Believability: A Study of Coincidence and Scarcity in Consumer Behavior

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ACCEPTANCE

This dissertation was prepared under the direction of the Gregory Scott Cohen’s Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Business Administration in the J. Mack Robinson College of Business of Georgia State University.

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ABSTRACT

Believability: A Study of Coincidence and Scarcity in Consumer Behavior

BY

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A central aspect of firm-consumer communications is the extent to which the consumer believes the firm. In this dissertation, I argue that belief, more specifically a consumer’s belief in claims or material generated by the firm, should be more adequately examined in contemporary consumer behavior literature. To this end, I review current theories that attempt to understand belief in consumer behavior literature. Furthermore, I explore two empirical examinations of how certain factors influence consumers’ belief in firm-generated communication and how differing levels of belief affect consumer behavior—first in the service failure context and second in the scarcity signaling context.
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CHAPTER 1:

Introduction to Believability

A central aspect of firm-consumer communications is the extent to which the consumer believes the firm. In this dissertation, I argue that belief, more specifically a consumer’s belief in claims or material generated by the firm, should be more adequately examined in contemporary consumer behavior literature. To this end, I will review current theories that attempt to understand belief in consumer behavior literature. Furthermore, I will explore two empirical examinations of how certain factors influence consumers’ belief in firm-generated communication and how differing levels of belief affect consumer behavior—first in the service failure context and second in the scarcity signaling context. The goal is to better understand the relationship between belief and consumer behavior, and, in particular, how belief can be managed more appropriately from both a managerial and a consumer standpoint. If I can show that belief affects consumer interactions with the firm, we will be better able to understand and control this latent construct, which has been underexplored.

BELIEF THEORY

Belief is a broad topic—Merriam-Webster defines belief as, “a state or habit of mind in which trust or confidence is placed in some person or thing” (Merriam-Webster 2017). This definition highlights many key elements of belief—first, it qualifies belief as a habit. Much of academic literature, at least literature which is psychological in nature, studies habits in one form or another. Consumer behavior researchers are familiar with habits (Campbell and Cochrane
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1999; Ouellette and Wood 1998). Therefore, if we think of belief as simply a habit, the topic is not as daunting as one might initially assume. Belief is just the habit of people to place trust in something. This ideology might portray belief as in a more long-term inflexible construct—for example, believing in a higher-power. However, this research focuses on belief in the veracity of a message or communication. This could be long-term (i.e. I believe my wife tells me the truth) or short term (i.e. I don’t believe she told me the truth, yesterday).

The second key element of this definition is the “thing”, or object of belief, that we are placing trust in is subjective. For example, if a consumer buys an item via an online marketplace that hosts a variety of sellers and reviews, whom are they placing their belief in? One might argue that they believe in the marketplace, the sellers, the reviewers, and the capitalist system as a whole (Gefen, Karahanna, and Straub 2003). Although habits are generalizable, people will not generally believe an individual unless given reason to place their trust in that individual.

Belief has a bidirectional relationship with trust. Oftentimes, these two terms are thought of as synonymous. However, belief is more transient in nature than trust. Typically, through repeated demonstrating the belief in a person or a thing is well placed, people develop trust. Or someone is trusted, they can most likely be believed. It is worth noting that throughout this essay, this form of belief is paramount. Specifically, do consumers believe this statement/claim/firm/representative?

MARKETING THEORY

Although, some people might have general tendencies to believe others, or general tendencies to believe specific firms, it would be erroneous to assume that all consumers inherently believe claims generated by the firm. In fact, any context in which a firm might
benefit from dishonesty is one in which consumer disbelief may arise (Fein and Hilton 1994; Fein 1996; Fein, Hilton, and Miller 1990; Main, Dahl, and Darke 2007). This is the fundamental ideology behind one of the most influential works in consumer behavior—The Persuasion Knowledge Model (PKM). PKM investigates consumers’ awareness about the strategies and tactics that firms employ to convince them to buy a product and the motives behind such strategies (Friestad and Wright 1994). This model begins to address consumers’ personal knowledge related to firms’ goals and tactics and the skepticism, or disbelief, which might arise from the use of such strategies. The PKM states that when confronted with a persuasion attempt, consumers recall and refine their knowledge on persuasive tactics to generate the appropriate response to the persuasive messages. Simultaneously, consumers dynamically adjust their attitudes toward the product and the firm based on the persuasive appeal and consumers’ knowledge associated with such appeals (1994).

Surprisingly, belief is often either ignored or taken for granted in research in non-persuasive firm-consumer communication domains such as social media (Kaplan and Haenlein 2010; Kietzmann et al. 2011), customer service (Blodgett, Wakefield, and Barnes 1995), and service failure recovery (Folkes 1984; Tsiros, Mittal, and Ross 2004; Umashankar, Srinivasan, and Parker 2016). However, perceptions of being deceived, regardless of reality, can create intense negative emotional reactions (Levine, McCornack, and Avery 1992; McCornack and Levine 1990) and can lead to resentment, disappointment, and suspicion (Bok 2011).

Furthermore, research shows that a brand’s credibility affects the formation of consideration sets (Erdem and Swait 2004). Therefore, a firm that is prone to unbelievable communications will undoubtedly see an impact in the willingness of consumers to trust them but also purchase from them, effecting market share and profit margins (Chaudhuri and Holbrook 2001).
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There are two forms of belief in marketing research—belief as a function of source credibility and belief as a function of information characteristics. Source credibility refers to the expertise, trustworthiness, or objectivity of the source of information. A credible source of information can give consumers confidence that the information is accurate. For example, a consumer that is not familiar with a product can be easily persuaded of its quality by a credible source (Ratneshwar and Chaiken 1991). There are a plethora of ways to manipulate a source’s credibility, from something as simple as altering their likeability by hiring a celebrity to deliver the message to more complex utilizations of social media to make it appear that a brand is endorsed by consumers’ personal acquaintances. In the following research, I am interested in belief as a function of source credibility and belief as a function of information characteristics. If the information is presented in a certain way, it can alter the likelihood that a consumer will believe the message. For example, at the end of most drug advertisements, the potential side effects of the drug are listed rapidly (i.e., Diarrhea, Nausea, and Death). Given that these are so quickly presented, it might indicate that the messenger is untrustworthy and has underhanded motives (Herbst et al. 2011). Throughout the next three chapters of this dissertation, many examples will be given of this form of belief manipulation.

RESEARCH

In the second chapter of this dissertation, I examine the extent to which consumers’ beliefs of the claims made by a point of contact (POC) evolve over repeated service failures. I draw on literature from deception detection (Ekman and Friesen 1969; Ekman 2009) and coincidence (Dessalles 2006; Dessalles 2008; Dimulescu and Dessalles 2009) to propose that consumers are more likely to believe a POC when the POC gives different (vs. the same) reasons for each of the service failures. Specifically, this dissertation aims to establish that ostensible
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coincidences (the same reason given repeatedly for service failures) provided by the POC lead consumers to discount these explanations for the service failures. The second chapter of this dissertation examines the idea that consumers find ostensible coincidences less believable despite that such coincidences suggest a systemic cause for the service failure that is both more likely and more expected by the consumer a priori. Further, when presented with an ostensible coincidence, consumers are more likely to blame the POC for the service failure.

In the third chapter of this dissertation, I conduct research that outlines the extent to which consumers’ beliefs of scarcity signals presented by a firm differ based on the manner in which those signals are delivered. Drawing on literature from creativity (Goldenberg, Mazursky, and Solomon 1999a, 1999b) and persuasion (Friestad and Wright 1994; Chaiken et al. 1987; Petty and Cacioppo 1986), I theorize that consumers are more likely to believe implicit (vs. explicit) scarcity signals. Specifically, implicit scarcity signals require the consumer to engage in the communication and alter the perceived source and intention of the scarcity signal. The consumer will no longer feel the source of the scarcity signal is the firm and that the intentions are to persuade the consumer. Further, when presented with an implicit scarcity signal, consumers should be more likely to believe the signal is authentic and alter their purchasing behavior to match previous scarcity literature.
CHAPTER 2:

“You're Not Going to Believe This: Ostensible Coincidences Reduce Belief in Reasons Given for Service Failures”

A critical concern of managers is their firms’ communications with consumers, which can range in scope from mass-media advertisements down to one-to-one communications between consumers and the firm’s customer-service representatives. A central aspect of firm-consumer communications is the extent to which the consumer believes the firm’s claims. It is unsurprising then that belief is one of the primary latent constructs of interest in much research on persuasion (Friestad and Wright 1995) and advertising (Mitchell and Olson 2000). However, belief is often either ignored or taken for granted in research on other firm-consumer communication domains such as social media (Kaplan and Haenlein 2010; Kietzmann et al. 2011), customer service (Blodgett, Wakefield, and Barnes 1995), and service failure recovery (Folkes 1984; Tsiros, Mittal, and Ross 2004; Umashankar, Srinivasan, and Parker 2016). Yet, it would be erroneous to assume that consumers inherently believe claims generated by the firm, or one of its representatives, in such non-persuasion contexts. In fact, any context in which an entity (the firm) might benefit from dishonesty is one in which consumer disbelief may arise (Fein and Hilton 1994; Fein 1996; Fein, Hilton, and Miller 1990; Main, Dahl, and Darke 2007). Here I focus on firm-consumer interactions after service failures and ask: To what extent do consumers believe the reasons firms give them for service failures?

Generally speaking, research on service failures and recoveries treats the brand as a single, holistic entity. However, in reality, firms are comprised of numerous employees that
differ in the amount of direct contact they have with the firm’s customers. Moreover, consumers interact with individual employees, not abstract firms, brands, or companies. Accordingly, the current research examines interactions between the consumer and a firm’s representative. More specifically, this essay examine consumers’ belief of the claims made by a representative of the firm after each of several minor but repeated service failures.

Be it for reasons of convenience (Mitchell 1979), loyalty (Jacoby and Chestnut 1978), or habit (Hoyer 1984), consumers often repeatedly do business with the same firm (e.g., restaurant, store, or website) from whom they purchase the same or similar items (e.g., products, services, or both). It is not uncommon for recurring firm-consumer relationships to involve repeated interactions between the consumer and the same firm representative (hereafter, point-of-contact or POC) over time. For instance, consumers regularly interact the same salesperson (POC) when making repeated purchases from a car dealership (e.g., when buying multiple cars for spouses and children). Likewise, companies such as Dell typically establish a single customer-service or technical-support representative (POC) for customers making purchases or seeking technical assistance. These POCs walk the consumer through the entire process and are often designated as the POC for future interactions, should the need arise. A more ubiquitous example is the consumer that frequents a specific restaurant; often that consumer will be waited on by the same server (POC) on numerous occasions. In short, it is not only common for consumers to repeatedly interact with specific firms, but also with specific POCs within those firms.

Recurring firm-consumer relationships are typically sustained by positive experiences with the firm, but repeated interactions also allow for the possibility of repeated, relatively minor service failures (e.g., long wait times, incorrect orders, or rude service). While minor failures are not ideal, they are less likely to result in the immediate termination of the firm-consumer
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relationship than more severe failures (Smith, Bolton, and Wagner 1999) and, therefore, are more likely to be tolerated, *ceteris paribus*. Thus, the context of repeated service failures is not a contrived one, and has received some attention in previous literature (Maxham III and Netemeyer 2002). In many, if not most, cases service failures are followed by communication between the firm’s POC and the consumer, which is likely to involve an explanation of the failure and, where appropriate, an apology from the firm (Folkes 1984). The question examined here is to what extent does the consumer believe the POC’s explanation for the failure and how does that belief evolve over repeated service failures?

Consider the following simple yet familiar scenario. A consumer has dined at a restaurant regularly for some time. Generally, the experience has been pleasant but this consumer has had to wait an excessive amount of time for her food on her last three visits, during each of which the consumer was waited on by the same server. The consumer could reasonably believe these delays were caused by either the server or the kitchen staff. Yet, only the server is in direct contact with the consumer. Thus, the ostensible reasons—the excuses, if you will—for the delayed meals will come from the server, not the kitchen staff. For simplicity, imagine that the server indicates that the delay is the fault of the kitchen in all three instances. With this context in mind, the focal question is whether *ostensible causal coincidence* (i.e., the same reason is given by the same source—the server in this example—for repeated service failures) versus *ostensible causal discordance* (i.e., different reasons are given by the same source for each of the service failures) in those reasons influence the extent to which the consumer believes them (and, equivalently, their source). In other words, are consumers more likely to believe the server if they give the same reason for all three failures (e.g., “The kitchen forgot your order.”) or
different reasons for each failure (e.g., “The kitchen forgot the order,” “The kitchen staff made the order incorrectly,” and “The kitchen staff dropped the order.”)?

Objectively speaking, it could be argued that a single reason for multiple failures is more believable as it would signal a systemic issue, which parsimoniously explains the repeated service failures. Moreover, it seems more likely that a repeat consumer could be the recurrent victim of a systemic issue than the victim of three distinct, if not necessarily independent, events generating similar consequences (excessive waits in the above example). Yet, I demonstrate that consumers are more likely to believe the server (POC) when the server (POC) gives different (vs. the same) reasons for each of the service failures. I argue that consumers’ intuitive lay beliefs about ostensible coincidences lead them to discount or neglect parsimonious explanations (a systemic issue) for repeated service failures. Instead, for reasons detailed in this paper, consumers find coincidences to be unexpected and, therefore, should find ostensible coincidences less believable. Consequently, the consumer is less likely to believe the POC (the server in the above example) and, in fact, more likely to blame the POC for the service failure.

**THEORY**

Scant research has examined how consumers cope with the uncertainty inherently surrounding the causes of service failures. Indeed, most service failure research either (i) takes it as a given that the cause of the failure is unambiguously known and examines factors influencing how consumers respond to the failure and subsequent recovery efforts (Smith, Bolton, and Wagner 1999; Maxham III and Netemeyer 2002) or (ii) examines the extent to which consumers attribute the blame for the failure to the firm versus themselves (Folkes 1984; Pham et al. 2010; Tsiros, Mittal, and Ross 2004). However, in reality, consumers are likely to recognize that there may exist a disconnect between what they are told and what actually caused a service failure.
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Hence, consumers may have grounds to disbelieve reasons they are given for failures, which may influence which individuals or entities within the firm they blame for the failure.

Notably, the reasons given for a service failure commonly originate from a single point-of-contact (POC) within the firm. The POC is typically responsible for many aspects of successfully delivering the firm’s product or service. Concurrently, there is typically a group of individuals responsible for the production or creation of the firm’s services or products that do not interact with the customer directly. Thus, when a service failure occurs, it may be the fault of the POC or of another employee or group within the firm.

The current work examines the context of repeated minor service failures, each of which is followed by the firm’s POC providing an ostensible cause for the failure. I am particularly interested in situations in which the repeated service failures could plausibly have been caused by the actions of the POC, but the POC attributes the causes of those failures to employees or groups other than themselves. Such instances are interesting because the POC has obvious motives for deflecting blame if he or she personally caused the service failures (e.g., avoiding customer complaints or anger directed at them), but could also be telling the truth when indicating that others in the firm caused the failures. Hence, in these circumstances, the consumer may either choose to believe the POC or, instead, infer that the POC is deflecting blame. The question then is when and why would the consumer be more or less likely to believe the ostensible causes provided by the POC? Part of the answer to that question lies in research on individuals’ abilities to detect deception.

Deception Detection. Lying is a common occurrence—people lie one to two times a day on average (DePaulo and Kashy 1998)—and perceptions of being deceived, regardless of reality, can create intense negative emotional reactions (Levine, McCormack, and Avery 1992;
McCornack and Levine (1990) and can lead to resentment, disappointment, and suspicion (Bok 2011). Yet, “behavioral cues that are discernible by human perceivers are associated with deceit only probabilistically” (DePaulo et al. 2003). Accordingly, substantial research has focused on the cues that people use to determine if someone is being deceptive (e.g., Trovillo 1939).

Much of the research on deception detection centers on verbal and nonverbal cues of deception (Zuckerman, DePaulo, and Rosenthal 1981). Eye contact (Kleinke 1986), tone of voice (Anolli and Ciceri 1997), and facial expressions (Ekman 2003) can all indicate that deception may be occurring (Ekman 2009). However, deception detection research also examines cues related to the content of the potential deception, such as the amount of detail in a story (Burgoon et al. 2003), the wordiness of a story (Zhou et al. 2004), or the compelling nature of the story (Mehrabian 1977). Content cues often reveal more insight into what a liar is trying to hide in addition to indicating that they are simply trying to hide something (Ekman and Friesen 1969). Here, I examine a heretofore unexamined content cue: the presence versus absence of coincidence in the message.

**Coincidences: Actual and Ostensible.** In the most objective sense, coincidences are merely the occurrence of two or more things at the same time. However, colloquially, the term “coincidence” conveys a sense of unexpectedness with the co-occurrence. In other words, coincidences are not merely the co-occurrence of any two or more events—they are a co-occurrence of events that was unexpected. To this end, the Cambridge Dictionary formally defines coincidences as events co-occurring “in a way that is unexpected or unlikely.” Dessalles (2006) characterized such unexpected co-occurring events in terms of their generation complexity (How many things needed to happen for the co-occurrence to arise?) and description.
complexity (How difficult is it to describe the co-occurrence?). The distinction between these two types of complexity is best conveyed via an example.

Imagine two friends: Nick who lives in Boulder, Colorado and Dan who lives in Chicago, Illinois. One day, Nick takes a trip to San Francisco and, as he is checking into his hotel, Dan walks up to also check into the same hotel. Assuming that Nick and Dan had not coordinated their travels, it is reasonable to assume that both would be pleasantly surprised by this co-occurrence. It would be quite unexpected, in fact, since neither of these friends lives near San Francisco. Now, think of the complexity of generating this co-occurrence. Both friends would have needed to independently decide to travel to San Francisco, plan travel accommodations from their respective home cities, choose to stay at the same hotel, and decide to travel on the same date. Accordingly, each friend’s reasoning in terms of location and timing are also necessary to generate this co-occurrence. In sum, it is no simple matter that Nick and Dan arrived at this same hotel at the same time. As they say, “What are the odds?” However, an observer would simply need to say, “Nick and Dan were booked at the same hotel at the same time in San Francisco,” to describe the co-occurrence. Dimulescu and Dessalles (2009) have argued that co-occurrences of this nature—far greater generation complexity than description complexity—are those which generate the unexpectedness so commonly associated with coincidences.

A critical finding of Dassalles’s (2008) is that the unexpectedness of a coincidence is diminished when the apparent generation complexity is reduced via a parsimonious explanation. For instance, if an observer were to learn that Nick and Dan in the preceding example were both attending the same conference, then the generation complexity would be reduced (they’d both be
operating under the same reasoning and motivation), as would the unexpectedness of them both arriving at the same hotel at the same time. I will return to this insight later.

While actual coincidences are interesting, the current work examines coincidences that are conveyed indirectly (i.e., are ostensible) via a firm’s POC (the restaurant server in the earlier example). A critical difference between actual and ostensible coincidences is that the former may be unexpected, but it cannot be denied. In the context of the Nick and Dan example, a witness that saw Nick and Dan arrive at the same hotel, could not deny the co-occurring events. In contrast, while ostensible coincidences should also be unexpected, it is also true that their veracity may be questioned.

Of interest here, are *ostensible causal coincidences*, which are coincidences in which the same (or similar) outcomes for the consumer (e.g., service failures) have been repeatedly attributed to the same cause by a firm’s POC. Critically, it is far from improbable that a systemic issue has caused the consumer to experience these repeated service failures. Indeed, were the cause directly observable, the consumer might readily accept the apparent coincidence given such a parsimonious explanation, as discussed above. However, ostensible coincidences are not observable and, instead, are conveyed by a second party—the POC—reasonably motivated to deflect blame. Accordingly, the consumer has at their disposal two plausible parsimonious explanations for the ostensible coincidence. On the one hand, the service failures may have in fact all been caused by the same systemic issue. On the other hand, they may have been caused by the POC, who has merely deflected blame to others.

I contend that, since coincidences are unexpected, ostensible causal coincidences are perceived as suspicious and consumers are likely to doubt the authenticity of the POC’s claims in such contexts. Therefore, in scenarios of *ostensible causal coincidence* (i.e., the same reason is
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given by the same POC for the same repeated service failures), when the factors of the coincidence are highly aligned across each separate interaction, the consumer will believe the POC less. Conversely, in scenarios of relative ostensible causal discordance (i.e., different reasons are given by the same source for the same repeated service failures), when the factors of the coincidence are less aligned across each separate interaction, the consumer will believe the POC more.

Figure 1: Conceptual Map

I tested the predictions regarding ostensible causal coincidences for repeated service failures across five studies. Study 1 demonstrates that consumers are (i) less likely to believe a POC (a restaurant server) that presents the same reason for late orders on three repeat visits than different reasons and (ii) more likely to blame the server presenting the same reason versus different reasons while (iii) exonerating the kitchen staff of responsibility. Study 2, using the same restaurant context, replicates the findings of Study 1 and concurrently examines how belief in the reasons given for service failures changes over the course of those service failures. Study 3 generalizes these findings to a new context (visiting a hotel that was booked by a travel agent). Study 4 demonstrates that these findings are a product of the level of coincidence by manipulating additional factors (i.e., the number of POCs conveying the reasons for the failures) that can mitigate the apparent coincidence of the situation. Study 5 demonstrates that consumers will predict that a single systemic reason is the most likely cause for repeated service failures, but still do not believe it when the single reason is presented to them by a POC.
STUDIES

Study 1: Coincidence and Deception Detection

In order to investigate the theory that disbelief stems from a coincidence being relayed via a POC, an experiment was developed that placed participants in a hypothetical scenario similar to the restaurant example in the introduction. The goals of this study were to demonstrate the hypothesis that people are less likely to believe a single repeated reason than different reasons for multiple service failures from the same source. Furthermore, people are more likely to blame the source offering them a single (vs. different) reason for multiple service failures. Study 1 further demonstrates that the reason used to explain the delay in service was not the cause of the findings.

Table 1: Example of Reasons for Food Being Late

<table>
<thead>
<tr>
<th>Visit</th>
<th>Date</th>
<th>What your server said about the wait</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>09/01/2015</td>
<td>“The kitchen forgot about your order. I will bring your food out as soon as it’s ready.”</td>
</tr>
<tr>
<td>B</td>
<td>09/15/2015</td>
<td>“A cook dropped your food in the kitchen. I will bring your food out as soon as it’s ready.”</td>
</tr>
<tr>
<td>C</td>
<td>09/30/2015</td>
<td>“The kitchen made a mistake and had to remake your food. I will bring your food out as soon as it’s ready.”</td>
</tr>
</tbody>
</table>

Method. One hundred sixty-three paid participants were recruited from Amazon Mechanical Turk. Twelve participants were removed from the analysis for failing to respond to all questions, leaving one hundred fifty-one participants. Participants were asked to imagine they had visited a local restaurant on three different occasions. During each visit the participants had the same server and had to wait an excessively long time for their orders. Participants were presented with a table, similar to Table 1, that either stated three different reasons for their food being late or one reason for their food being late on each visit. Participants were randomly
assigned to one of four conditions—the first condition presented a scenario identical to Table 1, where a different reason was given for each delay in service (diffReas). The other three conditions presented participants with the same reason given for the delay in service across all visits. Each individual reason presented in the different reasons condition was represented in a single reason condition; the kitchen forgot order (kitchenForgot), a cook dropped the order in the kitchen (cookDrop), and the kitchen made a mistake and had to remake the order (kitchenMistake). An example can be seen in Table 2.

**Table 2: Example of Reasons for Food Being Late (kitchenForgot)**

<table>
<thead>
<tr>
<th>Date</th>
<th>What your server said about the wait</th>
</tr>
</thead>
<tbody>
<tr>
<td>Visit A</td>
<td>“The kitchen forgot about your order. I will bring your food out as soon as it’s ready.”</td>
</tr>
<tr>
<td>09/01/2015</td>
<td></td>
</tr>
<tr>
<td>Visit B</td>
<td>“The kitchen forgot about your order. I will bring your food out as soon as it’s ready.”</td>
</tr>
<tr>
<td>09/15/2015</td>
<td></td>
</tr>
<tr>
<td>Visit C</td>
<td>“The kitchen forgot about your order. I will bring your food out as soon as it’s ready.”</td>
</tr>
<tr>
<td>09/30/2015</td>
<td></td>
</tr>
</tbody>
</table>

Participants then indicated how much they believed the reason given for the delay during each specific visit on an eleven point scale ranging from “completely do NOT believe (-5)” to “completely believe (5).” On another screen participants were asked to indicate their overall belief of the reasons the server gave for their food being late on a similar scale to the one utilized for the prior question. On this same page they were asked to indicate server fault and kitchen fault for the service failure on two separate eleven point scales ranging from “not the server’s (kitchen’s) fault at all (-5)” to “completely the server’s (kitchen’s) fault at all (5).” Participants were also asked why they believed the food was delayed during the visit to the restaurant in an open-ended text based response question. On a separate page, participants were asked an attention check question, their gender, and age prior to thanking them for their participation.
Results. The theory pertains to how belief responds to ostensible coincidence. Hence, this essay does not examine raw belief in the reasons given by the server (e.g., how believable it is that a kitchen forgets an order) but, instead, focus on the evolution of belief. Specifically, one’s belief in the reason given for each service failure should decrease to the extent to which that reason coincides with the reasons given for previously experienced failures. Accordingly, the primary interest lies in how belief in the reasons given for the service failures changed from the first visit (Visit A) to the subsequent visits (Visits B and C).

Belief in the reasons given by the server at each individual visit evolved in a manner consistent with the prediction (Figure 2). A mixed ANOVA was run on the manipulated, four-level, between-subjects “reasons” factor (diffReas vs. kitchenForgot vs. cookDrop vs. kitchenMistake) and the three-level within-subjects repeated measure of belief in the reasons given. The mixed-ANOVA revealed the expected interaction ($F(6, 294) = 12.82, p < .001$), which indicates that participants’ belief in the reasons they were given evolved differently across visits depending on the number of reasons they were given.
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The change in belief from Visit A to Visit B (i.e., belief after Visit B minus belief after Visit A; hereafter, ΔVisit B) varied significantly across conditions (M_{diffReas} = -0.72, M_{kitchenForgot} = -2.73, M_{cookDrop} = -3.55, M_{kitchenMistake} = -2.06, F(3,147) = 8.6, p < .0001). In the interest of brevity, I collapsed the three single-reason conditions and contrasted them with the different-reasons condition (the same results obtain if the different reason condition is independently contrasted with each single reason condition). As expected, ΔVisit B was significantly less negative in the different-reasons (vs. single-reason) condition (F(1,147) = 18.81, p < .0001). An identical analysis of ΔVisit C (i.e., belief after Visit C minus belief after Visit A) revealed the same pattern across all conditions (M_{diffReas} = -0.44, M_{kitchenForgot} = -4.37, M_{cookDrop} = -6.03, M_{kitchenMistake} = -4.36, F(3,147) = 15.72, p < .0001) and after collapsing across the single-reason conditions (F(1,147) = 41.85, p < .0001). Taken together these results show that consumers have progressively less belief in repeated identical reasons for service failures. Said differently, consumers are more likely to believe the reasons they are given for multiple service failures if those reasons differ over the various incidences.

*Figure 3: Mean of Server Belief Across Conditions*
Consistent with the predictions, participants’ overall belief (a single measure collected after participants indicated their belief in the specific reasons given for each service failure) in the server’s reasons also varied significantly across conditions ($M_{\text{diffReas}} = .33$, $M_{\text{kitchenForgot}} = -1.9$, $M_{\text{cookDrop}} = -3.26$, $M_{\text{kitchenMistake}} = -1.75$, $F(3,147) = 10.9$, $p < .0001$). Participant’s overall belief in the three reason condition differed significantly from each single reason condition—the kitchenForgot condition ($F(1,147) = 12.88$, $p < .0006$), the cookDrop condition ($F(1,147) = 32.14$, $p < .0001$), and the kitchenMistake condition ($F(1,147) = 10.50$, $p < .0016$). These results, illustrated in Figure 3, replicate the above visit-specific belief results, but at a more aggregate, overall level of belief.

Discussion. The results of Study 1 support the proposition that consumers are less likely to believe a single (vs. different) reason presented to them for multiple service failures. Study 1 also starts to develop a more compelling narrative around the observed phenomenon by examining how belief changed over the course of visits to the restaurant and communication with the server. However, the evolution of belief across visits was not measured over time, as the reasons were provided. Therefore, Studies 2 will examine if the findings hold when individuals are asked to indicate belief for individual reasons given during each visit as the service failures are revealed.

Study 2: Deception Detection Within Subjects

Method. Eighty paid participants were recruited from Amazon Mechanical Turk. Two participants were removed from the analysis due to incomplete responses (final $N = 78$). Similar to Study 1, this study asked participants to imagine they had visited a local restaurant on three different occasions. Each visit was then described one at a time, each on a separate screen. Each visit description included an excessively long wait time and the server’s reason for that wait
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time. The participants rated their belief in the reason they were given for the long wait (on the same scale used in Study 1, before proceeding to read about the subsequent visit. An example of the first visit can be seen in Table 3.

*Table 3: Example Reason for Food Being Late*

<table>
<thead>
<tr>
<th>Date</th>
<th>Date</th>
<th>What your server said about the wait</th>
</tr>
</thead>
<tbody>
<tr>
<td>Visit A</td>
<td>09/01/2015</td>
<td>“The kitchen forgot about your order.”</td>
</tr>
</tbody>
</table>

Additionally, participants were asked to indicate if the delay was the server’s fault or the kitchen’s fault after reading about each visit. Furthermore, participants were asked in an open text item to indicate the reason they believed their food was delayed during that specific visit.

Participants were randomly assigned to one of two conditions. In the first condition a different reason was given for each delay in service (diffReas). In the other condition, participants were given the same reason for the delay in service across all visits (sameReas). For simplicity, I chose to include only one single-reason condition, which used “the kitchen forgot the order” reason for all service failures.

*Figure 4: Mean of Server Belief Across Conditions and Visits*
Results. Consistent with the results of Study 1, a mixed ANOVA revealed a significant interaction between the two-level, between-subjects “reasons” factor (one vs. different reasons) and the repeated within-subjects measure of belief ($F(2,156) = 4.40, p < .02$)—more specifically, participants who were shown the same reason for their food being delayed during each visit began to believe their server less in a more exaggerated manner than those in the different-reasons condition (Figure 4). Contrasting belief across conditions at each visit, we see that the difference in the participants’ belief in the server’s reason given during their second visit ($\Delta$Visit B) did not differ significantly across conditions ($M_{\text{sameReas}} = -1.80$, $M_{\text{diffReas}} = -0.58$, $F(1,78) 2.18 = p > .14$). However, the difference in the participants’ belief in the server’s reason given during their third visit ($\Delta$Visit C) differed significantly across conditions ($M_{\text{sameReas}} = -3.88$, $M_{\text{diffReas}} = -1.45$, $F(1,78) 6.95 = p < .02$). These findings replicate the pattern of belief results from Study 1.

*Figure 5: Mean of Server Fault Across Conditions and Visits*

Fault was analyzed in the same manner as was belief. As seen in Figure 5, the evolution of server’s fault follows a reverse pattern to that of belief—in that it escalates over time. Participants’ evolution of the fault they placed on the server for their delayed food, analyzed via
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a mixed ANOVA, differed significantly across conditions ($F(2,156) = 7.14, p < .002$). More specifically, participants who were shown the same reason for their food being delayed during each visit began to blame their server in a more exaggerated manner than those in the different reasons condition. The difference in the participants’ attribution of fault to server during their second visit ($\Delta$Visit B) varied significantly across conditions ($M_{\text{sameReas}} = 1.50, M_{\text{diffReas}} = -1.00$, $F(1,78) 12.01 = p < .001$). Similarly, difference in the participants’ attribution of fault to server during their third visit ($\Delta$Visit C) varied significantly across conditions ($M_{\text{sameReas}} = 2.58$, $M_{\text{diffReas}} = -0.45$, $F(1,78) 7.21 = p < .009$).

**Figure 6: Mean of Kitchen Fault Across Conditions and Visits**

Finally, as illustrated in Figure 6, the evolution of kitchen’s fault follows the reverse pattern to that of server fault. Participants’ evolution of the fault they placed on the kitchen for their delayed food, per a mixed ANOVA, differed significantly across conditions ($F(2,156) = 5.44, p < .006$). More specifically, participants who were shown the same reason for their food being delayed during each visit began to exonerate the kitchen of fault more than those in the different reasons condition. The difference in the participants’ attribution of fault to kitchen
during their second visit varied significantly across conditions ($M_{\text{sameReas}} = -1.65$, $M_{\text{diffReas}} = -0.05$, $F(1,78) = 4.05 = p < .05$). Similarly, difference in the participant’s attribution of fault to kitchen during their third visit varied significantly across conditions ($M_{\text{sameReas}} = -3.30$, $M_{\text{diffReas}} = -0.78$, $F(1,78) = 8.62 = p < .005$).

Discussion. The results of Study 2 support the theory that consumers are less likely to believe a single reason presented to them for multiple service failures. Further this study demonstrates that these findings hold when measuring belief as the events unfold while solidifying the more business oriented dependent variable of fault. Study 3 examines whether these findings generalize to contexts other than the restaurant context examined thus far.

Study 3: Deception Detection vs. Unbelievable Reason (Hotel Context)

Study 3 was designed to generalize the results of the preceding studies to a new context. Specifically, two aspects of the study were changed. First, I stepped away from the restaurant context into a context in which participants were asked to imagine that they are often required to travel to a foreign city for work using the same travel coordinator to book the same hotel. Thus, I changed the setting of the service failure. Second, since the travel coordinator and the hotel do not represent a single firm, I have also shifted from a single-firm context to a two-firm context. As in the previous studies, the participant still hypothetically interacted with a single POC (i.e., the travel coordinator). But, as opposed to the previous studies, the third party (which would ultimately be blamed for repeated service failures by the POC) was a hotel for which the travel coordinator was not employed. While this context does differ from the other studies, it retains the essential elements of those studies and, thus, I predicted that the results of the preceding studies would be replicated.
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Method. One hundred thirty-one paid participants were recruited from Amazon Mechanical Turk. Eight participants were removed from the analysis due to incomplete responses (final N = 123). In a deviation from the other studies conducted throughout this paper, this study asked participants to imagine that they had upcoming business trips to Madrid, Spain. Their company had instructed them to book their itinerary through the same travel coordinator each trip, whom then booked the participants at the same hotel. Upon arrival for each business trip, the participants were told they had to wait an excessively long time for their room to be available. During these waits the travel coordinator talked to the hotel staff over the phone. Participants were then told that the coordinator gave them the reason for their long wait for each stay.

Participants were randomly assigned to one of two conditions—the first condition gave different reasons for each delay in service (diffReas—“The hotel had lost your reservation,” “The hotel had booked your reservation under the wrong name,” “The hotel had booked your reservation for the wrong dates”). The other condition gave participants the same reason given for the delay in service across all visits (sameReas—“The hotel had lost your reservation”). After seeing a table of these reasons for each visit, the participants were asked to indicate to what extent they believed the reason the travel coordinator gave for the delay in service on a scale similar to that of Study 1 and 2.
**Results.** Consistent with the results of Study 1 and 2, a mixed-analysis ANOVA revealed a significant interaction between the between-subjects “reasons” factor and the repeated within-subjects measure of belief ($F(2,242) = 16.21, p < .0001$; Figure 7): participants who were shown the same reason for their room being delayed during each visit began to believe their travel agent less in a more exaggerated manner than those in the different reasons condition. The difference in the participant’s belief ($\Delta$) in the travel coordinator’s reason given related to their second visit varied significantly across conditions ($M_{\text{sameReas}} = -2.42, M_{\text{diffReas}} = -0.85, F(1,121) 14.40 = p < .0002$). Consistently, the difference in the participant’s belief in the travel coordinator’s reason given related their third visit varied significantly across conditions ($M_{\text{sameReas}} = -4.17, M_{\text{diffReas}} = -1.34, F(1,121) 18.51 = p < .0001$). During both the second and third visits, the travel coordinator’s reason for the delay in their room was less believable if participants were given the same reason as the previous visit.

**Discussion.** The results of Study 3 support the theory that consumers are less likely to believe a single reason presented to them for multiple service failures. Further this study
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demonstrates that these findings hold within a different context and for scenarios where the POC and source of the ostensible cause of the service failure are not within the same firm.

**Study 4: Deception Detection Changing the Messenger**

The previous studies have demonstrated that ostensible coincidence is a cue people use to detect deception. I have even been able to moderate this effect through varying the degree of coincidence by having the same server give different (vs. the same) reasons for three service failures. Study 4 accomplishes a similar moderation through the variation of another component of the coincidence: the server. If the server is different across the three visits, the level of perceived coincidence diminishes and so should the perception of deception.

**Method.** One hundred seventy paid participants were recruited from Amazon Mechanical Turk. Five participants were removed from the analysis due to incomplete responses leaving a total of one hundred sixty-five participants included in the analysis. Similar to the first study, this study asked participants to imagine they had visited a local restaurant on three different occasions and had to wait an excessively long time for their orders. Participants were randomly assigned to conditions in a 2 (server: same vs. different) vs. 2 (reasons: same vs. different). Thus, participants ended up in four possible scenarios: either they had (i) a different server each visit that gave them a different reason for each service failure, (ii) the same server each visit that gave them the same reason for each service failure, (iii) the same server each visit that gave them a different reason for each service failure, or (iv) a different server each visit that gave them the same reason for each service failure. An example of what participants saw can be seen in Table 4.
Server names were selected on the basis that they could be either male or female names as to not bias the results. In the same server conditions, Charlie was the server as this is generally considered the most androgynous name.

Similar to study 1, participants rated the extent to which they believed the reasons for all service failures on one screen, then indicated how much they believed the overall reasons the server(s) gave for their food being late. Further, participants were asked to indicate to what extent they believed the food being delayed was the server’s fault or the kitchen’s fault during each individual visit. Participants were also asked to respond to an open ended question and indicate why they believed their food was delayed.

Results. In a divergence from the formatting of other studies within this paper, I look at overall belief across visit first in this Study 4. I do this because most readers will find all the information they need to inform them of the outcome of this study from this simple measure; however, for readers looking to fully understand the change in belief across visit, that information follows.
The number of servers (same vs. different) and number of reasons (same vs. different) did not significantly interact to influence participants’ overall beliefs in the reasons they were given ($F(1,160) = .06, p > .8$). However, consistent with the predictions and the visit-specific belief results, participants’ overall belief was significantly lower when they received the same (vs. different) reasons ($M = -1.98$ vs. $.24; F(1,160) = 23.12, p < .0001) and when those reasons were supplied by a single server (vs. different servers; $M = -2.08$ vs. $.30; F(1,160) = 27.12, p < .0001).

These results are particularly interesting because they demonstrate that changing the server while keeping the reason constant or changing the reason while keeping the server constant across visits systematically affects participant’s belief in the reason given for the service failure in a similar way. The full pattern of these results is presented in Figure 8. Thus, giving the theory that the level of coincidence is responsible for detecting deception in these scenarios.
### Table 5: Mean of Change in Belief and Fault Across Conditions and Visits

<table>
<thead>
<tr>
<th>Server Reason</th>
<th>Different</th>
<th>Same</th>
<th>Different</th>
<th>Same</th>
</tr>
</thead>
<tbody>
<tr>
<td>Believe</td>
<td>ΔVisit B</td>
<td>-0.44</td>
<td>-1.73</td>
<td>-1.68</td>
</tr>
<tr>
<td>Kitchen Fault</td>
<td>ΔVisit B</td>
<td>0.80</td>
<td>-0.78</td>
<td>-0.03</td>
</tr>
<tr>
<td>Server Fault</td>
<td>ΔVisit B</td>
<td>-2.10</td>
<td>0.48</td>
<td>-0.71</td>
</tr>
<tr>
<td></td>
<td>ΔVisit C</td>
<td>-1.12</td>
<td>0.65</td>
<td>-0.18</td>
</tr>
</tbody>
</table>

A mixed ANOVA of the between-subjects factors server (same vs. different) and reason (same vs. different), and the repeated within-subjects measure of belief revealed a significant interaction between server and belief ($F(2,320) = 10.67, p < .0004$) and between reason and belief ($F(2,320) = 21.32, p < .0001$). However, the three-way interaction between server, reason, and belief was not significant ($F(2,320) = .69, p > .45$). In combination, these results reveal that the two potential sources of coincidence in this scenario (same server and same reasons) additively influence participants’ belief in the reasons they are given. Varying either the number of sources (servers in this study) or number of reasons independently influences consumers’ beliefs.

When looking at how participant’s beliefs evolved across visits, we see the expected pattern of results. ΔVisit B belief (the change in belief from Visit A to Visit B) was significantly more negative when the reasons were given by the same server (vs. different servers: $M = -2.25$ vs. -1.07; $F(1, 163) = 5.70, p < .02$). Likewise, ΔVisit B belief was significantly more negative when the reasons given were the same (vs. different) across visits ($M = -2.26$ vs. -1.04; $F(1, 163) = 6.12, p < .02$), replicating the previous results. This pattern persisted and strengthened for ΔVisit C belief (the change in belief from Visit A to Visit C). ΔVisit C belief was significantly more negative when the reasons were given by the same server (vs. different servers: $M = -3.86$ vs. -2.40; $F(1, 163) = 10.82, p < .002$).
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vs. -1.41; \(F(1, 163) = 13.34, p < .0004\). Likewise, \(\Delta\text{Visit C} \) belief was significantly more negative when the reasons given were the same (vs. different) across visits (\(M = -4.24 \text{ vs. } -0.94; \)

\(F(1, 163) = 26.05, p < .0001\) , replicating the previous results. Number of servers and number of reasons did not significantly interact for either \(\Delta\text{Visit B} \) (\(F(1, 163) = .06, p > .80\) or \(\Delta\text{Visit C} \)

\(F(1, 163) = .48, p > .49\).

A mixed ANOVA of the between-subjects factors server (same vs. different) and reason (same vs. different), and the repeated within-subjects measure of server-fault revealed a significant interaction between server and sever-fault (\(F(2,320) = 13.58, p < .0001\) and between reason and server-fault (\(F(2,320) = 28.52, p < .0001\)). The three-way interaction between server, reason, and server-fault was significant as well (\(F(2,320) = 5.21, p < .02\)). In combination, these results reveal that (i) the two potential sources of coincidence in this scenario (same server and same reasons) additively influence participants’ attribution of fault to the server and (i) varying the coincidental factors of server and reason have a combined influence on how participants attribute fault to the server.

\(\Delta\text{Visit B} \) server-fault (the change in faulting the server from Visit A to Visit B) was significantly more positive when the reasons were given by the same server (vs. different servers: \(M = .98 \text{ vs. } -.83; \)

\(F(1, 163) = 13.70, p < .0004\) ). Likewise, \(\Delta\text{Visit B} \) server-fault was significantly more positive when the reasons given were the same (vs. different) across visits (\(M = 1.49 \text{ vs. } -1.43; \)

\(F(1, 163) = 40.34, p < .0001\) ). This pattern persisted and strengthen for \(\Delta\text{Visit C} \) server-fault (the change in faulting the server from Visit A to Visit C). \(\Delta\text{Visit C} \) server-fault was significantly more positive when the reasons were given by the same server (vs. different servers: \(M = 2.30 \text{ vs. } -.25; \)

\(F(1, 163) = 17.50, p < .0001\) ). Likewise, \(\Delta\text{Visit C} \) server-fault was significantly more positive when the reasons given were the same (vs. different) across visits (\(M = \)
Number of servers and number of reasons did not significantly interacted for \( \Delta \text{Visit B} \) \( F(1, 163) = 2.96, p > .54 \) but did significantly interact for \( \Delta \text{Visit C} \) \( F(1, 163) = 80.76, p < .02 \).

A mixed ANOVA of the between-subjects factors server (same vs. different) and reason (same vs. different), and the repeated within-subjects measure of kitchen-fault revealed a significant interaction between server and kitchen-fault \( F(2,320) = 15.74, p < .0001 \) and between reason and kitchen-fault \( F(2,320) = 25.32, p < .0001 \). The three-way interaction between server, reason, and kitchen-fault was significant as well \( F(2,320) = 5.82, p < .009 \). In combination, these results reveal that (i) the two potential sources of coincidence in this scenario (same server and same reasons) additively influence participants’ attribution of fault to the kitchen and (i) varying the coincidental factors of server and reason have a combined influence on how participants attribute fault to the kitchen.

\( \Delta \text{Visit B} \) kitchen-fault (the change in faulting the server from Visit A to Visit B) was significantly more negative when the reasons were given by the same server \( \text{vs. different servers: } M = -1.46 \text{ vs. } 0.2; F(1, 163) = 11.23, p < .001 \). Likewise, \( \Delta \text{Visit B} \) kitchen-fault was significantly more negative when the reasons given were the same \( \text{vs. different} \) across visits \( M = -1.78 \text{ vs. } 0.41; F(1, 163) = 26.98, p < .001 \). This pattern persisted and strengthen for \( \Delta \text{Visit C} \) kitchen-fault (the change in faulting the server from Visit A to Visit C). \( \Delta \text{Visit C} \) kitchen-fault was significantly more negative when the reasons were given by the same server \( \text{vs. different servers: } M = -2.92 \text{ vs. } -0.30; F(1, 163) = 19.52, p < .0001 \). Likewise, \( \Delta \text{Visit C} \) kitchen-fault was significantly more negative when the reasons given were the same \( \text{vs. different} \) across visits \( M = -3.13 \text{ vs. } 0; F(1, 163) = 29.88, p < .001 \). Number of servers and number of reasons did not
significantly interacted for ΔVisit B ($F(1, 163) = 11.48, p > .19$) but did significantly interact for ΔVisit C ($F(1, 163) = 86.90, p < .009$).

**Figure 9: Mean of Server/Kitchen Fault Across Visits in the Same-Server-Same-Reason Condition**

Although previously shown in Study 2, since I measured the server’s fault for each individual visit within the current study we can see that as the server is blamed more in the same-server-same-reason condition, the kitchen is exonerated of blame as depicted in Figure 9.

**Discussion.** The results of Study 4 support the claim that consumers are less likely to believe a single reason presented to them for multiple service failures. Further this study demonstrates that these findings are a product of the level of coincidence. By changing the server and the reasons across visits, there are less coincidental factors and participants detected less deception. Study 4 further demonstrates that the server will be blamed more for the service failure as coincidental elements manifest. Further, Study 4 corroborates the findings of Study 3 by showing that as participants place blame on the messenger they also alleviate blame from the ostensible cause.
Study 5: Prediction vs. Belief

Study 5 contrasts participant’s predictions about the reason for a service failure with their belief of that reason, or lack-there-of. It is quite reasonable that a consumer experience repeated service failures for a systemic reason. However, when such a seemingly systemic reason is given to them by an individual (the POC) plausibly motivated to avoid blame, they tend to discount its likelihood and, thus, not believe the POC or the reasons given. If true, then we should see that consumers expect a single reason explains repeated (and similar) service failures, yet still be surprised and express disbelief if they are told that this is the case.

Method. As part of a series of unrelated experiments, I recruited two hundred fourteen paid participants from Amazon Mechanical Turk. Twenty-one participants were removed from the analysis due to incomplete responses leaving a total of one hundred ninety-three participants. Similar to the first study, this study asked participants to imagine they had visited a local restaurant on three different occasions and had to wait an excessively long time for their orders. Half of the participants were randomly assigned to a condition in which they were asked to imagine a scenario similar to that in the single-reason conditions of previous studies. An example of this belief condition can be seen in Table 6.

Table 6: Example Reason for Food Being Late

<table>
<thead>
<tr>
<th>Visit</th>
<th>Date</th>
<th>Server</th>
<th>What your server said about the wait</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>two weeks ago</td>
<td>Charlie</td>
<td>“The kitchen forgot about your order.”</td>
</tr>
<tr>
<td>B</td>
<td>last week</td>
<td>Charlie</td>
<td>“The kitchen forgot about your order.”</td>
</tr>
<tr>
<td>C</td>
<td>today</td>
<td>Charlie</td>
<td>“The kitchen forgot about your order.”</td>
</tr>
</tbody>
</table>
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The other half of the participants were randomly assigned to a condition in which they were asked to imagine a scenario in which the server gave them the same reason for their first two visits to the restaurant and, although their food was delayed on the third visit, the server had yet to give them a reason. An example of this prediction condition can be seen in Table 7.

**Table 7: Example Reason for Food Being Late**

<table>
<thead>
<tr>
<th>Visit</th>
<th>Date</th>
<th>Server</th>
<th>What your server said about the wait</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>two weeks ago</td>
<td>Charlie</td>
<td>“The kitchen forgot about your order.”</td>
</tr>
<tr>
<td>B</td>
<td>last week</td>
<td>Charlie</td>
<td>“The kitchen forgot about your order.”</td>
</tr>
<tr>
<td>C</td>
<td>today</td>
<td>Charlie</td>
<td>--</td>
</tr>
</tbody>
</table>

Participants in both conditions were then asked, “What do you believe caused your food to be late today?” and were further randomly assigned to two other conditions. The first condition gave participants four options to choose from when answering the previous question. These consisted of the following: “The kitchen forgot about my order;” “A cook dropped my food in the kitchen;” “The kitchen made a mistake and had to remake my food;” and “The server (Charlie) was in some way responsible.” This condition was the closed-end condition. In the open-end condition, participants were given the same first three options to respond to what caused their food to be late; however, the final option was changed to an open-end response field that allowed participants to specify any cause. I theorized that if the participants are to predict the cause of their food being late on their third visit to the restaurant, they will predict the most likely cause to be the coincidental cause for a service delay. However, we theorized that participants in an identical parallel scenario that were given reasons by a server would not believe the coincidental cause is the actual cause of their service delay.
Results. The overwhelming majority of people in the prediction condition indicated that the cause of their food being delayed is that the kitchen forgot to make their order (closed-end: 62% and open-end: 98%). In contrast, significantly fewer participants in the belief condition actually believed that the kitchen forgot to make their order when that was the reason the server presented them (closed-end: 23% and open-end: 58%). This demonstrates that although a systemic cause makes the most probable sense for a repeat service failure, when this cause is presented by a motivated source (the POC), it constitutes an ostensible coincidence that consumers tend to not believe. Further, participants predicted the server was the cause for their food being late a fraction of the time (closed-end: 38% and open-end: 2%). However, when the server gave them a reason for their food being late that aligned with this prediction, they tended to say the server was the cause for their delayed food (closed-end: 67% and open-end: 25%). This demonstrates that not only do consumers not believe a coincidental cause for a service failure but they also tend to attribute blame the POC.

Discussion. The results of Study 5 support the assertion that consumers are less likely to believe a single reason presented to them for multiple service failures. Study 5 also supports the proposal that it is more probable that a systemic issue is causing the repeat service failures by demonstrating that consumers will predict that the single reason is the most likely cause for the service failure. However, these participants do not believe it when the single reason is presented to them as the reason for the service failure.

GENERAL DISCUSSION

In this research, I examined the extent to which consumers’ beliefs of the claims made by a point of contact (POC) evolved over repeated service failures. I drew on literature from deception detection (Ekman and Friesen 1969; Ekman 2009) and coincidence (Dessalles 2006;
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Dessalles 2008; Dimulescu and Dessalles 2009) to propose that consumers are more likely to believe a POC when the POC gives different (vs. the same) reasons for each of the service failures. Specifically, I established that ostensible coincidences (the same reason given repeatedly for service failures) provided by the POC lead consumers to discount these explanations for the service failures. Consumers find ostensible coincidences less believable although these coincidences suggest a systemic cause for the service failure that is both more likely and more expected by the consumer a priori (study 5). Further, when presented with an ostensible coincidence, consumers are more likely to blame the POC for the service failure.

Theoretical Implications. Paramount to the fundamental goal of consumer behavior literature, I utilized the phenomenon of ostensible coincidences to further understand the nature and behaviors of consumers. This research introduces the concept of coincidence to the academic marketing literature. Moreover, this research contributes to the literature on deception detection, by showing how the previously unexamined content cue of ostensible coincidence influences deception detection, and the literature on coincidence, by developing a link between coincidence and the social construct of disbelief through ostensible coincidences. Lastly, attribution literature and service failure literature benefit from the understanding this research provides around attributions of blame after the portrayal of coincidence via a POC.

Managerial Implications. Firms could benefit directly from this research. As mentioned in the introduction, many firms appoint a representative to handle all of a particular consumer’s needs. Although this undoubtedly has benefits explored in other research, this could have potentially negative consequences when considering the findings presented here. This phenomenon combined with the transparent business culture that is often promoted as a best
practice today, could lead to ostensible coincidences for repeat service failures. Thus, leading to a greater likelihood that consumers begin to distrust their service representative.

*Review of Key Findings.* Across differing contexts, I reveal that consumers are less likely to believe a POC that presents the same (vs. different) reason for repeat service failures and more likely to blame the POC presenting the same reason versus different reasons while exonerating the ostensible cause of responsibility. I show that consumers predict a systemic cause as the reason for multiple service failures; however, if the systemic reason is presented repeatedly by the same POC for multiple service failures, the consumer does not believe it.

*Alternative Explanations.* The studies presented within this paper support the conceptual framework and predictions. However, past research related to deception detection could have potentially predicted similar outcomes without the use of coincidence. Truthful people tend to take their perceived integrity for granted while liars commonly attempt to appear trustworthy (DePaulo, LeMay, and Epstein 1991). Thus, participants could have seen blaming the kitchen as an attempt to appear credible and concluded that the server is trying to deceive them. Although, one would imagine that the condition in which different reasons were given would appear more deceitful in the study if this were the only mechanism acting on belief and deception detection—since presenting different reasons could appear as an attempt to appear credible. Furthermore, liars tend to display an augmented other-focus (Ickes, Reidhead, and Patterson 1986) while using first-person singular pronouns less regularly (Newman et al. 2003) in an attempt to detach themselves from their lie. Therefore, placing blame on the kitchen could appear to be a telltale sign of deceit. However, if this mechanism was at work, one would imagine it would affect believability in all conditions of the study equally given that the POC always blames the kitchen.
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Limitation and Future Direction. As we navigate our everyday lives, unexpected events occur often. Within the experimental design employed in this paper, the comparison highlighting the coincidence leading to unexpectedness is unambiguous—it is easy to compare the reasons given across multiple services failure when they are presented without any noise. In a “real world” setting, this comparison might be less apparent—in addition, many other factors may pollute such observations. Therefore, one might argue that these findings might not hold in the dramatically more intricate setting of everyday life. I contend that the results would hold to the extent to which a consumer perceives an ostensible coincidence, and that may be influenced by many factors such as the consumers’ memory of past events and cues in the environment that might facilitate the recall of such an event. Broadly speaking, I argue that the phenomena detailed within this research can be manipulated to be more or less salient—in fact, this salience is a driving force in determining if a coincidence is unexpected or not. After all, a coincidence would not be influential in determining belief or perceptions of deceit if the coincidence is not recognized. Thus, examining how these contextual factors might be accentuated and unexpectedness achieved or avoided is an important direction for future research.

One might also take exception with the hypothetical nature of the experiments. Indeed, consumers might respond differently in incentive-compatible conditions. The magnitude of the effects would likely be larger in real versus hypothetical conditions for the phenomena being examined here, as has been the case in other domains (Kivetz and Simonson 2002). Hence, the results here may very well be muted relative to what would be expected if a consumer personal welfare was actually at stake. That said, future researchers may very well wish to determine the robustness of the current results in contexts in which the consumer may actually gain or lose via repeated interactions with the firm.
Admittedly, this research could benefit from a more managerial-oriented dependent variable—firm trust, customer loyalty, customer satisfaction, brand switching, and willingness to return are all dependent variables for future research that ostensible coincidence could potentially impact. This is apparent when we consider the practices of business transparency and assigning a single service representative to a customer. Although there is a plethora of research that already demonstrates that the relationship a consumer has with a firm’s representative will affect their relationship with the firm (i.e., (Hallowell 1996; Winer 2001)), establishing this link directly with instances of ostensible coincidence would be ideal.
CHAPTER 3:

Who Do I Believe Now? The Influence of Scarcity Presentation on Credibility and Purchase Behavior

Firms communicate with consumers through a variety of actions ranging from multifaceted mass-media advertising campaigns to simple shelf layout decisions. Media literate consumers are skeptical of most communication presented by a firm—consumers are becoming more keenly aware that all communication, no matter how seemingly trivial, is thoughtfully designed to coerce them into thinking, feeling, or doing something in relation to the firm that is the source of the message. As e-commerce takes a larger share of consumers’ wallets, traditional interactions (e.g. salespersons calling on consumers) between the firm and the consumer are replaced by websites and applications. As digital means of acquiring goods and services become more commonplace than direct interaction with a salesperson in a brick-and-mortar setting, consumers may assume there is less chance for the firm to use persuasive tactics of them. In reality, consumers might more susceptible to firms’ persuasive strategies in this scenario.

One such example of coercive communication prevalent in websites is scarcity signals. In marketing, scarcity is a well-researched topic—we know that the scarcity of goods and services affect consumer behavior. Marketing scholars have identified that scarcity increases purchase behavior because consumers infer that scarcer products are of higher popularity or quality (Van Herpen, Pieters, and Zeelenberg 2009), consumers typically choose popular products since they think other consumers are more knowledgeable (Cialdini 1993), scarcity enhances the perceived value of products and opportunities (Cialdini and Garde 1987), and scarcity induces arousal and
this heightened arousal polarizes the evaluations of individual items contained in the choice set (Zhu and Ratner 2015). Prior research demonstrates that scarcity signals relative to a firm’s product tend to have an impact on product choice in favor of the firm.

Firms are well aware of the effects of scarcity and utilize them as sales tactics to persuade consumers, even going as far as communicating artificial scarcity of products and services (Kain 2017). Companies communicate scarcity signals for products to create a sense of urgency, in hopes of impulse purchases, where such scarcity might not exist in reality. There are numerous web posts that question the authenticity of the message, “Only 1 Left in Stock—Order Soon” (“Only One Left!”—the Latest Online Marketing Ploy” 2014). Conversely, in some cases, consumers assume scarcity signals communicated by the firm are objective or inherently valid. This research focuses on understanding one driving factor in whether increasingly skeptical and media-literate consumers are susceptible to being coerced by scarcity signals controlled directly by the firm.

Consider the following simple yet familiar scenario. A consumer visits an airline’s website to purchase a ticket for upcoming travel. The consumer finds a direct flight with an acceptable departure time and is presented with a message that indicates there is a limited number of seats remaining on the flight. The consumer could reasonably believe this message and treat the scarcity signal as credible, thus, placing an additional impetus to act fast and reserve a seat before it is no longer an option. Contrariwise, the consumer could view this message as a ploy by the airline to get consumers to book the ticket immediately rather than wait to see if the price drops or price shop other competing airlines. With this context in mind, imagine that instead of seeing a message about a limited number of seats remaining the consumer saw a graphic representing the seat availability in the cabin of the aircraft and only a limited number of
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seats were highlighted as available. In both scenarios, the scarcity signal originated from and is controlled by the same source—the airline. However, is one message perceived as more believable by consumers? If so, what makes one scarcity signal more believable than another? Finally, does this belief translate into different purchasing patterns?

Across multiple studies, I focus on firms’ different approaches to communicating scarcity signals and how these signals are perceived by consumers. Objectively speaking, scarcity signals originate and are controlled by the same source—the firm. Therefore, each signal should be met with equal consumer skepticism. Yet, I predict that consumers are more likely to perceive a scarcity signal as believable under certain conditions. I argue that if the consumer is an active agent in uncovering or interpreting the scarcity signal, they will perceive the signal as more authentic. Furthermore, in accordance with scarcity literature, the believable scarcity signal will favorably affect purchase intentions and behaviors for the firm.

THEORY

Extant research has examined persuasion in a plethora of contexts. It is unsurprising then that belief is a headlining construct of interest in much research on persuasion (Friestad and Wright 1995) and advertising (Mitchel and Olson 2000). Conversely, belief is often either ignored or taken for granted in research on other, generally considered, non-persuasive firm-consumer communication domains. Yet, it would be erroneous to assume that consumers inherently believe claims generated by the firm in non-persuasion contexts. In fact, any context in which a firm might benefit from dishonesty is one in which consumer disbelief may arise (Fein and Hilton 1994; Fein 1996; Fein, Hilton, and Miller 1990; Main, Dahl, and Darke 2007). Furthermore, research does show that a brand’s credibility affects the formation of consideration sets (Erdem and Swait 2004). Therefore, consumers are likely to recognize that what they are
told and what actually is might differ. Hence, consumers may have grounds to disbelieve firm generated messages.

**Persuasion.** The Persuasion Knowledge Model (PKM) investigates consumers’ awareness about the strategies and tactics that firms employ to convince them to buy a product and the motives behind such strategies (Friestad and Wright 1994). This model begins to address consumers’ personal knowledge related to firm’s goals and tactics. Consumers refine their knowledge on persuasive tactics and manage persuasive messages while dynamically adjusting their attitudes toward the product and the firm. If we apply PKM to both conditions in the previous airline example, we begin to have an understanding of the active variables at work in either coercing consumers to act on or ignore scarcity signals. Perhaps consumers already have knowledge associated with explicit scarcity signals (i.e. “Only one remaining, act now!”). This cue could be seen as intended by the firm to mean the product is scarce or could be seen as a cue that the firm is simply trying to get you to purchase the firm’s goods or services quickly. While the more implicit method of allowing the consumer to infer scarcity does not conjure this same knowledge because it is not registered as a persuasive attempt. But why would this be the case if the source of information is the same? Perhaps, as far as the consumer is concerned, the source of the information is not the same.

**Activation Template.** The consumer, being actively involved in discovering a scarcity signal, will no longer perceive that the signal came from the firm but that they are responsible for uncovering it. The effort of interpretation shifts the perceived message source from external to internal. The activation template, or the activation version of the interactive experiment template, comes from Goldenberg et al.’s work to understand creativity (1999a, 1999b). The authors argue that requiring a consumer to engage in an interactive experience with a persuasive message will
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be thought of as more creative and encourages comprehension of the message (Goldenberg, Mazursky, and Solomon 1999a). Although there is empirical evidence that messages or products crafted through this template are thought of as more creative (Goldenberg, Mazursky, and Solomon 1999b), there is little empirical evidence that messages or products crafted with this template are more persuasive or, critically, more believable. However, this research aims to rectify this—at least in relation to scarcity signals.

I attempt to show that when one applies the activation template to scarcity signals, the consumer will distribute cognitive resources to understanding the signal and not to understanding the underlying coercive nature of the signal. Thus, the consumer will systematically reach an understanding that the product is scarce but will not treat this scarcity signal in accordance with a persuasive message in PKM. Therefore, consumers in the example that are presented with a graphic representing the limited seat availability in the cabin of the aircraft will uncover the scarcity of the available seats for themselves. Thus, in this condition, the consumer will assume they must make a purchase decision quickly. Conversely, when a consumer is presented with an explicit scarcity signal (ie. “Only 3 Seats Left!”), they will question the credibility of the source and believability of the message in accordance with PKM. This will ultimately be incorporated in their decision process.

Explicit Scarcity Signal and Believability. Consumers are skeptical towards a firm generated message, and skepticism negatively affects believability. Consumers are plausibly aware that explicit scarcity messages are a sales tactic, which decreases believability of this tactic in the future according to PKM. Content and valence of these positive or negative thoughts depends on the information that is available to the consumer at the time of evaluation (Feldman and Lynch 1988). Importantly, when consumers become aware of persuasion attempts directed at
them, they often use their persuasion knowledge to resist these attempts, hence consumers would be relatively less likely to choose scarcer alternatives (Friestad and Wright 1994). I argue that explicit scarcity signals trigger this coping behavior in consumers.

**H1: Communicating with consumers through explicit scarcity signals has a negative effect on believability of the message, which decreases purchase likelihood, because it is perceived as a persuasion attempt on behalf of the firm.**

*Implicit Scarcity Signal and Believability.* Friestad and Wright suggest that as a consumer’s familiarity with a persuasion coping task increases, the cognitive effort they expend to do those coping tasks decreases and aspects of their coping behavior become automatic (1994). Additionally, scarcity tends to impact choices among consumer goods only when consumers believe that market forces have caused that scarcity (Verhallen and Robben 1994; Worchel, Lee, and Adewole 1975). Since explicit scarcity signals will be interpreted as a persuasion attempt, the believability will decrease, and therefore scarcity will be perceived as an artificial phenomenon instead of a natural one (i.e. market forces causing scarcity). The opposite is expected to hold for implicit scarcity signals—since it is the consumer who inferred the scarcity through cognitive elaboration, they perceive the information is not coming from the same source. Presence of an implicit scarcity signal will involve consumers in discovering the scarcity signal through increased cognitive effort. This involvement will allow the consumer to treat the scarcity signal as a non-persuasive message. Thus, the consumer will not register a persuasive attempt and draw on their persuasive knowledge but instead will apply credibility to the scarcity signal. Ultimately, the purchase likelihood of the consumer will be greater with an implicit scarcity signal.
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**H2:** Communicating with consumer through implicit scarcity signals has a positive effect on believability of the message, which increases purchase likelihood, because it is not perceived as a persuasion attempt by the firm and the scarcity signal is perceived as discovered by the consumer.

**STUDIES**

**Figure 10: Graphic showing Available Seats in an Aircraft**

**Study 1: Indirect vs. Direct Scarcity Signals – Airline Scenario**

In order to investigate this hypotheses, the experiment placed participants in a hypothetical scenario similar to the airline example in the introduction. The goal of this study is demonstrate that implicit and explicit scarcity signals differ in consumers’ perception. Specifically, consumers are less likely to believe explicit (vs. implicit) scarcity signals. Furthermore, consumers are less likely to purchase when exposed to an explicit (vs. implicit) scarcity signal.
Method. One hundred and thirty-six paid participants were recruited from Amazon Mechanical Turk. Two participants were removed from the analysis for failing to respond to all questions, leaving one hundred and thirty-four participants. Participants were asked to imagine they had an upcoming optional business trip and had been instructed to book your domestic flight using your company credit card; therefore, price is no concern. Upon visiting the company designated airlines website to check for available tickets, the consumer found an available non-stop flight in the main cabin.

Participants were then randomly assigned conditions in a 2 (plane schematic: yes vs. no) v. 2 (explicit scarcity: yes vs. no). Thus, participants ended up in four possible scenarios: either they were presented with a graphic showing the available seats in an aircraft, Figure 10 (implicitPicture), presented with a graphic showing the same available seats in a text based format, Figure 11 (implicitText), showed the same graphic as the first condition except it featured red text that stated, “Hurry up! Only 3 seats are left” (explicitPicture), or showed the same graphic as the second condition except it featured red text that stated, “Hurry up! Only 3 seats are left” (explicitText).

Participants then indicated how likely they were to purchase a ticket on a seven-point scale ranging from “Extremely Unlikely 1” to “Extremely Likely 7”. On another page, participants were asked if they believed the messaging regarding the seats offered by the airline (“Completely DO NOT Believe 1” – “Completely Believe 7”). Furthermore, on the same page,
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participants were asked to what extent their purchase decision was influenced by the seat locations, seat availability, and number of seats in three separate measures (“Not At All Influenced 1” – “Entirely Influenced 7”). Lastly, participants were asked to what extent they felt pressured to make the purchase by the airline (“Not At All Pressured 1” – “Entirely Pressured 7”). On a separate screen, participants were asked their gender and age prior to thanking them for their participation.

Figure 12: Mean of Purchase Across Conditions

Results. A mixed ANOVA of the between-subjects factors plane schematic (yes vs. no) and the between-subjects measure of purchase likelihood revealed a significant difference ($F(1,130) = 4.14, p < .05$). Analysis of the between-subjects factors explicit scarcity statement (yes vs. no) and the between-subjects measure of purchase likelihood revealed a significant difference ($F(1,130) = 9.71, p < .003$). Finally the analysis of the between-subjects factors plane schematic (yes vs. no) and explicit scarcity statement (yes vs. no), and the between-subjects measure of purchase likelihood revealed a significant interaction ($F(1,130) = 4.33, p < .04$). In combination, these results reveal that (i) the two potential sources scarcity signals in this scenario
additively influence participants’ likelihood to purchase and (ii) varying the scarcity factors have a combined influence on participants’ likelihood to purchase.

A contrast between the implicit and explicit conditions showing the schematic of the plane demonstrates a significant difference in likelihood of purchase ($F(1, 130) = 13.51, p < .0004$). Thus, participants are significantly more likely to purchase a seat when presented with an implicit than an explicit scarcity signal in relation to the schematic of the plane. A similar contrast between the implicit and explicit text conditions shows no significant difference ($F(1, 130) = .54, p > .45$). Thus, there is no evidence that participants are more likely to purchase a seat on the plane when shown an implicit than an explicit scarcity signal in relation to the text. Interestingly, a contrast between the explicit schematic of the plan and the explicit text shows a significant difference ($F(1, 130) = 8.67, p < .004$). These results demonstrate that participants are more likely to purchase a seat on the plane when showed the explicit scarcity signal paired with text than paired with a schematic of the plane.

**Figure 13: Mean of Message Belief Across Conditions**

![Message Belief Chart](chart.png)
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As shown in Figure 13, the analysis on the manipulated between-subjects factors plane schematic (yes vs. no) and the between-subjects measure of message belief revealed no significant difference \( (F(1,130) = 1.28, p > .26) \). Analysis of the between-subjects factors explicit scarcity statement (yes vs. no) and the between-subjects measure of message belief a significant difference \( (F(1,130) = 10.82, p < .002) \). Finally the analysis of the between-subjects factors plane schematic (yes vs. no) and explicit scarcity statement (yes vs. no), and the between-subjects measure of message belief revealed no significant interaction \( (F(1,130) = .93, p > .33) \). In combination, these results reveal that (i) showing participants explicit scarcity signals additively influence participants’ belief in the scarcity signal and (ii) varying the scarcity factors have no combined influence on participants’ belief in the scarcity signal.

A contrast between the implicit and explicit conditions showing the schematic of the plane demonstrates a significant difference in belief in the messaging \( (F(1, 130) = 9.05, p < .004) \). In other words, participants believe the message more when shown implicit than an explicit scarcity signal accompanying a schematic of the plane. A similar contrast between the implicit and explicit text conditions shows no significant difference \( (F(1, 130) = 2.70, p > .1) \). Thus, there is no evidence participants believe the message more when shown implicit than an explicit scarcity signal accompanying text of the seat locations. A contrast between the explicit picture of the plane and the explicit text shows no significant difference \( (F(1, 130) = 2.30, p > .1) \). Similarly, a contrast between the implicit schematic of the plane and the implicit text shows no significant difference \( (F(1, 130) = .02, p > .8) \). Thus, there is no evidence suggesting a difference between similar conditions of the text vs similar conditions of the schematic across scarcity signals.
As shown in Figure 14, the analysis on the manipulated between-subjects factors plane schematic (yes vs. no) and the between-subjects measure of pressure to purchase revealed no significant difference ($F(1,130) = 1.02, p > .31$). Analysis of the between-subjects factors explicit scarcity statement (yes vs. no) and the between-subjects measure of pressure to purchase revealed a significant difference ($F(1,130) = 19.33, p < .0001$). Finally the analysis of the between-subjects factors plane schematic (yes vs. no) and explicit scarcity statement (yes vs. no), and the between-subjects measure of pressure to purchase revealed no significant interaction ($F(1,130) = 5.46, p > .03$). In combination, these results reveal that (i) showing participants explicit scarcity signals additively influence participants’ pressure to purchase and (ii) varying the scarcity factors have no combined influence on participants’ pressure to purchase.

A contrast between the implicit and explicit conditions showing the schematic of the plane demonstrates no significant difference in pressure to make the purchase by the airline ($F(1,130) = 2.12, p > .1$). A similar contrast between the implicit and explicit text conditions shows a significant difference ($F(1,130) = 22.67, p < .0001$). Therefore, even though there is no evidence
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to suggest people are affected by the differing scarcity signals in non-congruent ways when the schematic is present, there is evidence to suggest they are when only shown the text. A contrast between the explicit schematic of the plane and the explicit text shows a significant difference ($F(1, 130) = 5.38, p < .03$). Conversely, a contrast between the implicit schematic of the plane and the implicit text shows no significant difference ($F(1, 130) = .97, p > .3$).

The analysis of the between-subjects factors plane schematic (yes vs. no) and explicit scarcity statement (yes vs. no), and the between-subjects measure of seat availability revealed no significant omnibus ($F(3,130) = 1.41, p > .24$). Similar analysis of the between-subjects factors plane schematic (yes vs. no) and explicit scarcity statement (yes vs. no), and the between-subjects measure of seat location revealed no significant omnibus ($F(3,130) = 1.42, p > .24$). Lastly, analysis of the between-subjects factors plane schematic (yes vs. no) and explicit scarcity statement (yes vs. no), and the between-subjects measure of number of seats revealed a marginally significant omnibus ($F(3,130) = 2.52, p < .07$). Therefore, no evidence is present to suggest that participants were influenced to purchase or not by the seat availability and location; however, participants purchase was marginally influenced by the number of seats available. The only contrast that showed significance for number of seats was that of the implicit and explicit text conditions ($F(1, 130) = 5.68, p < .02$).

Discussion. Taken together, these results are convoluted at best—although, several significant differences can be seen between conditions, the more granular contrasts of these conditions indicate that it is not necessarily consistent with the hypotheses. Second, there is the issue that the conditions in which the schematic is shown, gives more information than the conditions with just the text. In the schematic, consumers are better able to understand the size of the plane, the position of the seats, etc. Lastly, using the words “hurry up” prior to the direct
scarcity signal can be easily interpreted as a persuasive message. Therefore, study two will aim to rectify these issues.

**Study 2: Scarcity Signals Information Consistency and No Pressure – Movie Scenario**

This study is motivated to ensure that all participants see the equivalent amount of information across conditions. Furthermore, using phrases like, “hurry up” in conjunction with the explicit scarcity signal is a confound that diminishes the finding in the previous study. To use this additional impetus to act is outside of the scope of the proposed theory and will certainly further bias our results. Study 2 will take shape with these precursors in mind.

*Method.* Seventy-five paid participants were recruited from Amazon Mechanical Turk. Participants were asked to imagine they were planning to go see a movie with a friend. They were told there are two movie premiers that they are interested in seeing. Upon visiting the theater’s website to check for available tickets and select their seats, participants were presented with a graphic showing Movie 1 and Movie 2 and indicating that they had similar start times and needed reserved seating. Unlike in Study 1, participants in all conditions were shown a schematic of the seating available in the theater for both Movie 1 and Movie 2. Movie 1 had sixty-five available seats all over the theater while Movie 2 only had seven seats remaining. Participants were then randomly assigned to one of two conditions—the first condition was presented with just the graphic showing the available seats in each theater (Implicit). The second condition was presented with a graphic, shown in Figure 15, showing the same available seats but each theater had a statement in red that said “Only 65 Seats Left” for Movie 1 and “Only 7 Seats Left” for Movie 2 (Explicit).
Participants then indicated how likely they were to purchase a ticket on a seven-point scale ranging from “Extremely Unlikely 1” to “Extremely Likely 7” for both Movie 1 and Movie 2. On another page, participants were asked if they believed the messaging regarding the seats offered by the movie theater (“Completely DO NOT Believe 1” – “Completely Believe 7”). Furthermore, on the same page, participants were asked to what extent their purchase decision was influenced by the seat locations, seat availability, and number of seats in three separate measures (“Not At All Influenced 1” – “Entirely Influenced 7”). On an additional page, participants were asked to what extent their purchase decision was influenced by preferring more popular movie screenings or less crowded movie screenings. Next, on a separate screen, participants were asked to what extent they felt pressured to make the purchase by the movie theater and the potential the movie would sell out in two separate measures (“Not At All Pressured 1” – “Entirely Pressured 7”). On the final screen, participants were asked an attention check question, their gender, and age prior to thanking them for their participation.
Results. A mixed ANOVA was run on the manipulated, two-level, between-subjects “purchase” factor for both Movie 1 and Movie 2. As seen in Figure 16, the analysis revealed that the implicit and explicit scarcity signals do not significantly affect purchase likelihood for Movie 1 ($M_{\text{implicit}} = 5.78$, $M_{\text{explicit}} = 5.95$, $F(1, 73) = .32, p > .5$) or Movie 2 ($M_{\text{implicit}} = 2.97$, $M_{\text{explicit}} = 3$, $F(1, 73) = .00, p > .9$). Similarly, a mixed ANOVA was run on the manipulated, two-level, between-subjects “believe” factor. The analysis revealed that the implicit and explicit scarcity signals do not significantly affect belief in the messaging ($M_{\text{implicit}} = 5.86$, $M_{\text{explicit}} = 5.71$, $F(1, 73) = .31, p > .5$). The analysis related to seat location ($M_{\text{implicit}} = 5.49$, $M_{\text{explicit}} = 5.18$, $F(1, 73) = .65, p > .4$), seat availability ($M_{\text{implicit}} = 5.78$, $M_{\text{explicit}} = 5.61$, $F(1, 73) = .26, p > .6$), and number of seats ($M_{\text{implicit}} = 4.92$, $M_{\text{explicit}} = 4.95$, $F(1, 73) = .00, p > .9$) all indicated no significant differences between conditions. The analysis related to preferring more popular movie screenings indicated no significance ($M_{\text{implicit}} = 3.08$, $M_{\text{explicit}} = 2.71$, $F(1, 73) = .72, p > .3$) while the analysis related to preferring less crowded movie screenings indicated marginal significance ($M_{\text{implicit}} = 4.59$, $M_{\text{explicit}} = 5.39$, $F(1, 73) = 2.9, p < .1$). Finally, the analysis revealed no
significant difference between conditions related to pressure to make a purchase by the movie theater \((M_{\text{implicit}} = 2.57, M_{\text{explicit}} = 2.5, F(1, 73) = .04, p > .8)\) and pressure that the movie would potentially sell out \((M_{\text{implicit}} = 3.24, M_{\text{explicit}} = 2.97, F(1, 73) = .5, p > .4)\).

*Discussion.* Although this study overcame the inconsistency in amount of information given to each condition and the implied persuasive attempt of adding “hurry up” to the direct scarcity signal, there was no significant difference between our conditions on all measures. Perhaps with a larger sample size, we would see a significant difference in preferring less crowded theaters but that does not pertain to the hypotheses at hand. Furthermore, this context is particularly poor at getting participants involved. Thus, we will attempt to rectify these issues with Study 3.

**Study 3: Scarcity Signals Activation – Car Dealer Scenario**

Unfortunately, it is difficult to determine exactly why Study 2 did not produce the theorized results; however, one potential culprit is participant’s lack of engagement. Study 3 will attempt to further participant’s engagement in a plethora of ways. After all, if the study itself does not engage participants enough then it will be difficult to determine if the activation theory is at work in the implicit scarcity signaling conditions.

*Method.* One hundred and one paid participants were recruited from Amazon Mechanical Turk. Participants were asked to imagine they were in the market for a new car and to answer a few questions about the car that interested them the most. Participants were asked what the make and model of their new car would be and how much they would be willing to spend at most on the new car. Then in an effort to engage with participants more, they imagined a scenario in which they go to a car dealership to check out the car they are interested in and find the make and model at the dealership for significantly less than the amount they were willing to pay. Their
answers from the previous questions about the make, model, and amount are piped in to increase engagement (further piped answers will be indicated by [Car Make] [Car Model]). Participants were then randomly assigned to one of two conditions—the participants in the first condition were presented with a statement that said, “While checking out the car, the salesperson points out that it is the only [Car Make] [Car Model] on the lot” (Explicit). The participants in the second condition were presented with a statement that said, “While checking out the car, you notice that it is the only [Car Make] [Car Model] on the lot” (Implicit). To even further increase participant’s engagement, they were then asked to describe the scenario briefly.

On another page, participants then indicated how likely they were to purchase the [Car Make] [Car Model] on a seven-point scale ranging from “Extremely Unlikely 1” to “Extremely Likely 7”. On another page, participants were asked to what extent they believe they saw all of the dealerships inventory and the information provided by the salesperson in two separate measures (“Completely DO NOT Believe 1” – “Completely Believe 7”). Furthermore, on the same page, participants were asked two questions about what extent their purchase decision was influenced by the availability of the [Car Make] [Car Model] (“Not At All Influenced 1” – “Entirely Influenced 7”). On a separate page, participants were asked to what extent they felt pressured to make the purchase by the salesperson and by the dealership in two separate measures (“Not At All Pressured 1” – “Entirely Pressured 7”). Lastly, on a separate screen, participants were asked their gender and age prior to thanking them for their participation.
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Figure 17: Means Across Conditions Car Dealer

Results. A mixed ANOVA was run on the manipulated, two-level factor (Implicit and Explicit) and all between-subjects measures. As seen in Figure 17, the analysis revealed a marginally significant difference on purchase likelihood (purchase) between conditions ($M_{Implicit} = 5.10, M_{Explicit} = 4.57, F(1, 99) = 2.99, p < .09$), which indicates that participants’ purchase likelihood was marginally significantly different depending on if the scarcity signal was explicit or implicit. Unfortunately, the analysis reveals no significant difference between conditions in the belief participants saw all of the dealerships inventory (allInventory) ($M_{Implicit} = 4.23, M_{Explicit} = 4.09, F(1, 99) = .16, p > .6$) or in what the salesperson told participants (believeSales) ($M_{Implicit} = 4.08, M_{Explicit} = 4.51, F(1, 99) = 1.85, p > .1$). Thus, indicating that the persuasion knowledge model was most likely not triggered differently with explicit vs. implicit scarcity signals. This is further confirmed in that analysis revealed no significant difference between conditions in either availability measure; purchase influenced by the availability of the [Car Make] [Car Model] (availability1) ($M_{Implicit} = 4.50, M_{Explicit} = 4.23, F(1, 99) = .61, p > .4$) and purchase influenced by the number of available [Car Make] [Car Model] (availability 2) ($M_{Implicit}$}
= 4.29, $M_{\text{Explicit}} = 3.77$, $F(1, 99) = 1.92, p > .1$). Thus, further demonstrating that participants were not influenced by an implicit scarcity signal more than an explicit one. However, the analysis does reveal a significant difference between conditions in the amount of pressure to make a purchase perceived by the participants in regards to both the sales person (pressSales) ($M_{\text{Implicit}} = 3.10$, $M_{\text{Explicit}} = 3.94$, $F(1, 99) = 4.92, p < .03$) and the dealership (pressDealer) ($M_{\text{Implicit}} = 3.13$, $M_{\text{Explicit}} = 3.98$, $F(1, 99) = 5.13, p < .03$). Thus, indicating that participants felt more pressured to make a purchase when explicit scarcity signals were used.

**Discussion.** Although this study attempted to engage participants more than previous studies, there is still lacking evidence that consumers believe implicit scarcity signals more than they believe explicit signals. Although only marginally, it is interesting to see some difference between implicit and explicit scarcity signals on purchase likelihood. Furthermore, it is apparent that even without the implied persuasion of a “hurry up” statement, participants still felt more pressured to make a purchase when presented with an explicit scarcity signal than when presented with an implicit scarcity signal.

**Study 4: Scarcity Signals Choice – Wine Store Scenario**

In Study 4, I attempt to make persuasion overtly apparent across all conditions—although, this should equally effect implicit and explicit scarcity conditions, this will make it easier to see the distinction between them if our hypotheses hold. In the explicit condition, we should see participants resist the persuasive attempt in a more exaggerated fashion than in Study 2 and 3. Furthermore, in the implicit condition, participants should be more willing to assume the scarcity signal is authentic and act accordingly.

**Method.** One hundred and forty-five paid participants were recruited from Amazon Mechanical Turk. Participants were first asked to provide some example of ways they thought
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retailers attempt to convince them to buy products. This was designed to prime them to be aware of persuasive tactics and should equally effect both conditions. Unfortunately, in other studies, it is difficult to tell if participants detected a persuasive element; therefore, priming them to be aware of persuasive attempts should help.

*Figure 18: Wine Graphic*

On a separate page, participants were asked to imagine they were running early on their way to a friend’s house and decided to stop in a wine store to see if there is anything they could share with their friend. On a separate page, participants are told that it is only them and the clerk in the store, so they have the clerk’s undivided attention and the clerk helps discuss wine options. The clerk and participants narrow the wine selection down to two bottles Wine A and Wine B. Participants are shown a picture of the two identical bottles only differing in the letter label (Figure 18). Participants were then randomly assigned to one of two conditions—the participants in the first condition were presented with a statement that said, “You are leaning toward purchasing Wine B. While looking at the display, the clerk points out that there are only two bottles of Wine A remaining” (Explicit). The participants in the second condition were presented with a statement that said, “You are leaning toward purchasing Wine B. While looking at the display, you notice that there are only two bottles of Wine A remaining” (Implicit). Participants were told that they were leaning toward purchasing the less scarce selection of Wine B to further
prime and persuasive attempts to get them to switch to Wine A (both explicit and implicit). Again, in order to make persuasive attempts as salient as possible in participants’ minds, they have been instructed that they already favor one brand. An attempt to pull them away from that brand should seem all the more suspicious and further trigger their resistance across conditions.

Participants were then asked to describe the scenario briefly. Furthermore, participants were asked how likely they are to purchase Wine A or Wine B on two seven-point scales ranging from “Extremely Unlikely 1” to “Extremely Likely 7”. On another page, participants indicated to what extent they believe they saw all of the wine store’s inventory and to what extent there are less bottles of Wine A in the store than Wine B in two separate measures (“Completely DO NOT Believe 1” – “Completely Believe 7”). Furthermore, participants were asked to what extent their purchase decision was influenced by the availability of Wine A and, in a separate measure, the availability of Wine B in three separate measures (“Not At All Influenced 1” – “Entirely Influenced 7”). On a separate page, participants were asked if they felt pressured by the clerk to purchase Wine A and, in a separate measure, pressured by the clerk to purchase Wine B (“Not At All Pressured 1” – “Entirely Pressured 7”). On another screen, participants were asked an attention check question, their gender, and age prior to thanking them for their participation.
Results. A mixed ANOVA was run on the manipulated, two-level, between-subjects factor (Implicit and Explicit). As seen in Figure 19, the analysis revealed no significant difference on purchase likelihood for Wine A between conditions ($M_{Implicit} = 4.12$, $M_{Explicit} = 3.68$, $F(1, 143) = 2.16, p > .1$), which indicates that participants’ purchase likelihood for Wine A (the scarcer wine) was not different depending on if the scarcity signal was explicit or implicit. Similarly, the analysis revealed no significant difference on purchase likelihood for Wine B between conditions ($M_{Implicit} = 4.36$, $M_{Explicit} = 4.74$, $F(1, 143) = 2.24, p > .1$), which indicates that participants’ purchase likelihood for Wine B was not different depending on if the scarcity signal was explicit or implicit. Unfortunately, the analysis revealed no significant difference on the belief participants saw all of the wine in the store’s inventory ($M_{Implicit} = 3.61$, $M_{Explicit} = 3.54$, $F(1, 143) = .06, p > .7$) and to what extent participants believed there are less bottles of Wine A in the store than Wine B ($M_{Implicit} = 4.87$, $M_{Explicit} = 4.65$, $F(1, 143) = .57, p > .4$). Thus, offering no evidence that participants believed implicit scarcity signals more than explicit signals. Although, there was no significance to purchase likelihood or believability of the scarcity
signals, analysis revealed that there was a significant difference between being exposed to an explicit or implicit scarcity signal in relation participants being influenced by the availability of Wine A ($M_{\text{Implicit}} = 4.21, M_{\text{Explicit}} = 3.35, F(1, 143) = 6.10, p < .02$) and Wine B ($M_{\text{Implicit}} = 3.94, M_{\text{Explicit}} = 3.31, F(1, 143) = 4.17, p < .05$). Thus, indicating that participants felt more influenced by availability of the wines when they were exposed to an implicit (vs. explicit) scarcity signals.

Finally, the analysis showed significant difference in pressure to purchase Wine A between conditions ($M_{\text{Implicit}} = 2.57, M_{\text{Explicit}} = 3.68, F(1, 143) = 13.41, p < .0005$) and no significant difference to purchase Wine B between conditions ($M_{\text{Implicit}} = 2.16, M_{\text{Explicit}} = 1.99, F(1, 143) = .58, p > .4$). Thus, indicating that participants were more pressured to purchase Wine A when presented with an explicit scarcity signal and also not pressured to purchase Wine B when presented with an explicit or implicit scarcity signal relating to Wine A.

Discussion. Although we consistently see differences in pressure in Study 3 and 4, we see inconsistent indications for purchase likelihood across studies and no indication that belief changes related to what scarcity signal a consumer is presented with. In this study, we did see indication that consumers feel more influenced to purchase by implicit than explicit scarcity signals. The information presented to each participant, regardless of condition, has remained consistent in the Study 2, 3, and 4; however, one could argue that all conditions see the implicit scarcity signal and the explicit scarcity signal is additional information. Thus, simply by being given additional information, participants are somehow more influenced. This will be addressed in Study 5.
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Study 5: Scarcity Signals Additional Information – Wine Store Scenario

If the explicit scarcity signal is simply thought of as additional information, then we could see pressure to purchase measures rise amongst participants regardless of the scarcity signal. This pressure to purchase could simply be interpersonal pressure have nothing to do with the scarcity signal—in order to test this, we would need to have a condition where the firm gives additional information that does not include an explicit scarcity signal and compare that with the explicit scarcity signal conditions. Study 5 aims to do just that.

Method. One hundred and twenty-nine paid participants were recruited from Amazon Mechanical Turk. Similar to Study 4, participants were first asked to provide some example of ways they thought retailers attempt to convince them to buy products. On a separate page, participants were asked to imagine they were running early on their way to a friend’s house and decided to stop in a wine store to see if there is anything they could share with their friend. On a separate page, participants are shown the graphic in Figure 20 and told they are considering two wines; Wine A and Wine B. Participants were then randomly assigned to one of three conditions. The participants in the first condition were shown no further manipulations—the graphic itself illustrates that Wine A is scarce (Implicit). The participants in the second condition were shown the graphic in Figure 20 as well as a statement that says, “The clerk points out that there are only two bottles of Wine A remaining” (Explicit). The participants in the third condition were shown the graphic in Figure 20 as well as a statement that says, “The clerk recommends either wine” (Control). The third condition represents the control in that participants are still shown further information, yet this information does not contain a further scarcity signal.
Participants were then asked to describe the scenario briefly. Furthermore, participants were asked which wine are they more likely to purchase on a six-point scale that consists of, “Definitely Wine A”, “Probably Wine A”, “Maybe Wine A”, “Maybe Wine B”, “Probably Wine B”, and “Definitely Wine B”. On another page, participants were asked to what extent their purchase decision was influenced by the availability of Wine A and, in a separate measure, the availability of Wine B (“Not At All Influenced 1” – “Entirely Influenced 7”). Furthermore, participants were asked if they felt pressured by the clerk to purchase Wine A and, in a separate measure, pressured by the clerk to purchase Wine B (“Not At All Pressured 1” – “Entirely Pressured 7”). On a separate page, participants were asked if they believed the wine store controls the inventory customers see, manipulates the inventory customers see, manipulated the inventory of Wine A to communicate scarcity, and attempting to convince you to buy Wine A by making it seem more scarce (“Completely DO NOT Believe 1” – “Completely Believe 7”). On a separate screen, participants were asked a manipulation check question, attention check question, their gender, and age prior to thanking them for their participation.

Results. A mixed ANOVA was run on the manipulated, three-level, between-subjects factor (Implicit, Explicit, Control). The analysis revealed no significant difference on purchase
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likelihood between conditions ($M_{\text{Implicit}} = 2.56, M_{\text{Explicit}} = 2.54, M_{\text{Control}} = 2.74, F(2, 126) = .51, p > .5$) which indicates that participants’ purchase likelihood for either wine was not different depending on if the scarcity signal was implicit, explicit, or implicit with a control of more information. Interestingly, although analysis revealed no significant difference in the extent participants purchase decision was influenced by the availability of Wine A ($M_{\text{Implicit}} = 5.42, M_{\text{Explicit}} = 5.03, M_{\text{Control}} = 5.23, F(2, 126) = .59, p > .5$), analysis did reveal significant differences in the extent participants were influenced by the availability of Wine B between conditions ($M_{\text{Implicit}} = 4.60, M_{\text{Explicit}} = 3.95, M_{\text{Control}} = 5.02, F(2, 126) = 4.90, p < .01$). This indicates that the availability of Wine B influences decisions more when explicit scarcity signal is not present. Furthermore, analysis showed that there was no significant difference between pressure to purchase Wine A ($M_{\text{Implicit}} = 2.70, M_{\text{Explicit}} = 3.28, M_{\text{Control}} = 3.47, F(2, 126) = 1.78, p > .1$) or pressure to purchase Wine B between conditions ($M_{\text{Implicit}} = 2.37, M_{\text{Explicit}} = 1.90, M_{\text{Control}} = 2.28, F(2, 126) = 1.11, p > .3$). Thus far, pressure has demonstrated fairly robust differences between the two types of scarcity; however, these results indicate that this difference might have been simply from the representative, the clerk in this example, giving any additional information. Lastly, analysis showed that all believability measures were not significantly different between conditions; believed the wine store controls the inventory customers see ($M_{\text{Implicit}} = 5.09, M_{\text{Explicit}} = 5.38, M_{\text{Control}} = 4.89, F(2, 126) = 1.18, p > .3$), manipulates the inventory customers see ($M_{\text{Implicit}} = 4.58, M_{\text{Explicit}} = 4.71, M_{\text{Control}} = 4.36, F(2, 126) = .54, p > .5$), manipulated the inventory of Wine A to communicate scarcity ($M_{\text{Implicit}} = 4.49, M_{\text{Explicit}} = 4.46, M_{\text{Control}} = 4.13, F(2, 126) = .62, p > .5$), and attempting to convince you to buy Wine A by making it seem more scarce ($M_{\text{Implicit}} = 4.58, M_{\text{Explicit}} = 4.36, M_{\text{Control}} = 4.02, F(2, 126) = 1.26, p > .2$).
**Discussion.** Although we consistently see differences in pressure in Study 3 and 4, the findings in Study 5 indicate that the difference in pressure felt by consumers might have more to do with the fact that a representative is giving them more information than a particular scarcity signal. Study 6 will move away from product scarcity and move into deal scarcity.

**Study 6: Scarcity Signals Discounts – Music Store Scenario**

**Method.** One hundred and seven paid participants were recruited from Amazon Mechanical Turk. Similar to Study 4 and 5, participants were first asked to provide some example of ways they thought retailers attempt to convince them to buy products. On a separate page, participants were asked to imagine they were shopping for a small birthday gift for their musician friend. Upon searching the web for potential gift ideas, they find out that a harmonica is an affordable gift idea. They go to the website of a popular woodwind instrument company and narrow the selection down to one harmonica. Participants were then randomly assigned to one of three conditions. The participants in the first condition were shown a graphic similar to that of Figure 21 sans the text after “Annual Sale” (Control). This differs from the control used in Study 5 in that participant are shown no scarcity signal whatsoever here vs. being shown scarcity and given additional information. The participants in the second condition were shown a graphic matching Figure 21 (Explicit). The participants in the third condition were shown a graphic similar to Figure 21 except it featured text in red after “Annual Sale” that read “Available Until [piped text featuring a date two days after the participants completed the experiment]” (Implicit). This implicit scarcity signal varies from ones used in previous studies—however, the goal of this signal is congruent in that it aims to allow participants to discover the scarcity through more in depth activation.
Participants were then asked to describe the scenario briefly. Furthermore, participants were asked how likely they are to purchase the harmonica on a seven-point scale ranging from “Extremely Unlikely 1” to “Extremely Likely 7”. Then, in an effort to move from the inconsistent dependent measure of purchase likelihood to a measure that could be more effected by pressure, participants were asked how satisfied they are with their shopping experience “Extremely Unsatisfied 1” to “Extremely Satisfied 7”). On another page, participants were asked to what extent their purchase decision was influenced by the availability of the Annual Sale (“Not At All Influenced 1” – “Entirely Influenced 7”). Furthermore, participants were asked to what extent they felt pressured to purchase the harmonica (“Not At All Pressured 1” – “Entirely Pressured 7”). On a separate page, participants were asked if they believed the music store was attempting to convince them to buy the harmonica by making the Annual Sale seem scarcer (“Completely DO NOT Believe 1” – “Completely Believe 7”). Finally, on a separate screen, participants were asked a manipulation check question, attention check question, their gender, and age prior to thanking them for their participation.
Results. A mixed ANOVA was run on the manipulated, three-level, between-subjects factors (Implicit, Explicit, Control). As shown in Figure 22, the analysis revealed marginally significant difference on purchase likelihood for between conditions (\(M_{\text{Implicit}} = 5.00, M_{\text{Explicit}} = 5.22, M_{\text{Control}} = 5.78\), \(F(2, 104) = 2.77, p < .07\)), which indicates that participants’ purchase likelihood was marginally different depending on if the scarcity signal was implicit, explicit, or non-existent. However, contrary to our theory and that of other scarcity theories, it indicates that consumers are more likely to purchase the harmonica when no scarcity signal is present. Furthermore, contrary to our theory, these findings indicate that consumers are more likely to purchase the harmonica when explicit scarcity is present than when implicit scarcity is present. Unfortunately, the analysis of the newly introduced measure of satisfaction showed no significant difference between conditions (\(M_{\text{Implicit}} = 5.26, M_{\text{Explicit}} = 5.19, M_{\text{Control}} = 5.63\), \(F(2, 104) = 1.01, p > .3\)). These results indicate that consumers are not more satisfied with a shopping experience if scarcity signals are present or not, regardless of their explicit nature. The analysis of the availability measure revealed that there was significant differences between conditions.
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($M_{\text{Implicit}} = 4.77$, $M_{\text{Explicit}} = 5.00$, $M_{\text{Control}} = 3.90$, $F(2, 104) = 4.75$, $p < .02$). These findings indicate that consumers again find themselves influenced by the availability of the deal; however, in this case, participants are more influenced when the deal is explicitly scarce than when they have to exert more resources to discover how scarce it is. Interestingly, the analysis revealed a significant difference of pressure between conditions ($M_{\text{Implicit}} = 3.48$, $M_{\text{Explicit}} = 4.22$, $M_{\text{Control}} = 2.23$, $F(2, 104) = 13.71$, $p < .0001$). These findings indicate that consumers feel less pressure when presented with no scarcity signal. A contrast between the Control and Implicit conditions further demonstrates this ($F(1, 104) = 9.76$, $p < .003$). A similar contrast between the Control and Explicit conditions shows similar significant difference ($F(1, 104) = 26.65$, $p < .0001$). A contrast between the Explicit and Implicit conditions shows a marginally significant difference ($F(1, 104) = 3.20$, $p < .07$). Thus, giving evidence that consumers feel marginally less pressure when implicit than when explicit scarcity signals are present. Lastly, the analysis revealed a significant difference of belief between conditions ($M_{\text{Implicit}} = 5.58$, $M_{\text{Explicit}} = 5.92$, $M_{\text{Control}} = 4.35$, $F(2, 104) = 11.15$, $p < .0001$). These findings indicate that consumers believe the music store was attempting to convince them to buy the harmonica by making the Annual Sale seem scarcer when a scarcity signal is present. A contrast between the Control and Implicit conditions further demonstrates this ($F(1, 104) = 11.40$, $p < .002$). A contrast between the Control and Explicit conditions shows similar significant difference ($F(1, 104) = 20.04$, $p < .0001$). However, a contrast between the Explicit and Implicit conditions shows no significant difference ($F(1, 104) = .81$, $p > .3$). These findings indicate that consumers feel no difference in belief that the store is attempting to convince them to buy the harmonica when presented with implicit than when explicit scarcity signals.
Discussion. Although Study 6 covered the novel context of deal scarcity, these results indicate that the hypotheses are misguided. Further evidence of this point can be seen in the control condition of this study demonstrating how a lack of a scarcity signal acts compared to implicit and explicit scarcity. Ultimately, additional studies are unnecessary—the original hypotheses do not hold. To note, throughout this empirical approach, several others studies were conducted that too closely resemble the studies outlined here to warrant a place in this essay.

GENERAL DISCUSSION

In the third chapter of this dissertation, I examined the extent to which consumers’ beliefs of scarcity signals presented by a firm difference based on the manner in which those signals are delivered. Drawing on literature from creativity (Goldenberg, Mazursky, and Solomon 1999a, 1999b) and persuasion (Friestad and Wright 1994; Chaiken et al. 1987; Petty and Cacioppo 1986), I proposed that consumers are more likely to believe implicit (vs. explicit) scarcity signals. Specifically, I proposed that implicit scarcity signals require the consumer to engage in the communication and alter the perceived source and intention of the scarcity signal. Ultimately, the hypotheses indicated that the consumer would no longer feel the source of the scarcity signal is the firm and that the intentions are to persuade the consumer. Further, when presented with an implicit scarcity signal, consumers are more likely to believe the signal is authentic and alter their purchasing behavior to match previous scarcity literature. Unfortunately, the hypotheses do not hold.

Theoretical Implications. Since the theory was grounded in prior research, it is worth exploring why the hypotheses did not hold. Foremost, it should be examined whether that the types of manipulation and strengths of manipulation used in the studies are substandard; however, given the amount of studies run and the varying manipulations, this seems less likely
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than other alternatives. Secondly, the hypotheses not holding could be related to the measures being substandard. This is entirely plausible—the measures could have been miscalibrated or insensitive to shifts in the underlying constructs. It is possible the measures did not accurately communicate the way participants were feeling. Ultimately, the reasons these hypotheses did not hold were most likely related to it not being an accurate representation of how consumers behave. It would seem that people do not perceive implicit and explicit scarcity signals as being different—at least not consistently so. Although a large portion of the theory is based on extant research related to how consumers treat persuasive attempts and scarcity signals, the theory is also based on the activation template (1999a, 1999b). As stated in the theory introduction, there is little empirical evidence that messages or products crafted with this template are more persuasive or, critically, more believable. Perhaps perceived effort on the part of the consumer is not substantial enough for them to attribute the scarcity discovery to themselves. Furthermore, perhaps this effort on the part of the consumer does not offset the source knowledge—i.e. consumers ultimately know the information comes from the firm.

Managerial Implications. Although it is difficult to elaborate on null hypotheses, it stands to reason that some of the lack of findings in this essay could be useful to managers. Given that purchase likelihood was mostly insignificantly affected by the presence of an explicit (vs. implicit) scarcity signal, it would make sense for managers to employ implicit scarcity signals when optional. In doing so, managers could avoid much of the perceived pressure exerted by the firm on their customers. Although not captured in the studies associated with this paper, perhaps this pressure would negatively affect customer relationships, either overtime or per individual visit. As the explicit vs. implicit scarcity signals did not make a demonstrable difference in likelihood of sale, it would seem worthwhile to reduce the potential for conflict or pressure.
Future Direction. Consumers encounter scarcity regularly—unfortunately, from the consumer perspective it is almost impossible to confirm if this scarcity is driven by market forces or if the scarcity of a product or service is being manipulated to affect the persuasiveness of a marketer’s message. Based on the lack of findings accompanying the research, it appears that consumers, even primed to be cognizant of scarcity manipulations, do not treat explicit scarcity any different than implicit scarcity in regards to the authenticity of the scarcity. In a “real world” setting, this comparison might still hold—in addition, many other factors may pollute such observations. Therefore, one might argue that these findings would be different in the dramatically more intricate setting of everyday life. Therefore, it would be interesting to try a similar manipulation to some of the studies in a field experiment.

When conducting future research about deal scarcity, it is important to note that a deal, by nature, is scarce. Therefore, one might argue that the control condition in Study 6 is really the most implicit scarcity signal. Ultimately, participants in this condition needed to recognize the deal was scarce through their own efforts—not through any mechanism explicitly telling them. If this is considered the case, the findings of this study are the most robust in favor of the hypothesis throughout the entire paper. Future studies would need to be conducted to determine if participants actually perceived the deal as scarce.

Beyond that, if the lack of findings in the third chapter of this dissertation are any indication, it appears that belief is not a factor that needs to be considered when discussing scarcity signals. However, perhaps this research could be drawn on to develop a deeper understanding of pressure. More specifically, if increased pressure could lead to less satisfaction—either with the purchase or the purchase environment. Ultimately, this pressure
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could influence a customer’s willingness to return to a store or purchase products from an organization.

Another novel question that stemmed from this research is: are people less skeptical of persuasive attempts in digital vs. interpersonal interactions? One could argue that there are more cues to go off of in interpersonal communication—since it is much easier to perfectly craft a message in a digital context than an interpersonal one. Beyond that, this idea rides a fine line of being too obvious and being surprising if people are actually less skeptical and potentially more susceptible to persuasive attempts in digital compared to interpersonal contexts. It appears, based on the findings, that people feel more pressure from interpersonal than digital communications.
CHAPTER 4:

Summary and Closing Thoughts

In this dissertation, I argued that belief, more specifically consumers’ belief in claims or material generated by the firm, should be more adequately examined in contemporary consumer behavior literature. To this end, I reviewed current theories that attempt to understand belief in consumer behavior literature. Furthermore, I explored two empirical examinations of how certain factors influence consumers’ belief in firm-generated communication and how differing levels of belief affect consumer behavior. My goal was to better understand the relationship between belief and consumer behavior, and, in particular, how belief can be managed more appropriately from both a managerial and a consumer standpoint.

In the second chapter of this dissertation, I examined the extent to which consumers’ beliefs of the claims made by a point of contact (POC) evolve over repeated service failures. I draw on literature from deception detection (Ekman and Friesen 1969; Ekman 2009) and coincidence (Dessalles 2006; Dessalles 2008; Dimulescu and Dessalles 2009) to propose that consumers are more likely to believe a POC when the POC gives different (vs. the same) reasons for each of the service failures. Specifically, I established that ostensible coincidences (the same reason given repeatedly for service failures) provided by the POC lead consumers to discount these explanations for the service failures. Consumers find ostensible coincidences less believable although these coincidences suggest a systemic cause for the service failure that is both more likely and more expected by the consumer a priori. Further, when presented with an ostensible coincidence, consumers are more likely to blame the POC for the service failure.
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In the third chapter of this dissertation, I conducted research in an attempt to outline the extent to which consumers’ beliefs of scarcity signals presented by a firm differ based on the manner in which those signals are delivered. Drawing on literature from creativity (Goldenberg, Mazursky, and Solomon 1999a, 1999b) and persuasion (Friestad and Wright 1994; Chaiken et al. 1987; Petty and Cacioppo 1986), I theorized that consumers are more likely to believe implicit (vs. explicit) scarcity signals. Specifically, implicit scarcity signals require the consumer to engage in the communication and alter the perceived source and intention of the scarcity signal. The consumer will no longer feel the source of the scarcity signal is the firm and that the intentions are to persuade the consumer. Further, when presented with an implicit scarcity signal, consumers should be more likely believe the signal is authentic and alter their purchasing behavior to match previous scarcity literature. Unfortunately, my research yielded mostly null results.

Further, I initially hypothesized that believability played an active role in consumer interactions. I aimed to help shape the understanding of the role of believability in consumer communications through an examination of both communication around ostensible coincidences in service failures and in the explicit and implicit communication of scarcity. Given that my results across these two aspects of believability achieved differing results, it would appear that this broader topic deserves further exploration and that belief should not be assumed or ignored as a factor in other evaluations of consumer behavior.
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