditions (see panel A of figure 1). The resulting primary *p*-curve (based on 30 significant *p*-values) was significantly right-skewed, $Z = -2.83$, $p = .002$, and so was the robustness curve (26 significant *p*-values), $Z = -1.98$, $p = .024$. Identical right-skewed patterns were found for the more generous primary *p*-curve (50 significant *p*-values), $Z = -3.74$, $p < .001$, and its respective robustness curve (44 significant *p*-values), $Z = -2.24$, $p = .013$.

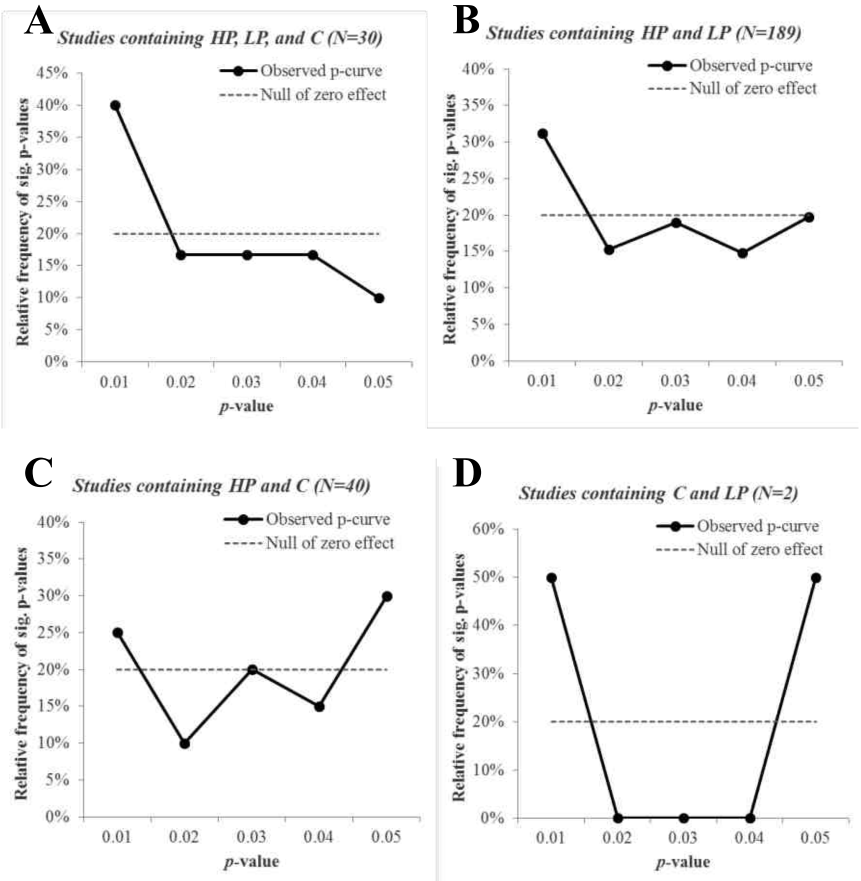
We then conducted the same analyses for studies that contained only high and low power conditions (see panel B of figure 1). Again, we found that both the primary *p*-curve (189 significant *p*-values), $Z = -4.58$, $p < .001$, and the robustness curve (181 significant *p*-values), $Z = -4.82$, $p < .001$, were significantly right-skewed. We found identical patterns for the more inclusive *p*-curve (198 significant *p*-values), $Z = -5.53$, $p < .001$, and its respective robustness curve (190 significant *p*-values), $Z = -5.75$, $p < .001$. Interestingly, the test showing that the observed *p*-curve is flatter than one based on studies powered at 33% was also significant in all four cases (all *Z*s < -2.51, all *p*s < .006). This may suggest that although the majority of studies reflect a true phenomenon, there may still be an overrepresentation of *p*-values between .025 and .05.

Next, we examined the shape of the *p*-curve of the set of studies that merely compared high power and a control condition (see panel C of figure 1). The null hypothesis that the primary *p*-curve (40 significant *p*-values), $Z = -.84$, $p =.20$, and the robustness curve (37 significant *p*-values), $Z = -1.49$, $p = .07$, were significantly different from a uniform distribution could not be rejected. Similarly, the more inclusive *p*-curve (41 significant *p*-values), $Z = -.94$, $p = .17$, and its respective robustness curve (38 significant *p*-values), $Z = -1.58$, $p = .06$, were not significantly right-skewed (marginal at best). This could allow for two potential conclusion: the set of underlying studies represent a) a precise estimate of a small or nonexistent effect, or b) a noisy estimate of a small to large effect (Simonsohn et al. 2014).

To distinguish between those two cases, we tested whether the observed *p*-curve is flatter than one produced by studies powered at 33%. Both the primary *p*-curve, $Z = -2.68$, $p = .004$, and the robustness curve, $Z = -1.99$, $p = .023$, were flatter than one based on studies powered at 33%. This was also the case for the generous *p*-curve, $Z = -2.62$, $p = .004$, and its respective robustness test, $Z = -1.94$, $p = .026$. In sum, these patterns suggest that either the effects tested by studies lacking a low power condition are unlikely to exist or are too small for the underlying samples to detect. Both conclusions call for additional research to better understand the effects of power.

We also conducted *p*-curve analyses for studies that only contained control and low power conditions (see panel D of figure 1). However, due to the small number of *p*-values (3 or less) the tests of the *p*-curve tool are inconclusive (all *p*s > .19). Finally, none of the cumulative sets of studies analyzed above exhibited any indication of QRPs (all *p*s > .79).

Figure H1: Observed p-curve as a function of study design



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APPENDIX I: FULL REFERENCES OF ARTICLES INCLUDED

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CHAPTER 5

GENERAL DISCUSSION

This dissertation challenges common methodological conventions used to study social influence in consumer behavior and, more broadly, social psychology. In the first part of this work, chapter 2 and 3, we move beyond the common methodological convention of focusing on how one agent (e.g., a review writer) influences another (e.g., a review reader) in a single dyadic relationship, and instead focus on the dynamic nature of social influence. Specifically, we consider how one dyadic relationship between two agents (e.g., a company and the review writer it incentivizes) influences another (e.g., the review writer and the review reader).

In chapter 2, we investigate how monetary incentives affect the generation of reviews by writers and the subsequent reception of these reviews by readers. Companies increasingly offer small monetary incentives to encourage consumers to write product reviews and post them online. However, it is unclear how such incentives will influence writers and, subsequently, their ability to persuade readers. Building on the idea that monetary incentives convey a social signal of the value ascribed to the role of reviewer, we show that writers infer their own legitimacy as reviewers from the size of the incentive provided. In a field study and two experiments we find that small (vs. large or no) monetary incentives reduced writers' perceptions of being legitimate reviewers. This in turn increased their uncertainty, which carried over to readers, raising doubt in the quality of the product reviewed and subsequently decreasing readers' product evaluations. We conclude this chapter by discussing the contribution of our research to the attitude formation, persuasion, and word-of-mouth literatures, and its ability to inform marketing managers' decisions regarding reviewer incentivization and sentiment analysis.

In chapter 3, we briefly consider how disclosure of such incentives affect review persuasiveness. Due to the proliferation in incentivized reviews, review writers are now required to disclosure if their review is incentivized by a third party. We find that, though such disclosure statements induce uncertainty about writer trustworthiness, this uncertainty does not necessarily lead reader to discount the writers' opinion when judging product quality. Instead, we predicted that the extent to which disclosing incentives affects review persuasiveness depends on whether readers deem their disclosure-induced uncertainty to be *integral* or *incidental* to judgment formation. This occurs through a metacognitive process in which readers elaborate on the relevance of their uncertainty. In a field study and two experiments, we showed that disclosure-induced uncertainty about reviewer trustworthiness deemed integral to judgement formation, affected product evaluations. This occurred when consumers elaborated on the fact that the disclosed source of the incentive had a stake in a positive review (study 2), and when incentive provision was uncommon (study 3). In contrast, when uncertainty was incidental to judgment formation, product evaluations were unaffected by incentive disclosure. This occurred for incentivized reviews of high involvement products (study 1), and when consumers elaborated on the fact that incentive provision was common practice (study 3). We then discussed the implications of these findings for theory and practice.

In the second part of this work, chapter 4, we challenged another prevalent methodological convention. Specifically, we considered the limitations of the widespread use of 2-cell instead of 3-cell designs, in the study of an important source of social influence – social power.

A pervasive assumption in the social power literature is that powerfulness is the driving causal force behind power's far-reaching effects. In chapter 4, we showed that this preoccupation with the powerful has led to the proliferation of experimental designs that contrast high power to either a low power *or* a control condition. Across a content analysis, an experiment, and a large-scale meta-analysis, we observed the attribution of effects, from studies comparing only high and low power, to powerfulness. Further, we found that comparing high power to a control condition, in the absence of low power, weakened construct validity and inflated the high-power effect. This quantitative review demonstrated how a prevailing methodological tradition in the study of social power limits our understanding of powerlessness, powerfulness and social influence more generally. We concluded with a discussion of the theoretical and methodological implications of our findings for social power and related fields.

DIRECTIONS FOR FUTURE RESEARCH

In this dissertation, I challenge two common methodological conventions used to study social influence; the study of social influence in isolated dyadic relationships, and the widespread use of 2-cell instead of 3-cell designs in social power research. Future research should seek to investigate how other relational characteristics of social interactions may shape influence attempts. For example, it is likely that the hierarchical relationship between a message sender and receiver will influence the type of information sent. Hierarchies are pervasive in social, organizational and even familial interactions and refer to the implicit or explicit rank order of individuals or subgroups with respect to a valued social dimension (Magee and Galinsky 2008). In a follow-up project David Dubois, Michael Schaerer and I investigate *when* consumers share more positive or negative information about a product they have used, as a function of their hierarchical relationship with the message recipient. We hypothesize and show that the higher a message recipient is in the social hierarchy, relative to the sender, the more positive the message is that they receive. We show that this occurs because message senders desire approval from receivers above them in the social hierarchy, and therefore omit negative information about the product they have used.

An important finding in this dissertation is that the widespread use of 2-cell instead of 3-cell designs in social power research may limit theoretical advancement because such designs only allow researchers to test for linear effects. However, we illustrate that the consequences of social power may sometimes be curvilinear, a finding that suggests that the psychological states of powerfulness and powerlessness may be distinct. An important directive for future research is therefore to investigate if other effects of power, such as its effect on risk-taking, perceived social distance or authenticity, are also non-linear.

In recent work, my co-authors and I have begun to explore one such effect – the effect of power on risk-taking. In contrast to the widely held belief that having power increases risk-taking because the powerful are more sensitive to rewards and less sensitive to losses (Anderson and Brion 2014; Galinsky, Rucker, and Magee 2015), we show that there is considerable variance and no significant main effect of power on risk-taking (*d*s < .16, *p*s > .19) across 20 published and unpublished experiments. Building on the functional account of risk-taking (Mishra 2014; Mishra, Barclay, and Sparks 2016), we show that both the powerful and powerless take risks, but under different circumstances; the powerful take risks when risk taking is ability-based whereas the powerless are more prone to take risks when risk-taking is need-based.

CONCLUSION

The objective of this research was to challenge common methodological conventions used to study social influence in consumer behavior and social psychological more broadly, and show how novel methodological approaches can lead to new theoretical insights. In the first part of this dissertation, I moved beyond the study of social influence in a single dyadic relationship, and investigated how one dyadic relationship influences another. Here, my co-authors and I demonstrated that influence attempts by one agent (e.g., a company) on another (e.g., a review writer) does not only influence the cognitions of the agent being influenced, but also others whom this agent attempts to influence (e.g., review readers). In the second part of this dissertation, my co-authors and I investigated how the widespread use of 2-cell instead of 3-cell experimental designs in social power research has limited our understanding of both the powerful and powerless. We showed that the pervasive practice of contrasting high power to either a low power or a control condition has weakened construct validity and inflated the size of the high power effect. We conclude by discussing different methodological strategies researchers in social power and related fields can use to circumvent the identified limitations of 2-cell designs.

These findings highlight the need for scholars in consumer behavior, and social psychologists more broadly, to question common methodological conventions used to study social influence. Overall, my research suggests that doing so may not only increase methodological rigor and our confidence in subsequently observed effects, but may also lead to novel theoretical insights.

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SUMMARY (ENGLISH)

This dissertation challenges common methodological conventions used to study social influence in consumer behavior and, more broadly, social psychology. In the first part of this work, chapter 2 and 3, we move beyond the common methodological convention of focusing on how one agent (e.g., a review writer) influences another (e.g., a review reader) in a single dyadic relationship, and instead focus on the dynamic nature of social influence. Specifically, we consider how one dyadic relationship between two agents (e.g., a company and the review writer it incentivizes) influences another (e.g., the review writer and the review reader).

In chapter 2, we investigate how monetary incentives affect the generation of reviews by writers and the subsequent reception of these reviews by readers. Companies increasingly offer small monetary incentives to encourage consumers to write product reviews and post them online. However, it is unclear how such incentives will influence writers and, subsequently, their ability to persuade readers. Building on the idea that monetary incentives convey a social signal of the value ascribed to the role of reviewer, we show that writers infer their own legitimacy as reviewers from the size of the incentive provided. In a field study and two experiments we find that small (vs. large or no) monetary incentives reduced writers' perceptions of being legitimate reviewers. This in turn increased their uncertainty, which carried over to readers, raising doubt in the quality of the product reviewed and subsequently decreasing readers' product evaluations. We

conclude this chapter by discussing the contribution of our research to the attitude formation, persuasion, and word-of-mouth literatures, and its ability to inform marketing managers' decisions regarding reviewer incentivization and sentiment analysis.

In chapter 3, we briefly consider how disclosure of such incentives affect review persuasiveness. Due to the proliferation in incentivized reviews, review writers are now required to disclosure if their review is incentivized by a third party. We find that, though such disclosure statements induce uncertainty about writer trustworthiness, this uncertainty does not necessarily lead readers to discount the writers' opinion when judging product quality. Instead, we predicted that the extent to which disclosing incentives affects review persuasiveness depends on whether readers deem their disclosure-induced uncertainty to be *integral* or *incidental* to judgment formation. This occurs through a metacognitive process in which readers elaborate on the relevance of their uncertainty. In a field study and two experiments, we showed that disclosure-induced uncertainty about reviewer trustworthiness deemed integral to judgement formation, affected product evaluations. This occurred when consumers elaborated on the fact that the disclosed source of the incentive had a stake in a positive review (study 2), and when incentive provision was uncommon (study 3). In contrast, when uncertainty was incidental to judgment formation, product evaluations were unaffected by incentive disclosure. This occurred for incentivized reviews of high involvement products (study 1), and when consumers elaborated on the fact that incentive provision was common practice (study 3). We then discussed the implications of these findings for theory and practice.

In the second part of this work, chapter 4, we challenged another prevalent methodological convention. Specifically, we considered the limitations of the widespread use of 2-cell instead of 3-cell designs, in the study of an important source of social influence – social power.

A pervasive assumption in the social power literature is that powerfulness is the driving causal force behind power's far-reaching effects. In chapter 4, we showed that this preoccupation with the powerful has led to the proliferation of experimental designs that contrast high power to either a low power *or* a control condition. Across a content analysis, an experiment, and a large-scale meta-analysis, we observed the attribution of effects, from studies comparing only high and low power, to powerfulness. Further, we found that comparing high power to a control condition, in the absence of low power, weakened construct validity and inflated the high-power effect. This quantitative review demonstrated how a prevailing methodological tradition in the study of social power limits our understanding of powerlessness, powerfulness and social influence more generally. We concluded with a discussion of the theoretical and methodological implications of our findings for social power and related fields.

SAMENVATTING (NEDERLANDS)

Dit proefschrift stelt algemene methodologische conventies die gebruikt worden om sociale beïnvloeding te bestuderen in consumentengedrag en, in bredere zin, de sociale psychologie ter discussie. In het eerste gedeelte van dit werk, hoofdstuk 2 en 3, gaan we voorbij de algemene methodologische conventie om te focussen op hoe één agent (bv., de schrijver van een review) een andere beïnvloedt (bv., de lezer van een review) in een enkele dyadische relatie, en focussen we in plaats daarvan op de dynamische aard van sociale beïnvloeding. In het bijzonder onderzoeken we hoe een dyadische relatie tussen twee agenten (bv., een bedrijf en de schrijver van een review die daarvoor door het bedrijf wordt beloond) een andere (bv., de schrijver van een review en de lezer van een review) beïnvloedt.

In hoofdstuk 2 onderzoeken we wat de invloed is van financiële beloningen op het genereren van reviews bij reviewers, en de reactie op deze reviews bij lezers. Bedrijven bieden steeds vaker kleine financiële beloningen aan om consumenten aan te moedigen om reviews te schrijven over producten en deze online te plaatsen. Het is echter onduidelijk wat de invloed is van dergelijke beloningen op reviewers en vervolgens op hun vermogen om lezers te overtuigen. Gebaseerd op het idee dat financiële beloningen een sociaal signaal afgeven dat wordt toegekend aan de waarde van de rol van reviewer, laten we zien dat schrijvers hun eigen legitimiteit als reviewer afleiden uit de grootte van de beloning die wordt gegeven. In een veldstudie en twee experimenten vonden we dat kleine (vs. grote of geen) financiële beloningen de perceptie van de schrijver als zijnde een legitieme reviewer verminderde. Dit verhoogde vervolgens hun onzekerheid, dat werd overgedragen op de lezers en twijfel teweeg bracht over de kwaliteit van het beoordeelde product en vervolgens productevaluaties van de lezer verlaagde. We eindigen dit hoofdstuk met een bespreking van de bijdrage van ons onderzoek aan de literatuur over de formatie van attitudes, beïnvloeding en mond-tot-mondreclame, en de toepassing van het onderzoek op beslissingen van marketing managers met betrekking tot het belonen van reviewers en sentiment analyses.

In hoofdstuk 3 beschouwen we kort hoe de vermelding van dit soort beloningen de overtuigingskracht van reviews beïnvloedt. Door de snelle toename van beloonde reviews is het nu vereist dat reviewers vermelden wanneer hun review is beloond door een derde partij. We vinden dat, hoewel deze vermeldingen onzekerheid over de betrouwbaarheid van de schrijver teweegbrengen, deze onzekerheid niet noodzakelijk tot gevolg heeft dat de lezer de mening van de schrijver naast zich neerlegt wanneer deze de productkwaliteit beoordeelt. In plaats daarvan voorspelden we dat de mate waarin het vermelden van beloningen de overtuigingskracht van de review beïnvloedt afhankelijk is van of de onzekerheid die is teweeggebracht door de vermelding door de lezer gezien wordt als *integraal* of *incidenteel* met betrekking tot de totstandkoming van de beoordeling. Dit gebeurt via een metacognitief proces waar in de lezers elaboreren over de relevantie van hun onzekerheid. In een veldstudie en twee experimenten laten we zien dat onzekerheid die teweeg wordt gebracht door de vermelding als integraal met betrekking tot de totstandkoming van de beoordeling de productevaluaties beïnvloedde. Dit gebeurde wanneer consumenten nadachten over het feit dat de vermelde bron van de beloning belang had bij een positieve review (studie 2), en wanneer het geven van een beloning ongebruikelijk was (studie 3). Echter, als de onzekerheid incidenteel was met betrekking tot de totstandkoming van de beoordeling werden productevaluaties niet beïnvloed door het vermelden van de beloning. Dit gebeurde voor beloonde reviews voor *high involvement* producten (studie 1), en wanneer consumenten nadachten over het feit dat het geven van de beloning gebruikelijk was (studie 3). Dit wordt gevolgd door een bespreking van de theoretische en praktische implicaties van deze bevindingen.

In het tweede gedeelte van dit werk, hoofdstuk 4, betwisten we een andere prevalente methodologische conventie. In het bijzonder onderzoeken we de beperkingen van de veelvoudig gebruikte onderzoeksopzet met 2 cellen, in plaats van 3 cellen, in onderzoek naar een belangrijke bron van sociale beïnvloeding – sociale macht.

Een prominente aanname in de literatuur over sociale macht is dat het hebben van veel macht de drijvende kracht is achter de verstrekkende effecten van macht. In hoofdstuk 4 tonen we aan dat de preoccupatie met de machthebbende heeft geleid tot een proliferatie van onderzoeksopzetten die het hebben van veel macht contrasteren met *of* weinig macht, *of* een controle conditie. In een content analyse, een experiment en een grootschalige meta-analyse observeren we dat de effecten van studies die alleen het hebben van veel en weinig macht vergelijken, worden toegekend aan de het hebben van veel macht. Verder vonden we dat het vergelijken van het hebben van veel macht met een controle conditie, in de afwezigheid van het hebben van weinig macht, de constructvaliditeit verzwakt en het effect van het hebben van veel macht overschat. Deze kwantitatieve review heeft aangetoond hoe een gangbare methodologische traditie in het onderzoek naar sociale macht ons begrip van het hebben van weinig macht, het hebben van veel macht, en sociale invloed in het algemeen, heeft gelimiteerd. We concluderen met een bespreking van de theoretische van methodologische implicaties van onze bevindingen voor sociale macht en gerelateerde gebieden.

ABOUT THE AUTHOR

Christilene du Plessis was born in Phalaborwa, South Africa on March 31, 1986. She received her Bachelor's degrees in Science (majors in physiology and biomedical science) and Business Management (major in marketing) from the University of Queensland. She received her Honor's degree (first class) in Business Management from the University of Queensland, and her Master's degree in Management from INSEAD. In 2014, she started her Ph.D research in Marketing at the Erasmus Research Institute in Management. Her main research interests concern social influence and how it shapes (and is shaped by) consumers and marketers in the context of digital marketing. Her work has been published in the Journal of Experimental Social Psychology and she has presented her research at numerous international conferences (e.g., the Association for Consumer Research, Society for Consumer Psychology, Society for Personality and Social Psychology, and European Marketing Academy conferences). Her work has won numerous awards, including the prestigious Best Dissertation Proposal Award by the Society for Consumer Psychology and the Best Student Paper Award by the International Association for Conflict Management. From October 2015 to March 2016, she was a visiting research scholar at the Johnson Graduate School of Management, Cornell University. In July 2017, Christilene started working as an Assistant Professor in Marketing at Singapore Management University.

PORTFOLIO

AWARDS, HONORS & GRANTS

Awards
Winner Dissertation Proposal Award, Society for Consumer Psychology, 2015
Winner Best Student Paper, International Association for Conflict Management, 2016
Winner Best Graduate Student Poster, Society for Personality and Social Psychology, 2016
Winner Conference Travel Award (US$500), Society for Personality and Social Psychology, 2016
Winner ERIM Talent Placement Award, Erasmus Research Institute of Management, 2016
Best Paper Proceedings (awarded to ~ 10% of papers), Academy of Management, 2016

Scholarships
RSPCA Honors Scholarship ($25,000), University of Queensland, 2008 – 2009
Australian Postgraduate Award ($20,000), Australian Federal Government, 2010 – 2011
INSEAD PhD Scholarship and Tuition Waiver, 2011 - 2014
Erasmus Trustfonds Scholarship, 2015 – 2016

Honors
First Class Honors (Summa Cum Laude), University of Queensland, 2009

Grants
INSEAD R&D Grant for "Power Meta-Analysis" (€8,900), 2015
INSEAD R&D Grant for "Impact of monetary incentives on WOM persuasiveness" (€5,990), 2016

Fellowships
AMA-Sheth Doctoral Consortium Fellow, 2016
Trans-Atlantic Doctoral Consortium (TADC) Fellow, 2016

PUBLISHED PAPERS & PAPERS UNDER REVIEW

1. Tierney et al (2016), "Data from a pre-publication independent replication initiative examining ten moral judgement effects." *Scientific Data*, 3, 160082.

2. Schweinsberg et al (2016), "Pre-Publication Independent Replications of a Single Laboratory's Research Pipeline," *Journal of Experimental Social Psychology,* 66 (September), 55 – 67.

3. Schaerer, Michael, **Christilene du Plessis**, Andy Yap and Stefan Thau, "Preoccupied with the Powerful: A Quantitative Review of Experimental Designs, Attribution of Results, and Effect Sizes in Social Power Research." *Revising for 3rd round review at Organizational Behavior and Human Decision Processes*.
   ****Winner Best Student Paper, International Association for Conflict Management, 2016*
   ****Winner Best Graduate Student Poster, SPSP, 2016*
   ****Best Paper Proceedings (awarded to ~10% of papers), Academy of Management, 2016*

WORKING PAPERS

4. **Du Plessis, Christilene** and David Dubois, "Paying Peanuts Limits Legitimacy: When and Why Monetary Incentives Affect Review Generation and Reception."

5. **Du Plessis, Christilene**, Andrew T. Stephen, Yakov Bart and Dilney Goncalves, "When Paying Does (Not) Pay Off: When and Why Incentive Disclosure Lowers Product Evaluations."

6. **Du Plessis, Christilene**, David Dubois and Michael Schaerer, "La vie en Rose at the top? Why Positive (Negative) Information Goes Up (Down) in Power Hierarchies."

7. Schaerer, Michael, **Christilene du Plessis** and Adam Galinsky, "Power and Risk Revisited: Risk-taking as an Affordance of Power and a Pathway to Power?"

8. Tuk, Mirjam, **Christilene du Plessis**, and Steven Sweldens, **"**From Physical to Psychological Restraint: Restraint-Based Visceral Drives Reduce Impulsive Preferences."

9. Schaerer, Michael, **Christilene du Plessis** and Adam Galinsky, "Power and Risk Revisited: Risk-taking as an Affordance of Power and a Pathway to Power?"

SELECTED WORK IN PROGRESS

10. **Du Plessis, Christilene,** Steven Sweldens and Stijn van Osselaer, "How Distraction Improves Consumer Learning of Brand Associations."
    ****Winner Dissertation Proposal Award, Society for Consumer Psychology, 2015*

CHAIRED CONFERENCE SESSIONS

Du Plessis, Christilene (2017), "When Consumer Multitasking Emerges and How It Shapes Consumer Behavior," *Society for Consumer Psychology Conference,* San Francisco, California.

Du Plessis, Christilene (2016), "When Consumer Multitasking Emerges and How It Shapes Consumer Behavior," *Association for Consumer Research North America Conference,* Berlin, Germany.

Du Plessis, Christilene (2016), "Context Effects in Word-of-Mouth (WOM): How Innocuous Characteristics of Message Generation Influence Sharing and Persuasiveness," *Society for Consumer Psychology Conference,* St. Pete Beach, Florida.

CONFERENCE PRESENTATIONS (* = presenter)

Du Plessis, Christilene* and David Dubois (2017), "Paying Peanuts Limits Legitimacy: When and Why Monetary Incentives Affect Review Generation and Reception." Paper presented at the *La Londe Conference Marketing Communications and Consumer Behavior,* La Londe les Maures, France, competitive paper.

Du Plessis, Christilene* and David Dubois (2017), "Paying Peanuts Limits Legitimacy: When and Why Monetary Incentives Affect Review Generation and Reception." Paper presented at *EMAC*, Groningen, The Netherlands, special session.

Du Plessis, Christilene*, Steven Sweldens and Stijn van Osselaer (2017), "How Distraction Improves Consumer Learning of Brand Associations." Paper to be presented at the *Society for Consumer Psychology Conference,* San Francisco, California, special session.

Du Plessis, Christilene*, Steven Sweldens and Stijn van Osselaer (2016), "How Distraction Improves Consumer Learning of Brand Associations." Paper presented at the *Association for Consumer Research North America Conference,* Berlin, Germany, special session.

Du Plessis, Christilene*, Andrew T. Stephen, Yakov Bart and Dilney Goncalves (2016), "Reducing Uncertainty by Increasing It: How Inducing Uncertainty in Uncertainty Appraisals of Reviewer Trustworthiness Attenuates Bias Correction and Product Devaluation." Paper presented at the *Association for Consumer Research North America Conference*, Berlin, competitive paper.

Schaerer, Michael*, **Christilene du Plessis**, Andy Yap and Stefan Thau (2016), "What about the Powerless? Theoretical and Empirical Consequences of Science's Obsession with the Powerful." Paper presented at the *2016 Academy of Management Annual Meeting*, Anaheim, divisional (Organizational Behavior) paper session.

Du Plessis, Christilene*, Andrew T. Stephen, Yakov Bart and Dilney Goncalves (2016), "Reducing Uncertainty by Increasing It: How Inducing Uncertainty in Uncertainty Appraisals of Reviewer Trustworthiness Attenuates Bias Correction and Product Devaluation." Paper presented at the *2016 London Business School Trans-Atlantic Doctoral Consortium*, London, full paper presentation.

Schaerer, Michael*, **Christilene du Plessis**, Andy Yap and Stefan Thau (2016), "What about the Powerless? Theoretical and Empirical Consequences of Science's Obsession with the Powerful." Paper presented at the *2016 London Business School Trans-Atlantic Doctoral Consortium*, London, full paper presentation.

Schaerer, Michael*, **Christilene du Plessis**, Andy Yap and Stefan Thau (2016), "What about the Powerless? Theoretical and Empirical Consequences of Science's Obsession with the Powerful." Paper presented at the *2016 Annual Conference of the International Association for Conflict Management*, New York City, full paper presentation.

Du Plessis, Christilene*, Steven Sweldens and Stijn van Osselaer (2016), "How Distraction Improves Consumer Learning of Brand Associations." *Society for Consumer Psychology Conference,* St. Pete Beach, FL.

***Part of the "Special Awards Session" with the SCP Fellow, Early Career Award, and Dissertation Proposal Award Winner.*

Du Plessis, Christilene* and David Dubois (2016), "Paying Peanuts Limits Legitimacy: When and Why Monetary Incentives Affect Review Generation and Reception." Paper presented at the *Society for Consumer Psychology Conference*, St. Pete Beach, FL, special session.

Du Plessis, Christilene*, Andrew T. Stephen, Yakov Bart and Dilney Goncalves (2016), "Reducing Uncertainty by Increasing It: How Inducing Uncertainty in Uncertainty Appraisals of Reviewer Trustworthiness Attenuates Bias Correction and Product Devaluation." Paper presented at the *Society for Consumer Psychology Conference*, St. Pete Beach, FL, poster.

Du Plessis, Christilene, Andrew T. Stephen, Yakov Bart* and Dilney Goncalves (2016), "Reducing Uncertainty by Increasing It: How Inducing Uncertainty in Uncertainty Appraisals of Reviewer Trustworthiness Attenuates Bias Correction and Product Devaluation." Paper presented at the *American Marketing Association Winter Educators Conference*, Las Vegas, NV, special session.

Schaerer, Michael, **Christilene du Plessis***, Andy Yap and Stefan Thau (2016), "What about the Powerless? Theoretical and Empirical Consequences of Science's Obsession with the Powerful." Poster presented at the *Society for Personality and Social Psychology Conference*, San Diego.

Du Plessis, Christilene* and David Dubois (2015), "Paying Peanuts Limits Legitimacy: When and Why Monetary Incentives Affect Review Generation and Reception." Paper presented at the *Association for Consumer Research North American Conference*, New Orleans, special session.

Du Plessis, Christilene* and David Dubois (2015), "La vie en Rose at the top? Why Positive (Negative) Information Goes Up (Down) in Power Hierarchies." Paper presented at the *Society for Consumer Psychology Winter Conference*, Phoenix, special session.

Stephen, Andrew*, **Christilene du Plessis**, Yakov Bart Dilney Goncalves (2014), "When Does Paying for Online Product Reviews Pay Off? The Doubt-Inducing Effects of Monetary Incentives on Consumers' Product Evaluations." Paper presented at *American Marketing Association Winter Educators Conference*, Orlando, special session.

Du Plessis, Christilene* and David Dubois (2013), "La vie en Rose at the top? Why Positive (Negative) Information Goes Up (Down) in Power Hierarchies." Paper presented at the *Association for Consumer Research North American Conference*, Chicago, special session.

Stephen, Andrew T., **Christilene du Plessis***, Yakov Bart and Dilney Goncalves (2012), "Does Paying for Online Product Reviews Pay Off? The Effects of Monetary Incentives on Content Creators and Consumers." Paper presented at *Advertising and Consumer Psychology Conference*, Singapore, special session.

Stephen, Andrew, Yakov Bart, **Christilene du Plessis*** and Dilney Goncalves (2012), "Does Paying for Online Product Reviews Pay Off? The Effects of Monetary Incentives on Content Creators and Consumers." Paper presented at *Association for Consumer Research North American Conference*, Vancouver, special session.

INVITED TALKS

"Paying Peanuts Limits Legitimacy: When and Why Monetary Incentives Affect Review Generation and Reception."

- Singapore Management University, 2017

- Erasmus University, 2015

"How Distraction Improves Consumer Learning of Brand Associations."
- Cornell University, 2016
- Polish Academy of Sciences, 2015

"Does Paying for Online Product Reviews Pay Off? The Effects of Monetary Incentives on Content Creators and Consumers."
- ESSEC, 2013
- INSEAD, 2012

TEACHING EXPERIENCE

Instructor and Bachelor Thesis Supervisor, Erasmus University
Bachelor Thesis Research Training, undergraduate course, 2015 & 2017

Teaching Assistant, INSEAD
Brand Management (with Pierre Chandon), MBA elective course, 2013
Marketing Management (with Hilke Plassmann), MBA core course, 2012
Social Media (with David Dubois), MBA elective course, 2011

Tutor, University of Queensland
Foundations of Advertising, undergraduate elective course, 2009 – 2010
Advertising Management, undergraduate elective course, 2009 – 2010
Business Research Methods, undergraduate core course, 2009 – 2010
Consumer Behavior, undergraduate elective course, 2009
Strategic Marketing, undergraduate elective course, 2008
Integrated Marketing Communications, undergraduate elective course, 2008

PHD COURSEWORK

Marketing
Consumer Behavior A (with Amitava Chattopadhyay)
Consumer Behavior B (with Monica Wadwah)
Consumer Decision Making (with Ziv Carmon)
Marketing Models B: Empirical Models (with Yakov Bart and Hernan Bruno)
Marketing Management B: Marketing Strategy (with Hubert Gatignon)

Behavioral Sciences
Behavioral Decision Theory (with Neil Bearden)

Behavioral Science A: Social Psychology (with Zoe Kinias)
Behavioral Science B: Social Theory (with Henrich Greve)
Social Psychological Foundations of Management (with Steven Sweldens)

Methods and Quantitative Coursework
Econometrics A & B (with Amine Ouazad)
Experimental Design (with Neal Bearden and Steven Sweldens)
Microeconomics A (with Timothy Van Zandt)
Microeconomics C (with Morten Bennedsen)
Multivariate Statistics A & B (with Hubert Gatignon)
Network Analysis A (with Jarrett Spiro)
Probability and Statistics A & B (with Ilia Tsetlin)
Research Methods (with Francis De Véricourt)

SERVICE TO THE FIELD

Trainee Reviewer, Journal of Consumer Research
Reviewer, Association for Consumer Research North America Conference, 2013 – 2016
Reviewer, Society for Consumer Psychology Winter Conference, 2015 – 2016
Reviewer, American Marketing Association Winter Conference, 2014
Reviewer, IACM Conference, 2016

PROFESSIONAL AFFILIATIONS

Association for Consumer Research (ACR)
American Marketing Association (AMA)
Society for Consumer Psychology (SCP)
Society for Personality and Social Psychology (SPSP)

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Dissertations in the last five years

Abbink, E.J., *Crew Management in Passenger Rail Transport*, Promotors: Prof. L.G. Kroon & Prof. A.P.M. Wagelmans, EPS-2014-325-LIS, http://repub.eur.nl/pub/76927

Acar, O.A., *Crowdsourcing for Innovation: Unpacking Motivational, Knowledge and Relational Mechanisms of Innovative Behavior in Crowdsourcing Platforms*, Promotor: Prof. J.C.M. van den Ende, EPS-2014-321-LIS, http://repub.eur.nl/pub/76076

Akemu, O., *Corporate Responses to Social Issues: Essays in Social Entrepreneurship and Corporate Social Responsibility,* Promotors: Prof. G.M. Whiteman & Dr S.P. Kennedy, EPS-2017-392-ORG, https://repub.eur.nl/pub/95768

Akin Ates, M., *Purchasing and Supply Management at the Purchase Category Level: Strategy, structure and performance*, Promotors: Prof. J.Y.F. Wynstra & Dr E.M. van Raaij, EPS-2014-300-LIS, http://repub.eur.nl/pub/50283

Akpinar, E., *Consumer Information Sharing*, Promotor: Prof. A. Smidts, EPS-2013-297-MKT, http://repub.eur.nl/pub/50140

Alexander, L., *People, Politics, and Innovation: A Process Perspective*, Promotors: Prof. H.G. Barkema & Prof. D.L. van Knippenberg, EPS-2014-331-S&E, http://repub.eur.nl/pub/77209

Alexiou, A. *Management of Emerging Technologies and the Learning Organization: Lessons from the Cloud and Serious Games Technology*, Promotors: Prof. S.J. Magala, Prof. M.C. Schippers and Dr I. Oshri, EPS-2016-404-ORG, http://repub.eur.nl/pub/93818

Almeida e Santos Nogueira, R.J. de, *Conditional Density Models Integrating Fuzzy and*

Probabilistic Representations of Uncertainty, Promotors: Prof. U. Kaymak & Prof. J.M.C. Sousa, EPS-2014-310-LIS, http://repub.eur.nl/pub/51560

Bannouh, K., *Measuring and Forecasting Financial Market Volatility using High-frequency Data*, Promotor: Prof. D.J.C. van Dijk, EPS-2013-273-F&A, http://repub.eur.nl/pub/38240

Ben-Menahem, S.M., *Strategic Timing and Proactiveness of Organizations*, Promotors: Prof. H.W. Volberda & Prof. F.A.J. van den Bosch, EPS-2013-278-S&E, http://repub.eur.nl/pub/39128

Benschop, N, *Biases in Project Escalation: Names, frames & construal levels*, Promotors: Prof. K.I.M. Rhode, Prof. H.R. Commandeur, Prof. M. Keil & Dr A.L.P. Nuijten, EPS-2015-375-S&E, http://repub.eur.nl/pub/79408

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Beusichem, H.C. van, *Firms and Financial Markets: Empirical Studies on the Informational Value of Dividends, Governance and Financial Reporting*, Promotors: Prof. A. de Jong & Dr G. Westerhuis, EPS-2016-378-F&A, http://repub.eur.nl/pub/93079

Bliek, R. de, *Empirical Studies on the Economic Impact of Trust*, Promotor: Prof. J. Veenman & Prof. Ph.H.B.F. Franses, EPS-2015-324-ORG, http://repub.eur.nl/pub/78159

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Bouman, P., *Passengers, Crowding and Complexity: Models for Passenger Oriented Public Transport*, Prof. L.G. Kroon, Prof. A. Schöbel & Prof. P.H.M. Vervest, EPS-2017-420-LIS, https://repub.eur.nl/

Brazys, J., *Aggregated Marcoeconomic News and Price Discovery*, Promotor: Prof. W.F.C. Verschoor, EPS-2015-351-F&A, http://repub.eur.nl/pub/78243

Byington, E., *Exploring Coworker Relationships: Antecedents and Dimensions of Interpersonal Fit, Coworker Satisfaction, and Relational Models*, Promotor: Prof. D.L. van Knippenberg, EPS-2013-292-ORG, http://repub.eur.nl/pub/41508

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Caron, E.A.M., *Explanation of Exceptional Values in Multi-dimensional Business Databases*, Promotors: Prof. H.A.M. Daniels & Prof. G.W.J. Hendrikse, EPS-2013-296-LIS, http://repub.eur.nl/pub/50005

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Feng, Y., *The Effectiveness of Corporate Governance Mechanisms and Leadership Structure: Impacts on strategic change and firm performance*, Promotors: Prof. F.A.J. van den Bosch, Prof. H.W. Volberda & Dr J.S. Sidhu, EPS-2017-389-S&E, https://repub.eur.nl/pub/98470

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Hogenboom, A.C., *Sentiment Analysis of Text Guided by Semantics and Structure,* Promotors: Prof. U. Kaymak & Prof. F.M.G. de Jong, EPS-2015-369-LIS, http://repub.eur.nl/pub/79034

Hogenboom, F.P., *Automated Detection of Financial Events in News Text*, Promotors: Prof. U. Kaymak & Prof. F.M.G. de Jong, EPS-2014-326-LIS, http://repub.eur.nl/pub/77237

Hollen, R.M.A.*, Exploratory Studies into Strategies to Enhance Innovation-Driven International Competitiveness in a Port Context: Toward Ambidextrous Ports,* Promotors: Prof. F.A.J. Van Den Bosch & Prof. H.W.Volberda, EPS-2015-372-S&E, http://repub.eur.nl/pub/78881

Hout, D.H. van, *Measuring Meaningful Differences: Sensory Testing Based Decision Making in an Industrial Context; Applications of Signal Detection Theory and Thurstonian Modelling*, Promotors: Prof. P.J.F. Groenen & Prof. G.B. Dijksterhuis, EPS-

2014-304-MKT, http://repub.eur.nl/pub/50387

Houwelingen, G.G. van, *Something To Rely On*, Promotors: Prof. D. de Cremer & Prof. M.H. van Dijke, EPS-2014-335-ORG, http://repub.eur.nl/pub/77320

Hurk, E. van der, *Passengers, Information, and Disruptions*, Promotors: Prof. L.G. Kroon & Prof. P.H.M. Vervest, EPS-2015-345-LIS, http://repub.eur.nl/pub/78275

Iseger, P. den, *Fourier and Laplace Transform Inversion with Applications in Finance*, Promotor: Prof. R. Dekker, EPS-2014-322-LIS, http://repub.eur.nl/pub/76954

Jaarsveld, W.L. van, *Maintenance Centered Service Parts Inventory Control*, Promotor: Prof. R. Dekker, EPS-2013-288-LIS, http://repub.eur.nl/pub/39933

Khanagha, S., *Dynamic Capabilities for Managing Emerging Technologies*, Promotor: Prof. H.W. Volberda, EPS-2014-339-S&E, http://repub.eur.nl/pub/77319

Khattab, J., *Make Minorities Great Again: a contribution to workplace equity by identifying and addressing constraints and privileges*, Prof. D.L. van Knippenberg & Dr A. Nederveen Pieterse, EPS-2017-421-ORG, https://repub.eur.nl/pub/99311

Kil, J., *Acquisitions Through a Behavioral and Real Options Lens*, Promotor: Prof. H.T.J. Smit, EPS-2013-298-F&A, http://repub.eur.nl/pub/50142

Klooster, E. van't, *Travel to Learn: the Influence of Cultural Distance on Competence Development in Educational Travel*, Promotors: Prof. F.M. Go & Prof. P.J. van Baalen, EPS-2014-312-MKT, http://repub.eur.nl/pub/51462

Koendjbiharie, S.R., *The Information-Based View on Business Network Performance: Revealing the Performance of Interorganizational Networks*, Promotors: Prof. H.W.G.M. van Heck & Prof. P.H.M. Vervest, EPS-2014-315-LIS, http://repub.eur.nl/pub/51751

Koning, M., *The Financial Reporting Environment: The Role of the Media, Regulators and Auditors*, Promotors: Prof. G.M.H. Mertens & Prof. P.G.J. Roosenboom, EPS-2014-330-F&A, http://repub.eur.nl/pub/77154

Konter, D.J., *Crossing Borders with HRM: An Inquiry of the Influence of Contextual Differences in the Adoption and Effectiveness of HRM*, Promotors: Prof. J. Paauwe & Dr L.H. Hoeksema, EPS-2014-305-ORG, http://repub.eur.nl/pub/50388

Korkmaz, E., *Bridging Models and Business: Understanding Heterogeneity in Hidden Drivers of Customer Purchase Behavior*, Promotors: Prof. S.L. van de Velde & Prof. D. Fok, EPS-2014-316-LIS, http://repub.eur.nl/pub/76008

Krämer, R., *A license to mine? Community organizing against multinational corporations*, Promotors: Prof. R.J.M. van Tulder & Prof. G.M. Whiteman, EPS-2016-383-ORG, http://repub.eur.nl/pub/94072

Kroezen, J.J., *The Renewal of Mature Industries: An Examination of the Revival of the Dutch Beer Brewing Industry*, Promotor: Prof. P.P.M.A.R. Heugens, EPS-2014-333-S&E, http://repub.eur.nl/pub/77042

Kysucky, V., *Access to Finance in a Cros-Country Context*, Promotor: Prof. L. Norden, EPS-2015-350-F&A, http://repub.eur.nl/pub/78225

Lee, C.I.S.G, *Big Data in Management Research: Exploring New Avenues*, Promotors: Prof. S.J. Magala & Dr W.A. Felps, EPS-2016-365-ORG, http://repub.eur.nl/pub/79818

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Leunissen, J.M., *All Apologies: On the Willingness of Perpetrators to Apologize*, Promotors: Prof. D. de Cremer & Dr M. van Dijke, EPS-2014-301-ORG, http://repub.eur.nl/pub/50318

Li, D., *Supply Chain Contracting for After-sales Service and Product Support*, Promotor: Prof. M.B.M. de Koster, EPS-2015-347-LIS, http://repub.eur.nl/pub/78526

Li, Z., *Irrationality: What, Why and How*, Promotors: Prof. H. Bleichrodt, Prof. P.P. Wakker, & Prof. K.I.M. Rohde, EPS-2014-338-MKT, http://repub.eur.nl/pub/77205

Liu, N., *Behavioral Biases in Interpersonal Contexts*, Supervisors: Prof. A. Baillon & Prof. H. Bleichrodt, EPS-2017-408-MKT, https://repub.eur.nl/pub/95487

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Liket, K., *Why 'Doing Good' is not Good Enough: Essays on Social Impact Measurement*, Promotors: Prof. H.R. Commandeur & Dr K.E.H. Maas, EPS-2014-307-STR, http://repub.eur.nl/pub/51130

Loos, M.J.H.M. van der, *Molecular Genetics and Hormones: New Frontiers in Entrepreneurship Research*, Promotors: Prof. A.R. Thurik, Prof. P.J.F. Groenen, & Prof. A. Hofman, EPS-2013-287-S&E, http://repub.eur.nl/pub/40081

Lu, Y., *Data-Driven Decision Making in Auction Markets*, Promotors: Prof. H.W.G.M. van Heck & Prof. W. Ketter, EPS-2014-314-LIS, http://repub.eur.nl/pub/51543

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Mell, J.N., *Connecting Minds: On The Role of Metaknowledge in Knowledge Coordination*, Promotor: Prof. D.L. van Knippenberg, EPS-2015-359-ORG, http://hdl.handle.net/1765/78951

Meulen,van der, D., *The Distance Dilemma: the effect of flexible working practices on performance in the digital workplace*, Promotors: Prof. H.W.G.M. van Heck & Prof. P.J. van Baalen, EPS-2016-403-LIS, http://repub.eur.nl/pub/94033

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Social influence is the corner stone of consumer psychology. In fact, in the last decade of the 19th century the study of consumer psychology emerged from an interest in advertising and its influence on people. Traditionally research on social influence has focused on understanding how people respond to influence attempts and how social influence emerges. This dissertation challenges common methodological conventions used to study social influence in consumer behavior and, more broadly, social psychology.

The first part of this dissertation moves beyond the study of social influence in a single dyadic relationship, and investigates how one dyadic relationship influences another. Here, influence attempts by one agent (e.g., a company) on another (e.g., a review writer) are shown to not only influence the cognitions of the agent being influenced, but also their ability to influence others (e.g., review readers) in turn. The second part of this work investigates how the widespread use of 2-cell instead of 3-cell experimental designs in social power research limits understanding of both the powerful and powerless. The pervasive practice of contrasting high power to either a low power or a control condition is found to weaken construct validity and inflate the size of effects attributed to high power. In contrast, using a 3-cell experimental design facilitates theoretical advancement by enabling the identification of curvilinear effects.

The findings in this dissertation highlight the need for scholars in consumer behavior, and social psychologists more broadly, to question common methodological conventions used to study social influence. Overall, this research suggests that doing so may not only increase methodological rigor and confidence in observed effects, but may also lead to novel theoretical insights.

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