EXPLORING CHALLENGES AND THE EVOLUTION OF THE RETAIL INDUSTRY: A CONSUMER PERSPECTIVE

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ABSTRACT

Over the last several years, the retail industry has faced increasing pressure due to the rise of e-commerce and consumer expectations. Further, retail supply chains have faced unprecedented challenges due to the disruptions from the COVID-19 pandemic, which has led to shifts in consumers' shopping behavior. An in-depth exploration of these pressures and challenges is hence warranted. The aim of this three-essay dissertation is to examine: (1) consumer behavior in retail supply chains over the recent period of change and (2) how consumers react to retail supply chain issues of shortages and stockouts. In doing so, I aim to contribute to the consumer-centric supply chain management (SCM) literature.

Accordingly, in the first essay (see Chapter 2), I explore the pattern of e-commerce growth from 2010 through 2020 from a regionalized perspective. In particular, I examine whether there are different adoption patterns in urban versus rural markets and the impact of the COVID-19 shock on e-commerce growth. By employing secondary data from NielsenIQ's Consumer Panel Dataset and applying discontinuous growth models, I find that consumers in urban areas adopted e-commerce to a greater degree and, to some extent, at a faster rate during the period of 2010 through 2019. Following the onset of the pandemic, urban areas also experienced the greatest increase in adoption of e-commerce. However, controlling for the urban and rural nature of an area, the areas that had the highest levels of e-commerce adoption prior to COVID-19 (in 2019) exhibited a smaller increase in adoption in 2020, indicating changing adoption patterns.

In the second essay (see Chapter 3), I examine consumer stockpiling behavior in response to shortages of perishable products and how this behavior is moderated by household income. I explore this topic in the context of COVID-19 related issues in the meat industry in the United States (U.S.) during late spring and early summer 2020 and utilize NielsenIQ's Consumer Panel

Dataset to employ a difference-in-differences design. Overall, I find evidence that consumers do stockpile perishable products facing a scarcity, and this effect continues even after the threat of the shortage is subsiding. Additionally, lower income households stockpile to a greater degree than higher income households, indicating that income is an important boundary condition of stockpiling behavior.

Lastly, in the third essay (see Chapter 4), I explore whether retailers can influence consumer reactions to stockouts by disclosing the cause of the stockout. I focus on whether disclosing different stockout causes impacts consumers' repurchase intentions via trust in the online retailer and whether this relationship is moderated by consumer gender. I employ a series of scenario-based experiments and find that the disclosure of some stockout causes in online retail can serve as signals impacting consumer behavior, with opportunities to influence both trust and change in trust over time and subsequent repurchase intentions. This effect, however, is not moderated by the gender of the consumer.

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CHAPTER 1 – INTRODUCTION AND MOTIVATION

Over the last several years, the retail industry has faced increasing pressure and challenges from a supply chain management (SCM) perspective due to the rise of e-commerce (Hortaçsu & Syverson, 2015; U.S. Census Bureau, 2023) and increasing consumer expectations (Daugherty et al., 2019). Further, in the past few years, retail supply chains have been tested by the impacts of the COVID-19 pandemic, which has resulted in challenges such as upstream manufacturing disruptions (Naughton & Hufford, 2020), import transportation capacity challenges (Maiden, 2021), and altered consumer shopping patterns (Bureau of Economic Analysis, 2022; Internal Revenue Service, 2021; Nassauer & Kapner, 2020). In response to these pressures and challenges, the retail environment has evolved rapidly (Brewster, 2022; Nassauer & Maloney, 2020). Thus, to understand the evolution of the industry and how retailers can respond to challenges, the aim of this three-essay dissertation is to examine: (1) consumer behavior in retail supply chains over the recent period of change and (2) how consumers react to retail supply chain issues of shortages and stockouts. In doing so, I contribute to the consumer-centric supply chain management (SCM) literature (Esper et al., 2020; Esper & Peinkofer, 2017) and develop important theoretical insights as well as managerial implications to influence supply chain strategies.

Accordingly, given the increase in online shopping (i.e., e-commerce) in the United States (U.S.) (Hortaçsu & Syverson, 2015) and an acceleration in adoption of e-commerce at the onset of COVID-19 in 2020 (Torry, 2020), in the first essay (see Chapter 2) I explore the pattern of e-commerce growth in the U.S. from 2010 through 2020. I take a regionalized perspective, focusing on whether consumers in urban or rural areas adopt e-commerce more quickly, and I look at adoption prior to and during the COVID-19 pandemic. In doing so, I reconcile conflicting findings in the literature, which has found support for both an innovation diffusion perspective (e.g., Farag et al., 2006)—whereby it is expected that urban consumers will be more likely than rural consumers to adopt e-commerce—and an efficiency perspective (e.g., Farag et al., 2006)—whereby it is expected that rural consumers will be more likely than urban consumers to adopt e-commerce. To develop my hypotheses, I draw on the innovation diffusion and efficiency perspectives (e.g., Anderson et al., 2003; Farag et al., 2006) as well as the consumer-centric retail framework and four stages of shopping (Gauri et al., 2021). From a methodological perspective, I utilize discontinuous growth models (Bliese & Lang, 2016) and employ secondary data from Nielsen IQ's Consumer Panel Dataset, the Economic Research Service U.S. Department of Agriculture, and the U.S. Census Bureau.

Overall, in the first essay I find some evidence that urban consumers adopt e-commerce at a faster rate than rural consumers, and this is especially evident as it relates to the jump in adoption exhibited at the onset of COVID-19. Further, areas that had a lower level of online shopping adoption just prior to the onset of COVID-19 (2019) exhibited the greatest increase in adoption in 2020. From a theoretical perspective, my first essay contributes to (a) the empirical literature that explores the spatial determinants of e-commerce adoption that focuses on differences in adoption based on whether consumers are in an urban or rural area (e.g., Beckers et al., 2021; Cao et al., 2013; Kirby-Hawkins et al., 2019; Zhou & Wang, 2014) and (b) the consumer-centric SCM literature (Esper et al., 2020). From a managerial perspective, the findings from this essay inform both retailers and last mile delivery providers regarding the pattern of e-commerce adoption to influence network decisions, especially given the different challenges to serve urban versus rural areas from a last mile perspective (Bretzke, 2013; Rose et al., 2016; Rose et al., 2020).

In the second essay (see Chapter 3), given increased discussion around consumers' stockpiling behavior (Knoll, 2020; Taylor, 2020) and a heightened environment of supply chain disruptions (Flynn et al., 2021), I examine consumer stockpiling behavior in response to shortages of perishable products. I concentrate on perishable products given that this focus has been neglected in the literature and that the nature of perishable products—which results in unique challenges as it relates to storage, for example—might result in different consumer behavior as compared to commonly explored nonperishable products (e.g., Prentice et al., 2022; Yoshizaki et al., 2020). Further, given that retailers can tailor inventory management decisions based on demographics of their stores' customers and that there has been a call to explore how different consumers behave in relation to supply chain topics (Esper & Peinkofer, 2017), I explore the role of household income as a boundary condition (Makadok et al., 2018) of stockpiling behavior. Drawing on scarcity literature (e.g., Bell, 1982; Lynn, 1991), I explore this topic in the context of COVID-19 related issues in the meat industry in late spring and early summer 2020. I utilize data from NielsenIQ's Consumer Panel Dataset and the U.S. Bureau of Labor Statistics to employ a difference-in-differences design.

As a result of the second essay, I find evidence that consumers do stockpile perishable products facing a scarcity, and this effect continues even after the threat of the shortage is subsiding. Additionally, lower income households stockpile to a greater degree than higher income households. From a theoretical perspective, essay two contributes to the literature regarding stockpiling during disruptions as well as the scarcity literature. This research also provides insights for managers, informing retailers and their suppliers to more accurately forecast demand and allocate limited inventory during and following a scarcity of perishable products.

Lastly, given the increased prominence of stockouts (e.g., Maloney & Terlep, 2022; Nassauer & Terlep, 2021; Scott & Kapner, 2021; Thomas, 2021) and supply chain disruptions (Flynn et al., 2021) which have resulted in greater consumer awareness of supply chain issues

(Shih, 2022), the third essay (see Chapter 4) explores whether retailers can influence consumer reactions to stockouts by disclosing the cause of the stockout. In particular, I focus on whether disclosing different stockout causes impacts consumers' repurchase intentions (RPI) via trust in the online retailer given trust is especially important in online retailing (Gefen, 2000; Grabner-Kraeuter, 2002). Further, I explore whether this relationship is moderated by consumer gender, as (a) the SCM literature has called for a consumer-focused approach to understand how segments of consumers behave (Esper & Peinkofer, 2017) and (b) retailers aim to tailor advertisements and information to individual consumers (Halzack, 2015).

Drawing on signaling theory (Connelly et al., 2011; Spence, 1973) and the impression formation literature (Kim et al., 2006; Paruchuri et al., 2021), I employ a series of scenario-based experiments to test my hypotheses. While Experiment 1 focuses on broad stockout causes, subsequent experiments build upon this by exploring four more contextualized operational (Moussaoui et al., 2016) stockout causes, as compared to not disclosing a stockout cause. Further, Experiments 3A and 3B explore the development of trust over time—measuring trust in the online retailer both before and after the stockout encounter—rather than looking at the level of trust only following a stockout. Further, while Experiments 1, 2, and 3A utilize a fictitious online retailer in the stockout scenario, representing a small, startup retailer (Peinkofer & Jin, 2023), Experiment 3B replicates Experiment 3A by looking at the effect for two well-known retailers. This enables the establishment of external validity and exploration of whether retailer reputation is a boundary condition for the effect of disclosing stockout causes (Makadok et al., 2018).

In the third essay, I find that there are opportunities to influence levels of trust and change in trust over time, subsequently influencing RPI in both positive and negative ways, by disclosing upstream and focal firm stockout causes to consumers. However, disclosing a downstream stockout cause does not impact trust and subsequent RPI. In general, upstream causes pose an opportunity to generate higher levels of trust and subsequent RPI as compared to not disclosing a stockout cause, while focal firm causes can negatively impact trust levels and RPI. As it relates to change in trust over time, however, small or startup retailers as well as established retailers with positive reputations have opportunities to develop trust even by disclosing focal firm-related causes; however, established retailers with weaker reputations can build trust or risk diminishing trust depending on the type of stockout cause disclosed. I also find a lack of significant results regarding the moderating role of gender, indicating that retailers do not need to cater their stockout cause disclosure strategy based on consumer gender. This essay contributes to literature which empirically explores how information disclosure can influence consumer reactions to stockouts, as well as literature that studies trust in online retailing. From a managerial perspective, this research generates insights for retailers in terms of when to disclose, or not disclose, the cause of a stockout.

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CHAPTER 2 – THE EVOLUTION OF E-COMMERCE ADOPTION: A REGIONALIZED PERSPECTIVE

2.1 INTRODUCTION

Over the last several years, there has been an increase in online shopping by consumers in the United States (U.S.) (Hortaçsu & Syverson, 2015). Illustratively, e-commerce spend accounted for 4.4% of total retail sales in 2010, and retailers such as Walmart began to heavily focus on their e-commerce strategies by the mid-2010s (Tabuchi, 2015). By 2019, e-commerce spend grew to account for 10.6% of retail sales (U.S. Census Bureau, 2023a). Further, adoption was accelerated by the onset of the COVID-19 pandemic (Torry, 2020), with e-commerce spend accounting for 14.5% of total retail sales in 2020 (U.S. Census Bureau, 2023a).

To understand the adoption of online shopping, the academic literature has explored how various factors—which can influence how supply chain parties, such as retailers and last mile delivery providers, tailor their services—can impact consumers' adoption of e-commerce (e.g., Avery et al., 2012; Fisher et al., 2019; Yoh et al., 2003). One stream of literature has addressed one such factor and studies a regional perspective of adoption of e-commerce, focusing on adoption patterns in rural versus urban areas. However, this literature has resulted in conflicting findings, supporting both an innovation diffusion perspective (e.g., Farag et al., 2006b)—whereby it is expected that urban consumers will be more likely than rural consumers to adopt e-commerce—and an efficiency perspective (e.g., Farag et al., 2006b)—whereby it is expected that rural consumers will be more likely than urban consumers to adopt e-commerce. This study aims to reconcile these conflicting findings by addressing several features of the extant literature.

Accordingly, this essay addresses the following research questions: *How has the adoption of e-commerce evolved over the period of 2010 to 2020? How has it differed in urban versus rural areas? How did the level of adoption prior to COVID-19 relate to the adoption increase after the* *onset?* Exploring the e-commerce adoption patterns of urban versus rural areas is important from a supply chain management (SCM) perspective for two primary reasons. First, the challenges to serve urban areas are unique as compared to the challenges to serve rural areas from a last-mile perspective (Bretzke, 2013; Rose et al., 2016; Rose et al., 2020a). For example, urban areas face greater traffic congestion (Rose et al., 2016) and constraints as it relates to making left turns or easily changing directions (Rose et al., 2020a; Rosenbush & Stevens, 2015). Further, real estate costs in urban areas make it difficult to locate fulfillment centers close to urban consumers (Gibson et al., 2018). In contrast, rural areas consist of less concentrated delivery areas and, therefore, experience higher last mile costs and longer delivery times (Roberson, 2021). Accordingly, it is important for retailers and last mile transportation providers to be aware of adoption patterns to tailor their networks and services appropriately. Second, some last mile delivery providers have over-extended their networks in recent years. FedEx, for example, extended weekend deliveries following a surge in demand at the height of the COVID-19 pandemic but in 2022 revealed that they would cut back on Sunday deliveries in some rural areas to increase efficiency (Matthews, 2022), and they cut back further early in 2023 (Black, 2023). This indicates that providers need a deeper understanding of e-commerce demand patterns over time to better plan expanded offerings for the last mile, which are driven by e-commerce demand. This is similarly important for retailers as they look to further build out their e-commerce networks.

Drawing on the innovation diffusion and efficiency perspectives (e.g., Anderson et al., 2003; Farag et al., 2006b) as well as the consumer-centric retail framework and four stages of shopping (Gauri et al., 2021), I hypothesize regarding the expected adoption patterns prior to and following the onset of the COVID-19 pandemic. I rely on data from NielsenIQ's Consumer Panel Dataset from 2010 through 2020 to measure the percentage of shopping trips in the online channel each year and from the Economic Research Service U.S. Department of Agriculture and the U.S. Census Bureau to assign data to commuting zones—which group counties into regions reflecting local economies in which people live and work (U.S. Department of Agriculture, 2019)—and develop a measure of degree of ruralness. To test my hypotheses, I employ a discontinuous growth modeling approach (Bliese & Lang, 2016), which allows me to (a) understand how the trajectory of e-commerce adoption relates to predictors (Bliese & Ployhart, 2002) and (b) isolate the period prior to COVID-19 and following the onset of the pandemic to understand adoption patterns in each time period (Bliese & Lang, 2016). Overall, I find that there is some evidence that urban consumers adopt e-commerce at a faster rate than rural consumers; this is especially evident as it relates to the jump in adoption exhibited at the onset of COVID-19 as urban consumers demonstrated a larger increase in e-commerce adoption in 2020. Additionally, commuting zones that had a lower level of adoption pre-COVID (2019) exhibited the greatest increase in adoption in 2020.

This research makes several theoretical and managerial contributions. From a theoretical perspective, I firstly contribute to the literature which explores the spatial determinants of e-commerce adoption that focuses on differences in adoption based on whether consumers are in an urban or rural area. Given the existing conflicting findings in this literature (e.g., Beckers et al., 2021; Cao et al., 2013; Kirby-Hawkins et al., 2019; Zhou & Wang, 2014), I reconcile these findings by taking a different empirical approach and building upon the theoretical foundation by focusing on benefits of e-commerce for different consumers based on the four stages of shopping (Gauri et al., 2021), ultimately establishing urban versus rural as a boundary condition (Makadok et al., 2018) of e-commerce adoption. Second, I contribute by taking a longitudinal perspective by looking at how e-commerce adoption before and after the onset of COVID-19—an important event altering consumer behavior (e.g., Eger et al., 2021; Guthrie et al., 2021; Sheth, 2020)—related and how trajectories in both periods were moderated by the urban-rural delineation. Lastly, this research contributes to the consumer-centric SCM literature (Esper et al., 2020) by generating insights for retailers and last mile providers that are informed by consumer behavior and preferences, with a focus on how consumers behave heterogeneously (Esper & Peinkofer, 2017) across urban versus rural areas to inform supply chain strategies.

From a managerial perspective, this research contributes by informing both retailers and last mile delivery providers regarding the pattern of e-commerce adoption. First, given that there is some evidence that urban consumers were adopting e-commerce at a quicker rate from 2010-2019 and especially exhibited the largest jump in 2020, retailers and last mile delivery providers should focus on e-commerce expansion in urban areas, while retailers should consider expanding their brick-and-mortar offerings in rural areas. Second, given the challenges to serve urban areas from a last mile perspective—such as high real estate costs (Gibson et al., 2018) and congestion (Rose et al., 2016)—and the strength of adoption in these areas, service providers should focus innovation efforts at addressing such challenges moving forward. In particular, challenges can be addressed by building out parcel delivery lockers (Ranjbari et al., 2023) and/or micro fulfillment from urban retail stores (Young, 2022), for example. Lastly, given a negative relationship between the level of e-commerce adoption in 2019 and the increase in adoption exhibited at the onset of COVID-19 (2020), service providers should be cognizant that in the face of future increases in demand for services, previous patterns of adoption may not necessarily hold.

This essay is organized as follows. The next section reviews the relevant literature, while the following reviews the theoretical perspective and develops hypotheses. Next, I describe the research design and econometric approach, followed by results, robustness tests, and post-hoc analysis. Lastly, I provide both theoretical and managerial contributions, as well as limitations and directions for future research.

2.2 LITERATURE REVIEW

There are two areas of literature to which this research contributes. First, this study contributes to the general literature regarding consumer-centric SCM. As defined by Esper et al. (2020), consumer-centric SCM is centered on recognizing the importance of prioritizing consumer behavior and preferences when designing supply chain services—while not abandoning a focus on efficiency—as well as understanding how SCM-related activities can impact the behavior and experiences of consumers. The value of this perspective has grown in recent years with the rise of the Internet (Esper et al., 2020) and heightened consumer demands (Daugherty et al., 2019). The focus in the literature on consumer issues in SCM has accordingly grown in recent years (Esper & Peinkofer, 2017)—with a focus on how consumers interact with issues and topics such as stockouts (e.g., Jin et al., 2023; Peinkofer et al., 2016, 2022), last mile delivery (e.g., Nguyen et al., 2019; Ta et al., 2018; Tokar et al., 2020), and omnichannel fulfillment options (e.g., Akturk et al., 2018)—and there have been recent calls for additional consumer-centric SCM research (Esper et al., 2020; Esper & Peinkofer, 2017). The current study responds to this call and contributes by exploring consumer behavior as it relates to e-commerce adoption, with a focus on changes in adoption over time and how the changes are heterogeneous for consumers in urban versus rural areas. Thus, this research broadens the context (Makadok et al., 2018) of the consumer-centric SCM literature and responds to a call to understand how to tailor SCM services to different consumer segments (Esper & Peinkofer, 2017).

More specifically, this research also contributes to the empirical literature that explores the spatial determinants of consumers' online shopping adoption, particularly that which focuses on whether consumers are from a rural or urban area and how that relates to their adoption. Key studies are summarized in Table 2.1. From a conceptual perspective, this literature dates back to Anderson et al. (2003) who proposed two competing hypotheses. The first is the innovation-diffusion perspective (e.g., Farag et al., 2006b), whereby it is expected that urban consumers will be more likely than rural consumers to adopt e-commerce based partly on education levels and the use of the Internet for other purposes (Anderson et al., 2003). The second is the efficiency perspective (e.g., Farag et al., 2006b), whereby it is expected that rural consumers will be more likely than urban consumers to adopt e-commerce based on rural consumers having “the most to gain” from accessing the variety of goods available online (Anderson et al., 2003, p. 421). Since this conceptualization, mixed conclusions have been reached in the empirical literature, with studies finding support for both the innovation diffusion perspective (e.g., Beckers et al., 2021; Cao et al., 2013; Farag et al., 2006a; Zhou & Wang, 2014) and the efficiency perspective (e.g., Cao et al., 2013; Farag et al., 2003; Kirby-Hawkins et al., 2019). The present study aims to extend the literature by reconciling the mixed findings by addressing three key features of the extant literature.

Table 2.1 Empirical Literature Exploring the Impact of Rural-Urban on Online Shopping Adoption

Citation	Online shopping data type	Perspective	Sample	Measure of online shopping	Measure of urban/rural	Findings regarding adoption in urban vs. rural areas	Hypothesis supported
Farag et al. (2003)	Survey	Cross-sectional	Consumers in the Netherlands	Whether the consumer has shopped online	5 categories based on location in relation to urban area at focus	Consumers in less urbanized areas shopped online more than those in more urban areas.	Efficiency
Krizek et al. (2005)	Survey	Cross-sectional	Consumers in three metro areas in the U.S.	Has shopped online versus not	Urban, suburban	Suburban consumers were more likely to shop online than urban consumers.	Efficiency
Farag et al. (2006a)	Survey	Cross-sectional	Consumers in the U.S. (Minneapolis) and the Netherlands (Utrecht)	Online shopping frequency; have shopped online before vs. have not	No specific measure; evaluates distance to shops	Dutch consumers who live farther from stores are less likely to shop online, indicating some positive relationship between urban-ness and online shopping.	Innovation-diffusion
Farag et al. (2006b)	Survey	Cross-sectional	Consumers in the Netherlands	Online shopping frequency; have shopped online before vs. have not	5 categories based on number of addresses in a square kilometer	While online shopping is largely an urban phenomena and people living in heavily urban areas are more likely to shop online, consumers with low accessibility to stores shop online more often.	Both
Cao et al. (2013)	Survey	Cross-sectional	High-income consumers in the U.S. (Minneapolis)	Frequency of purchasing 'nondaily' products online	Urban, suburban, exurban	Support for both sides: consumers in urban areas and with greater accessibility to shopping shop online more frequently. Also, consumers in exurban areas with lower shopping accessibility shop online more than consumers in exurban areas with greater accessibility.	Both
Zhou and Wang (2014)	Survey	Cross-sectional	Consumers in the U.S.	Propensity to shop online	Urban or non-urban	Consumers in urban areas are more likely to shop online.	Innovation-diffusion
Clarke et al. (2015)	Survey	Cross-sectional (survey is longitudinal but not analyzed as such)	Grocery consumers in the UK	Online shopping frequency	Classified urban, rural town/fringe, rural village based on postcode	Mixed results; no concrete conclusions.	Neither

Table 2.1 (cont'd)

Motte-Baumvol et al. (2017)	Survey and interviews	Cross-sectional	Consumers in France	Online shopping frequency; have shopped online before vs. have not	City, inner suburb, outer suburb	Suburban consumers were more likely to shop online and also shopped online more often. Consumers in smaller urban areas are more likely to shop online than those in larger urban areas.	Efficiency
Beckers et al. (2018)	Survey	Cross-sectional	Consumers in Belgium	Online shopping frequency	Classified urban, suburban, or rural based on zip code	No significant relation between the area type and online shopping frequency.	Neither
Zhen et al. (2018)	Survey	Cross-sectional	Consumers in Nanjing, China	Online shopping frequency for clothing and books	4 categories based on living and working locations in urban/suburban	Consumers that work and live in suburban areas were more likely to shop at stores than online.	Both
Kirby-Hawkins et al. (2019)	Sales data from a grocery retailer	Cross-sectional	Consumers in the UK	Sales (only online sales figures are included)	Population density within postal sector	Support for both sides: a higher percentage of online shoppers are from rural than urban areas, but online shopping is very popular among young urban professionals.	Both
Shi et al. (2019)	Interviews	Cross-sectional	Consumers in Chengdu, China	Online shopping frequency and share	3 categories (urban, suburban, exurban) based on work or home location	Consumers in urban areas purchase online more frequently, while those in both urban and exurban areas have higher shares of shopping online than suburban consumers.	Both
Beckers et al. (2021)	Survey	Cross-sectional	Consumers in Belgium	Monthly online shopping frequency pre-pandemic and during	Urban, suburban, rural based on zip code	Urban consumers shop online more frequently.	Innovation-diffusion
Song (2022)	Data from online retailer Alibaba	Cross-sectional	Counties in China	Online shopping index (represents online shopping development level in the county)	Ratio of urbanization	Consumers in urban areas shop online more than those in rural areas.	Innovation-diffusion
Young et al. (2022)	Survey	Longitudinal	Consumers in 11 cities in the US	Frequency of online shopping	Urban, suburban, rural/small town	Prior to the pandemic (fall 2019), urban consumers shopped online more than rural consumers. After COVID, suburban consumers were more likely to shop online.	Both

Table 2.1 (cont'd)

Wieland (2023)	Survey	Cross-sectional	Consumers in Germany	Frequency of online shopping	Urban area as defined by German classifications (large city)	More urban consumers shopped for furniture online, but there was no difference in urban versus rural consumers for other product categories (groceries, clothing, electronics). Accessibility of stores was also negatively related to shopping online for furniture but not significant for other categories.	Both

First, the literature exploring the impact of consumers' location as urban or rural on online shopping adoption has primarily relied on survey and/or interview data, with some exceptions including Kirby-Hawkins et al. (2019) and Song (2022). Thus, the measure of online shopping behavior has been retrospective and perceptual. For example, Beckers et al. (2018) asked respondents to select from seven categories to indicate online shopping frequency: never, less than once per year, every 6-12 months, every 3-6 months, every 1-3 months, monthly, and weekly. Additionally, Clarke et al. (2015) examined online shopping frequency with four categories: often, sometimes, rarely, and never. Measures with options such as 'often' or 'sometimes' are left up to interpretation by the respondent (Bocklisch et al., 2012). Further, retrospective measures can be affected by recall bias (Tax et al., 1998), thereby affecting the accuracy of responses and subsequent findings. I accordingly build upon the literature by utilizing transaction data and, consequently, measuring actual consumer behavior more precisely to strengthen the insights in this literature stream.

Second, the research to date has measured whether a consumer resides in an urban or rural area in a way that could be contributing to the mixed conclusions. In particular, urban/rural measures have primarily related to where the responding consumer *lives* (e.g., Beckers et al., 2021; Clarke et al., 2015; Young et al., 2022). As pointed out by Shi et al. (2019), consumers might do most of their shopping in the area in which they work instead of where they live. Consequently, basing the measure on the consumers' area of residence may affect results by not taking into account the overall area in which they might live and work and, thereby, shop. As will be explained, I conduct analyses at a commuting zone level, which helps account for the expectation that consumers may shop outside of their area of residence.

Lastly, the empirical research examining heterogeneous adoption of e-commerce based on the urban/rural divide has primarily taken a cross-sectional perspective (e.g., Cao et al., 2013; Farag et al., 2006a; Song, 2022; Zhen et al., 2018), thereby looking at consumers' shopping behaviors at a point in time. These studies have resulted in valuable insights to understand consumers' e-commerce adoption patterns, but it is important to consider that such patterns can change over time (Miller et al., 2018)—both gradually over time and in response to particular events—which is enabled by the use of longitudinal data (Singer & Willett, 2003; Willett, 1989) in the present study. Further, as recognized by Young et al. (2022), one important event that could shift the adoption patterns was the COVID-19 pandemic. I build upon Young et al. (2022) to further explore how this event changed adoption behavior by employing transaction data to gain a deeper understanding of behavior prior to and following the onset of the pandemic.

2.3 THEORY AND HYPOTHESES

To develop my hypotheses, I draw from the previously mentioned innovation diffusion and efficiency hypotheses developed by Anderson et al. (2003) as well as the customer-centric retail framework and four stages of shopping as presented by Gauri et al. (2021). The four stages of shopping include information search, purchase, acquisition, and returns (Gauri et al., 2021), and I will explain how benefits from online shopping in different stages of the shopping process inform my predictions regarding e-commerce adoption. As shown by the extant literature and its mixed findings regarding e-commerce adoption, especially from the perspective of differences in rural and urban areas (e.g., Cao et al., 2013; Farag et al., 2003; Farag et al., 2006a), there is theoretical ambiguity, and there may be competing mechanisms in play as it relates to the adoption of e-commerce. Accordingly, I derive competing predictions for each hypothesis.

First, I hypothesize regarding the expectation for adoption prior to the COVID-19 pandemic (from 2010-2019). I focus on prior to the pandemic given that COVID-19 drove significant changes in consumer behavior (e.g., Eger et al., 2021; Guthrie et al., 2021; Sheth, 2020), indicating that the predictors of e-commerce adoption could have shifted—temporarily or permanently—following its onset. Further, e-commerce spend in the U.S. overall was growing during this period, as evidenced by data from the U.S. Census Bureau (2023a). I first explain why I expect that consumers in urban areas will adopt e-commerce at a faster rate than those in rural areas. Following the innovation diffusion perspective (e.g., Farag et al., 2006b), urban consumers are expected to be more likely to adopt e-commerce shopping as they are in general more likely to adopt innovations, and this is expected for e-commerce specifically given their higher level of education and higher likelihood of using the Internet for other reasons (Anderson et al., 2003). Additionally, urban consumers tend to be younger and have less time but more monetary resources to devote to shopping (Farag et al., 2006b). These reasons that lead to the expectation of urban consumers being more likely to adopt e-commerce should translate to a quicker rate of adoption in such areas.

In addition to the commonly presented reasons in the literature exploring how consumer location in urban versus rural areas impacts adoption, there are additional reasons to expect that urban consumers will adopt e-commerce at a faster rate than rural consumers. These reasons relate to anticipated greater benefits for urban consumers adopting e-commerce from the perspective of the stages of shopping (Gauri et al., 2021). In the information search stage (Gauri et al., 2021), urban consumers stand to benefit more from e-commerce due to lower ownership of vehicles (LeBeau, 2014). Such lower vehicle access has been shown to reduce the willingness to travel to stores (Ellickson et al., 2020) and the effort that consumers put forth in the search process at retailers (Talukdar, 2008), indicating that search at brick-and-mortar retailers is more burdensome for those consumers without personal vehicles. Second, in the purchase stage (Gauri et al., 2021), urban consumers stand to benefit from e-commerce given that increased congestion in urban areas (Blaine, 1967; Figliozzi, 2011; Rose et al., 2017) has made accessing brick-and-mortar stores more difficult, which leads to the expectation that online shopping will improve urban consumers' shopping experience and ease of access (Gielens et al., 2021; Seiders et al., 2000). Third, in the acquisition stage, urban consumers stand to benefit more from quick delivery, as there is a higher concentration of customers (Rose et al., 2020b) which leads to faster delivery (Gibson et al., 2018). This is supported by findings that faster delivery leads to higher sales (Fisher et al., 2019). Finally, urban consumers benefit in the returns stage given their proximity to stores which enables consumers to conveniently make free returns of online purchases (Akturk & Ketzenberg, 2022), thereby reducing the risk of buying online (Gibson et al., 2018) and leading to the expectation of greater adoption. Based on the innovation-diffusion perspective as well as the benefits of e-commerce along urban consumers' shopping stages, I hypothesize:

H1: Prior to COVID-19, urban areas will adopt e-commerce at a faster rate than rural areas.

As noted, there are conflicting extant findings in the literature and theoretical ambiguity, so I next explore a competing rationale and hypothesis related to why I expect rural consumers will adopt e-commerce at a faster rate than urban consumers. While I expect that the overall *level* of shopping convenience will be greater for urban consumers shopping online than rural consumers—due to, for example, the quicker delivery (Gibson et al., 2018; Rose et al., 2020b) and easier access to make returns (Akturk & Ketzenberg, 2022), as explained in H1—I expect the overall *improvement* in shopping convenience attributed to switching from offline to online shopping will be greater for rural consumers. This may overpower the greater level of shopping convenience and contribute to faster adoption for rural consumers.

This view aligns with the efficiency hypothesis (e.g., Farag et al., 2006b), which argues that rural consumers have more to *gain* than urban consumers—indicating greater improvement potential—from adopting online shopping and accessing the larger variety of goods that are available online as compared to in physical retail locations (Anderson et al., 2003). This is supported by the fact that there are fewer stores within close proximity to rural households (Rhone et al., 2019), which impacts their access convenience (Gielens et al., 2021; Seiders et al., 2000) during the information search and purchase stage (Gauri et al., 2021) and increases the benefits of online shopping for these consumers. This should impact consumers across the stages of shopping, as brick-and-mortar shopping at retailers a distance away from rural consumers increases friction (i.e., inconvenience) (Gauri et al., 2021) experienced in the shopping process. In all, online shopping helps to reduce spatial constraints (Mokhtarian, 2004) that rural shoppers face given their lack of close proximity to stores (Rhone et al., 2019), leading to the expectation that such consumers will be motivated to adopt online shopping. Accordingly, I hypothesize:

H1alt: Prior to COVID-19, rural areas will adopt e-commerce at a faster rate than urban areas.

Next, I explain how I expect e-commerce adoption levels prior to COVID-19 (2019) will relate to an expected jump in adoption following the onset of the pandemic (2020), controlling for the classification of areas as urban or rural. First, consider that the challenges posed by COVID-19, such as store closures (Kapner, 2020; Miller, 2020) and fear of contracting the virus (Mertens et al., 2020), are expected to increase online shopping adoption overall. However, it is unclear how adoption level prior to the pandemic will relate to the jump in adoption. In the first competing perspective, I explain why I would expect that areas which had adopted e-commerce to the greatest degree pre-COVID would experience the largest jump in adoption following the onset. This is based on the idea of search costs, which are present when consumers do not have full information about an alternative—in this case an alternative channel, online—and must be incurred before consumers consider switching (Moshkin & Shachar, 2000). Such costs are likely present for consumers who had not already adopted e-commerce, creating higher costs for them to adopt online shopping following the onset of COVID-19. Such costs are expected to prohibit consumers from undertaking information search (Gauri et al., 2021) and subsequently changing their shopping behaviors, thereby reducing the likelihood that the areas which had adopted e-commerce to a lower degree pre-pandemic would increase to a large degree following its onset. Contrastingly, such search costs would not be present for those already using e-commerce to a greater degree, leading to the expectation of continued adoption in areas that already had greater e-commerce adoption in order to decrease the new inconveniences associated with brick-and-mortar shopping. Accordingly, I hypothesize:

H2: The geographic areas that have the highest levels of e-commerce adoption pre-COVID will experience the largest percentage increase in adoption after the onset.

Next and alternatively, I explain why I would instead expect that the geographic areas that had adopted e-commerce to a lesser degree prior to COVID-19 would experience the largest jump in adoption after the onset. The reasoning for this rests on the idea that areas that had adopted e-commerce to a lesser degree prior to the pandemic should have greater scope for adoption (Hansman et al., 2020). Greater adoption scope indicates that such areas have more opportunities to increase their adoption than areas which have adopted e-commerce to a greater degree already. Similar to the development of H1alt, consumers in such areas thus have more opportunities for improvement across the stages of shopping (Gauri et al., 2021) from adopting e-commerce than consumers that had already adopted e-commerce to a greater degree. Therefore, I hypothesize:

H2alt: The geographic areas that have the lowest levels of e-commerce adoption pre-COVID will experience the largest percentage increase in adoption after the onset.

Lastly, in the final two competing hypotheses, I explore whether urban or rural areas will display the greatest jump in adoption of e-commerce shopping following the onset of COVID-19. Both arguments are centered around an impact on consumers' access convenience (i.e., the ease at which consumers reach a retailer) (Seiders et al., 2000) to brick-and-mortar retail locations in the purchase stage (Gauri et al., 2021). First, I consider the perspective that urban areas will experience a larger increase in adoption of e-commerce following the onset than urban areas. As compared to rural populations, those who reside in urban areas tended to be more concerned about contracting COVID-19 (Chauhan et al., 2021) and were more in favor of taking measures such as wearing masks (Callaghan et al., 2021), closing businesses, and staying home to reduce the spread of the virus (Chauhan et al., 2021). Based on their concerns and perspectives regarding the virus and mitigation measures, I expect that this will negatively impact the access convenience (Seiders et al., 2000) for those consumers in terms of reaching stores, as it will feel largely more inconvenient for such consumers to visit the brick-and-mortar location. Accordingly, I hypothesize:

H3: Urban areas will experience the largest percentage increase in adoption of e-commerce after the onset of the COVID-19 pandemic.

Finally, I consider why rural areas would experience a greater jump in adoption following the onset of COVID-19 as compared to urban areas. This again rests on the idea that there are fewer stores in close proximity to rural households (Rhone et al., 2019). Adding to this, given the prominence of stockouts (Cannon, 2020; Taylor et al., 2020) and store closures during the pandemic (Kapner, 2020; Miller, 2020), the impact of distance to stores for consumers in rural areas will be exacerbated in that it will be even more difficult to meet their shopping needs with the few stores that are available to them as compared to 'normal' non-pandemic times. Thus, rural consumers' access convenience (Seiders et al., 2000) will be heavily impacted by the onset of the pandemic. It is unclear which group of consumers' access convenience, however, will be impacted to a greater degree and whether the fear of the virus for urban consumers (Chauhan et al., 2021) or lacking brick-and-mortar proximity for rural consumers (Rhone et al., 2019) will have a larger impact on access convenience. Summarizing the alternative prediction, I hypothesize:

H3alt: Rural areas will experience the largest percentage increase in adoption of e-commerce after the onset of the COVID-19 pandemic.

2.4 RESEARCH DESIGN

2.4.1 Sample

To test my hypotheses, I rely on annual data from 2010 through 2020 for households in the U.S. 2010 is chosen as the starting year to allow for an adequate period of time prior to the onset of COVID-19 to understand trends in adoption without interference from other disruptions such as the Great Recession from late 2007 through mid 2009 (Rich, 2013). 2020 is chosen as the final year given that this allows me to address my research questions and based on data availability at the time of this research. To source transaction data, I utilize the NielsenIQ Consumer Panel Dataset, which has been employed in recent research in supply chain and operations management such as Lim et al. (2021), Pan et al. (2023), and Sodero (2022). This dataset from a panel of around 60,000 U.S. households provides data for each shopping trip made by a household (e.g., retail channel type, products purchased, date, total spent, etc.) as well as household characteristics (e.g., county of residence, demographics, etc.). While the transaction data is available at a trip by household level, to ensure adequate coverage and address issues that could occur by assigning a measure of urban-rural at a residence location level (e.g., based on the household's zip code)—which might not adequately reflect the surrounding region in which consumers shop (Shi et al., 2019)—I group the data at a commuting zone by year level.

Commuting zones group the more than 3,000 counties in the U.S. into 741 regions to depict local economies which encompass areas where people live and work (U.S. Department of Agriculture, 2019). Thus, it is likely that consumers that live within a given commuting zone will also do their shopping within that zone, indicating that assigning a representation of the urban-rural makeup of that zone is an accurate reflection of a household's shopping environment. I thus link households to a commuting zone based on each household's reported county of residence. For the primary analysis, I remove any commuting zones that do not have at least six out of eleven years of data given that at least three waves of data are needed to study change over time (Willett, 1989) and that zones with records in less than half of the years of the study are unlikely to have many households contributing information in those years that *do* have data, making it unlikely that the data provided is representative of the population within the zone. Further, during initial data analysis (Chatfield, 1985, 2002) one commuting zone by year observation was removed as it was identified as a clear outlier (over 90% of trips were indicated as in the online channel). This results in an unbalanced panel sample of 7,230 commuting zone by year observations across 662 commuting zones.[1]

[1] Before removing commuting zones which contributed less than 6 observations, the sample consisted of 7,264 observations across 677 commuting zones.

2.4.2 Variables

2.4.2.1 Dependent Variable

I will utilize i to index each commuting zone and t to index the measurement occasion (year). My dependent variable is a commuting zone's share of total trips that were conducted via the online channel in a given year. I retrieve each commuting zone's share of online trips from NielsenIQ's Consumer Panel Dataset by linking each household's county to the respective commuting zone. From there, I pool all shopping trips within a commuting zone and calculate the proportion of trips in the online channel as follows:

$$ShareTripsOnline_{it} = \frac{Count\ of\ Trips\ Online_{it}}{Total\ Trips_{it}} \qquad (1)$$

After calculating the percent of trips online, I add 0.005 to each proportion—as some proportions are 0—then employ a logit transformation following Smithson and Verkuilen (2006) as follows:

$$LogitShareTripsOnline_{it} = Ln\left(\frac{ShareTripsOnline_{it}}{1 - ShareTripsOnline_{it}}\right) \qquad (2)$$

The logit transformation is applied to ensure results are within the 0-1 bounds as the dependent variable is a proportion (Smithson & Verkuilen, 2006). Thus, *LogitShareTripsOnline* becomes the dependent variable.

2.4.2.2 Independent Variables

The primary predictor of interest is the passage of time. I split this into two variables to model the slope pre-COVID and then the jump in adoption in 2020. The first predictor is labeled *Time* and indicates the rate of adoption of e-commerce from 2010-2019. In order to test H2, as will be explained shortly, the *Time* variable is equal to 0 in 2019 and 2020 and decreases by 1 unit each year prior to 2019. The second predictor is labeled *COVID*. This is a dummy variable which indicates that the observation is from the year in which the pandemic hit (i.e., it is equal to 1 in

2020 and 0 otherwise). These variables are assigned to commuting zone by year observations as shown in Table 2.2.

Table 2.2 Design Matrix for Time and COVID Variables

Year	Intercept	Time	COVID
2010	1	–9	0
2011	1	–8	0
2012	1	–7	0
2013	1	–6	0
2014	1	–5	0
2015	1	–4	0
2016	1	–3	0
2017	1	–2	0
2018	1	–1	0
2019	1	0	0
2020	1	0	1

The last predictor is a moderating variable denoted $\overline{Rural}$. For the primary analysis, this is a continuous variable which represents the degree of ruralness for the commuting zone. It is sourced from the Economic Research Service, U.S. Department of Agriculture, which assigns Rural-Urban Continuum codes ranging from 1 (most urban) to 9 (most rural) (U.S. Department of Agriculture, 2019). Thus, higher values indicate that an area is more rural. Codes are available at a county-level. Given that each commuting zone consists of several counties (U.S. Department of Agriculture, 2019), for each commuting zone I utilize annual population data from the U.S. Census Bureau to assign a weighted value of the rural-urban continuum for each year. Next, I take the average rural-urban continuum for each commuting zone across the study timeframe to ensure that effects are signifying pure between-subject (between commuting zone) estimates (Certo et al., 2017). Lastly, $\overline{Rural}$ is grand mean centered to improve interpretability.

2.4.2.3 Control Variable

Given the nature of the predictors of $Time$, $COVID$, and $\overline{Rural}$—which are all exogenous and unlikely to be impacted by upstream predictors which would need to be controlled for (Cinelli et al., 2022; Miller & Kulpa, 2022)—concerns regarding endogeneity related to omitted variables are reduced (Ketokivi & McIntosh, 2017). However, to decrease the standard errors of the estimates (Cohen et al., 2003), I control for one important factor—the presence of an Amazon fulfillment center. Given Amazon's dominance in online retailing (Droesch, 2021) and its increasing presence across the U.S. (Daleo, 2022), it is expected that its presence will impact consumers' e-commerce adoption. I control for the presence of an Amazon fulfillment center specifically because, based on previous research, the presence of a fulfillment center reduces delivery time, subsequently increasing online shopping volume (Fisher et al., 2019). Data for this control is sourced from MWPVL International, which provides information such as addresses and opening dates of Amazon facilities (MWPVL International Inc., 2023) and has been employed in prior research (e.g., Ahmed et al., 2022; Rodrigue, 2020). For each commuting zone by year observation, this control—denoted *Amazon*—equals 1 if the zone had an Amazon fulfillment center present during that year and 0 otherwise.[2]

2.4.2.4 Descriptive Statistics and Plots

Descriptive statistics for the key variables are reported in Table 2.3. Further, to provide model-free evidence (Davis-Sramek et al., 2023) in support of the general upward trend over time in e-commerce adoption and a jump in adoption in 2020, Figures 2.1, 2.2, and 2.3 provide plots of the share of shopping trips in the online channel for three groups of commuting zones: (1) two

[2] Even if the fulfillment center was opened in the fourth quarter of the given year, the observation is assigned a 1 for *Amazon* given the seasonality of retail sales, with sales being heavily concentrated in the final months of the year (U.S. Census Bureau, 2023b).

large commuting zones, (2) eight randomly selected commuting zones, and (3) one strongly urban and one strongly rural commuting zone. In addition to showing the expected upward trend and jump, these figures provide additional insights justifying the study. Figure 2.2, for example, shows that there is heterogeneity in adoption patterns across zones, while Figure 2.3 shows differences between urban and rural areas as predicted.

Table 2.3 Descriptive Statistics

Variable	Mean	Standard deviation	Minimum	Maximum
$Share\ of\ trips\ online$	2.401%	2.671%	0%	52.381%
$\overline{Rural}$	4.757	2.185	1	9

Notes: Descriptive statistics for share of trips online are reported before the logit link transformation. Descriptive statistics for $\overline{Rural}$ are reported before grand mean centering. N = 7,230.

Figure 2.1 Share of Trips Per Year Online for 3 Large Commuting Zones

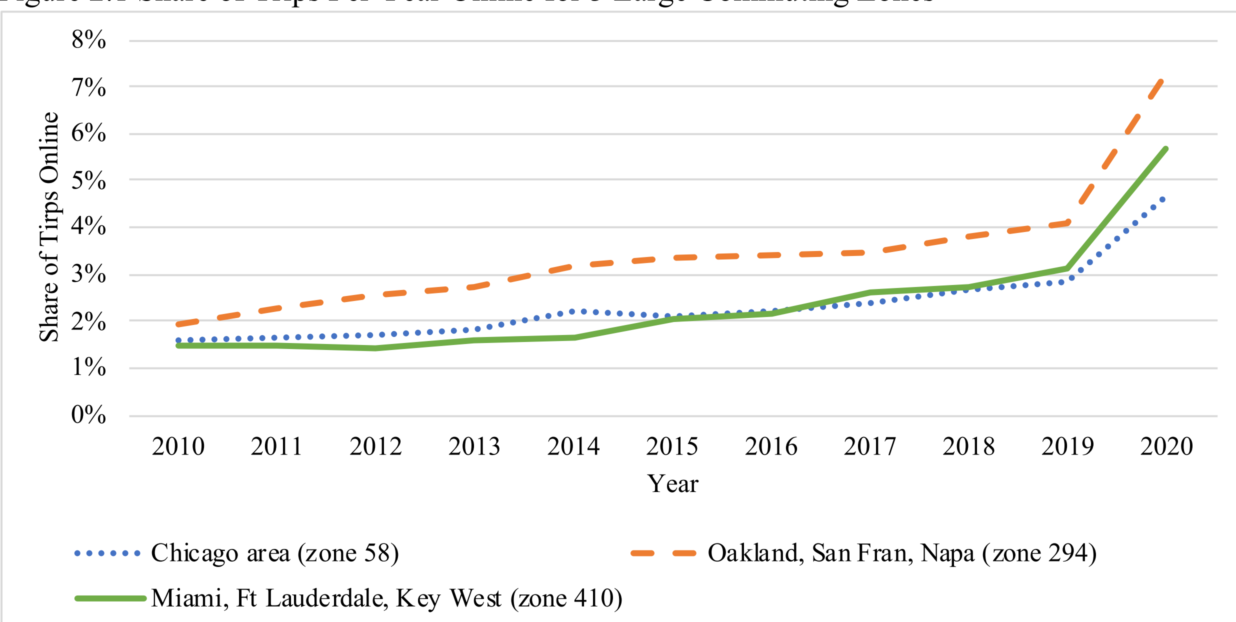


Figure 2.2 Share of Trips Per Year Online for 8 Randomly Selected Commuting Zones

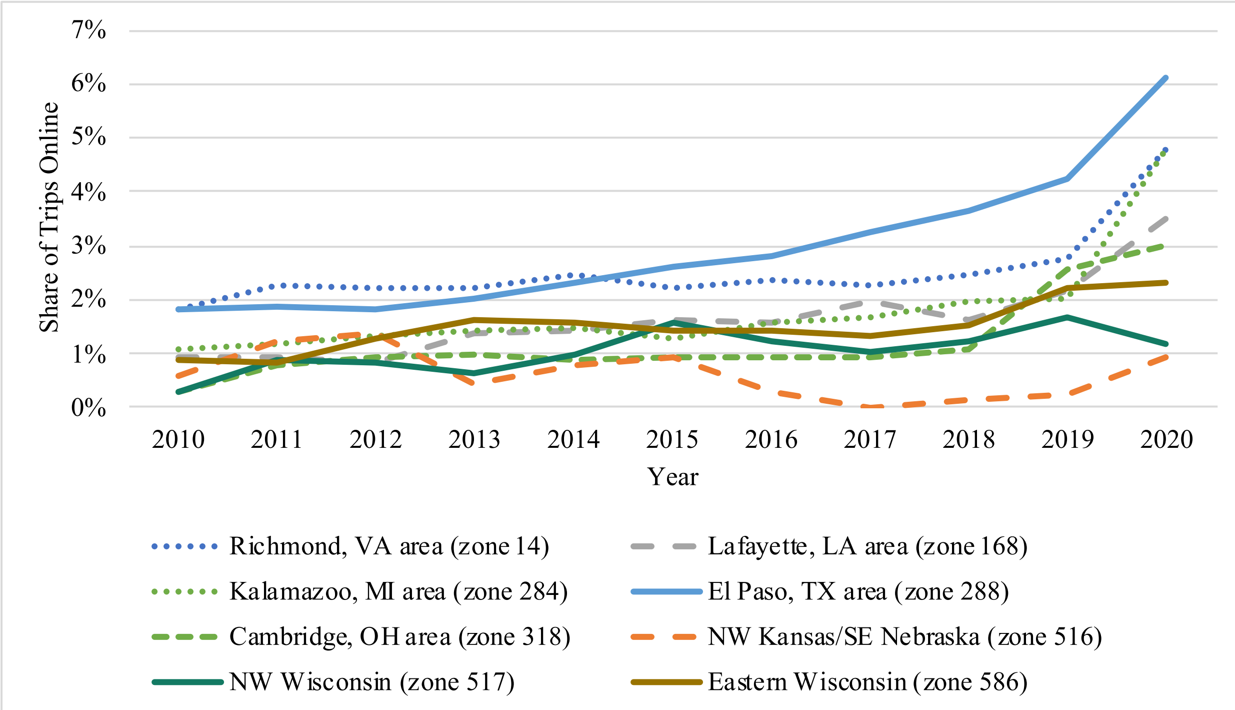
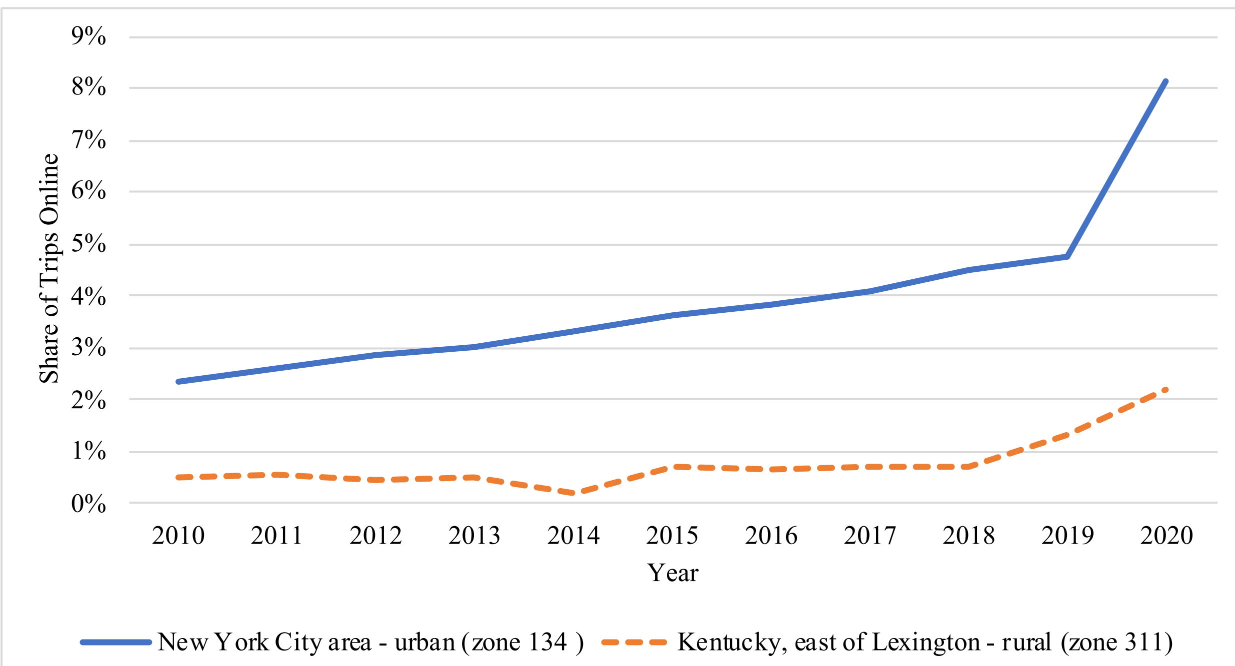


Figure 2.3 Share of Trips Per Year Online for 1 Strongly Urban and 1 Strongly Rural Commuting Zone



2.5 ANALYSIS AND RESULTS

2.5.1 Modeling Approach

To test my hypotheses, I implement a series of discontinuous growth models (Bliese & Lang, 2016). While growth models enable an examination of whether there are changes over time and differences among commuting zones in the pattern of change, discontinuous growth models build upon such models to capture a discontinuity which affects the trajectory of change (Bliese & Lang, 2016). Accordingly, employing discontinuous growth models allows me to (a) understand how the trajectory of e-commerce adoption relates to predictors (Bliese & Ployhart, 2002) and (b) isolate the period prior to COVID-19 and following the onset of the pandemic to understand adoption patterns in each time period (Bliese & Lang, 2016). As H1, H1alt, H3, and H3alt require a more complex model in which H2 and H2alt are nested, I first present the model to test H2 and H2alt. Following notation from Singer and Willett (2003), I specify the below model to test H2 and H2alt:

$$LogitShareTripsOnline_{it}$$

$$= \pi_{0i} + \pi_{1i}TIME_{it} + \pi_{2i}COVID_{it} + \pi_{3i}Amazon_{it} + \pi_{4i}\overline{RURAL}_i + \epsilon_{it} \quad (3)$$

$$\pi_{0i} = \gamma_{00} + \xi_{0i} \quad (4)$$

$$\pi_{1i} = \gamma_{10} + \xi_{1i} \quad (5)$$

$$\pi_{2i} = \gamma_{20} + \xi_{2i} \quad (6)$$

$$\pi_{3i} = \gamma_{30} \quad (7)$$

$$\pi_{4i} = \gamma_{40} \quad (8)$$

The level-1 model (Equation 3) represents the change process for commuting zones, including both a slope for 2010-2019 (Time) and a discontinuity in 2020 (COVID) (Bliese & Lang, 2016). It also includes controls for the effects of *Amazon* and $\overline{Rural}$. The first three components of the level-2 model (Equations 4-6) allow for differences across commuting zones in the level-1 parameters by incorporating random effects (ξ_{0i}, ξ_{1i}, and ξ_{2i}), while Equations 7 and 8 specify that the effects of *Amazon* and $\overline{Rural}$ do not vary by commuting zone, thereby entering only as fixed effects (Bliese & Lang, 2016). Substituting level-1 into level-2, I develop the below composite model to test H2 and H2alt:

$$\begin{aligned} &LogitShareTripsOnline_{it} \\ &= [\gamma_{00} + \gamma_{10}TIME_{it} + \gamma_{20}COVID_{it} + \gamma_{30}Amazon_{it} + \gamma_{40}\overline{RURAL}_i] \\ &+ [\xi_{0i} + \xi_{1i}TIME_{it} + \xi_{2i}COVID_{it} + \epsilon_{it}] \end{aligned} \tag{9}$$

I allow the random effects to covary, and the corresponding variance covariance matrix for Equation 9 is reported in Equation 10. Equation 10 indicates that the residuals from the level-2 model are normally distributed with a mean of 0 (Singer & Willett, 2003). Further, the variances are along the diagonal, with covariances on the off-diagonals (Singer & Willett, 2003). Given that *Time* is equal to 0 in 2019 (see Table 2.2), I am able to test H2 and H2alt by examining the covariance between the random effects for the intercept (ξ_{0i})—which represents the level of adoption in 2019—and COVID (ξ_{2i}). H2 predicts σ_{02} will be positive and significant, while H2alt predicts that σ_{02} will be negative and significant.

$$\begin{bmatrix} \xi_{0i} \\ \xi_{1i} \\ \xi_{2i} \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_0^2 & & \\ \sigma_{01} & \sigma_1^2 & \\ \sigma_{02} & \sigma_{12} & \sigma_2^2 \end{bmatrix} \right) \tag{10}$$

Next, I present the model utilized to test H1, H1alt, H3, and H3alt, which incorporates interactions between *Time* and $\overline{Rural}$ as well as *COVID* and $\overline{Rural}$, allowing the slope and discontinuity to change based on a commuting zone's degree of ruralness (Singer & Willett, 2003). The level-1 model is as follows:

$$LogitShareTripsOnline_{it} = \pi_{0i} + \pi_{1i}TIME_{it} + \pi_{2i}COVID_{it} + \pi_{3i}Amazon_{it} + \pi_{4i}\overline{RURAL}_i + \pi_{5i}(TIME_{it} \times \overline{RURAL}_i) + \pi_{6i}(COVID_{it} \times \overline{RURAL}_i) + \epsilon_{it} \quad (11)$$

The corresponding level-2 model includes Equations 4-8, plus the following fixed components:

$$\pi_{5i} = \gamma_{50} \quad (12)$$

$$\pi_{6i} = \gamma_{60} \quad (13)$$

Accordingly, the composite model is as follows in Equation 14:

$$LogitShareTripsOnline_{it} = [\gamma_{00} + \gamma_{10}TIME_{it} + \gamma_{20}COVID_{it} + \gamma_{30}Amazon_{it} + \gamma_{40}\overline{RURAL}_i + \gamma_{50}(TIME_{it} \times \overline{RURAL}_i) + \gamma_{60}(COVID_{it} \times \overline{RURAL}_i)] + [\xi_{0i} + \xi_{1i}TIME_{it} + \xi_{2i}COVID_{it} + \epsilon_{it}] \quad (14)$$

Given the random effects are consistent with the model in Equation 9, the variance-covariance matrix for Equation 14 is consistent with that in Equation 10. H1 predicts that γ_{50} will be negative and significant, while H1alt predicts a positive and significant γ_{50}. Lastly, H3 predicts that γ_{60} will be negative and significant, while H3alt predicts a positive and significant γ_{60}.

2.5.2 Main Results

I tested my hypotheses via maximum likelihood estimation with robust standard errors utilizing the mixed command in Stata 16.1. Results from the primary analysis are presented in Table 2.4. In Model 1 in Table 2.4 I report results from a random slopes model to calculate the ICC (intraclass correlation coefficient), which is 0.434. This indicates that 43.4% of the variation in the proportion of trips online in the data resides between commuting zones, indicating the relevance of aiming to explain such variation. Next, Model 2 reports results from an unconditional growth model without the fixed effects for *Amazon* and $\overline{Rural}$. Translating coefficients to percent form via Allison (2012), the coefficient for *Time* indicates that with each additional year from 2010-2019, there was a 4.50%[3] increase in the odds of e-commerce adoption, while *COVID* indicates that the onset of COVID-19 increased the odds of adoption by 51.59%.

[3] The coefficient is translated to percent form following Allison (2012) ($100 \times [\exp(0.044) - 1]$).

Table 2.4 Main Results

	Label	Model 1	Model 2	Model 3	Model 4
Fixed effects					
Intercept	γ_{00}	–3.485*** (–158.91)	–3.592*** (–154.32)	–3.597*** (–155.51)	–3.594*** (–157.63)
Time	γ_{10}	0.061*** (19.40)	0.044*** (13.25)	0.044*** (12.92)	0.044*** (13.30)
COVID	γ_{20}		0.416*** (17.90)	0.414*** (17.75)	0.415*** (17.86)
Amazon	γ_{30}			0.051*** (2.95)	0.023 (1.29)
$\overline{\text{Rural}}$	γ_{40}			–0.028*** (–3.07)	–0.034*** (–2.75)
Time x $\overline{\text{Rural}}$	γ_{50}				–0.002 (–1.22)
COVID x $\overline{\text{Rural}}$	γ_{60}				–0.025** (–2.09)
Variance components					
Var. intercept	σ_0^2	0.182 (0.150, 0.220)$^+$	0.301$^+$ (0.253, 0.359)	0.294$^+$ (0.245, 0.353)	0.295$^+$ (0.245, 0.354)
Var. time	σ_1^2		0.005$^+$ (0.004, 0.007)	0.005$^+$ (0.004, 0.007)	0.005$^+$ (0.004, 0.007)
Var. COVID	σ_2^2		0.135$^+$ (0.072, 0.253)	0.135$^+$ (0.072, 0.253)	0.133$^+$ (0.069, 0.253)
Cov. time, COVID	σ_{12}		–0.005 (–0.011, 0.001)	–0.005 (–0.011, 0.001)	–0.005 (–0.011, 0.001)
Cov. time, intercept	σ_{01}		0.024$^+$ (0.0170.031)	0.024$^+$ (0.017, 0.030)	0.024$^+$ (0.017, 0.030)
Cov. COVID, intercept	σ_{02}		–0.063$^+$ (–0.103, –0.023)	–0.067$^+$ (–0.107, –0.027)	–0.068$^+$ (–0.107, –0.029)
Var. residual	σ_ϵ^2	0.237 (0.212, 0.266)$^+$	0.163$^+$ (0.142, 0.188)	0.163$^+$ (0.142, 0.188)	0.163$^+$ (0.142, 0.188)
Model fit					
–2 log likelihood		11,598.249	10,390.587	10,374.614	10,364.704
Sample					
Observations		7,230	7,230	7,230	7,230
Number of groups		662	662	662	662

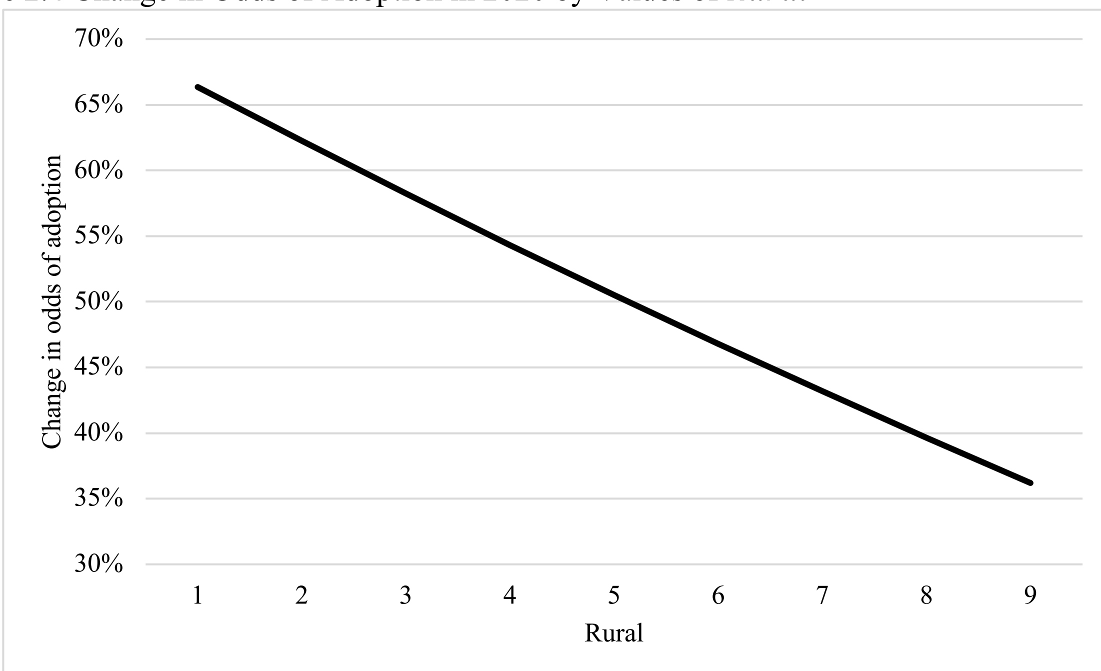
Notes: Outcome: $LogitShareTripsOnline_{jt}$. Models were analyzed with robust standard errors. z-statistics are reported in parentheses for fixed effects; *p < .10, **p < .05, ***p < .01. 95% confidence intervals are reported in parentheses for variance components; $^+$ indicates a significant variance or covariance.

Model 3 presents results from Equation 9 to test H2 and H2alt, which predicted correlation between the intercept and *COVID* (i.e., between the pre-COVID level of e-commerce adoption and the jump in adoption at the onset of COVID-19). The covariance between the random intercept and *COVID* is negative and significant (σ_{02} = –0.067, CI = [–0.107, –0.027]). This indicates that, conditional on *Amazon* and $\overline{Rural}$, there is a significant negative correlation between the level of adoption of e-commerce in 2019 and the jump in adoption in 2020; the commuting zones that had lower levels of adoption in 2019 experienced a greater jump in adoption in 2020. Converting the covariance, the magnitude of the relationship is a correlation of –0.34. Accordingly, *H2alt is supported, while H2 is not supported*.

Model 4 reports results from Equation 14, incorporating the interactions needed to test H1, H1alt, H3, and H3alt. Considering H1 and H1alt, which predicted that the rate of adoption prior to COVID-19 would be moderated by $\overline{Rural}$, the coefficient for the interaction between *Time* and $\overline{Rural}$ is not significant (γ_{50} = –0.002, z-statistic = –1.22, p > 0.10). This indicates that from 2010-2019, the rate of adoption of e-commerce was not moderated by $\overline{Rural}$ (i.e., the rate of adoption was not significantly different based on to what degree a commuting zone was considered urban or rural). Thus, *neither H1 nor H1alt are supported*.

Lastly considering H3 and H3alt, which predicted that the degree of increase in adoption at the onset of COVID-19 in 2020 would be moderated by $\overline{Rural}$, in Model 4 (see Table 2.4) the coefficient for the interaction between *COVID* and $\overline{Rural}$ is negative and significant (γ_{60} = –0.025, z-statistic = –2.09, p < 0.05). This indicates that urban zones experienced a larger percentage increase in adoption of e-commerce than rural zones following the onset of the COVID-19 pandemic. Specifically, the increase in the odds of adoption of e-commerce at the onset of COVID-19 decreased by 2.47% (Allison, 2012) per one-unit increase in $\overline{Rural}$ (i.e., a one-unit increase on the rural scale of 1-9). Thus, *H3 is supported, while H3alt is not supported*. This effect is plotted in Figure 2.4, which shows that at the minimum of $\overline{Rural}$ (i.e., in the most urban commuting zones), the increase in odds of adoption of e-commerce in 2020 was 66.35%, while at the maximum of $\overline{Rural}$ (i.e., in the most rural commuting zones), the increase in odds of adoption of e-commerce in 2020 was 36.20%.[4]

Figure 2.4 Change in Odds of Adoption in 2020 by Values of $\overline{Rural}$



2.6 ROBUSTNESS TESTING

2.6.1 Alternate Dependent Variable

I next report results from two robustness tests. In the first, I utilize an alternate dependent variable. Instead of focusing on the proportion of shopping trips that are conducted in the online channel as the dependent variable, I focus on the proportion of dollars spent in the online channel. Similar to the primary analysis, I pool all dollars spent within a commuting zone in a year and calculate the proportion of spend in the online channel as follows:

[4] Unlike in the analysis, the values of $\overline{Rural}$ in Figure 2.4 are not mean-centered.

$$ShareSpendOnline_{it} = \frac{Dollars\ Spent\ Online_{it}}{Total\ Dollars\ Spent_{it}} \tag{15}$$

After calculating the percent of spend online, I add 0.005 to each proportion then employ a logit transformation following Smithson and Verkuilen (2006) as follows:

$$LogitShareSpendOnline_{it} = Ln\left(\frac{ShareSpendOnline_{it}}{1 - ShareSpendOnline_{it}}\right) \tag{16}$$

Results are reported in Table 2.5. Conclusions generally align with that in the primary analysis. As it relates to H2 and H2alt, Model 3 reports a significant negative covariance between the random intercept and *COVID* (σ_{02} = –0.068, CI: [–0.117, –0.020]), indicating that *H2alt is supported, while H2 is not supported*. Next, Model 4 reports a nonsignificant interaction between *Time* and $\overline{Rural}$ (γ_{50} = –0.002, z-statistic = –1.06, p > 0.10), indicating a *lack of support for both H1 and H1alt*. Lastly considering H3 and H3alt, there is a marginally significant interaction between *COVID* and $\overline{Rural}$ in Model 4 (γ_{60} = –0.022, z-statistic = –1.71, p < 0.10). While only marginally significant, this, combined with the statistical significance in the primary analysis—in which the dependent variable aligned more closely to that commonly studied in the literature, frequency of online shopping (e.g., Beckers et al., 2018), and arguably has greater implications for parties such as last mile providers—offers *support in favor of H3 and a lack of support for H3alt*, indicating that urban zones experienced a larger percentage increase in adoption of e-commerce than rural zones following the onset of the COVID-19 pandemic.

Table 2.5 Robustness Test Results: Logit Transformed Proportion of Spend in Online Channel as the Outcome

	Label	Model 1	Model 2	Model 3	Model 4
Fixed effects					
Intercept	γ_{00}	–3.263*** (–131.51)	–3.357*** (–127.13)	–3.362*** (–128.72)	–3.360*** (–130.53)
Time	γ_{10}	0.054*** (14.84)	0.039*** (10.18)	0.039*** (9.89)	0.039*** (10.19)
COVID	γ_{20}		0.367*** (14.76)	0.365*** (14.60)	0.366*** (14.69)
Amazon	γ_{30}			0.059*** (3.02)	0.030 (1.48)
$\overline{\text{Rural}}$	γ_{40}			–0.041*** (–3.84)	–0.046*** (–3.21)
Time x $\overline{\text{Rural}}$	γ_{50}				–0.002 (–1.06)
COVID x $\overline{\text{Rural}}$	γ_{60}				–0.022* (–1.71)
Variance components					
Var. intercept	σ_0^2	0.235^+ (0.193, 0.285)	0.375^+ (0.315, 0.447)	0.363^+ (0.303, 0.435)	0.363^+ (0.303, 0.436)
Var. time	σ_1^2		0.007^+ (0.005, 0.009)	0.007^+ (0.005, 0.009)	0.007^+ (0.005, 0.009)
Var. COVID	σ_2^2		0.077^+ (0.026, 0.232)	0.077^+ (0.026, 0.233)	0.075^+ (0.024, 0.235)
Cov. time, COVID	σ_{12}		–0.003 (–0.010,0.005)	–0.003 (–0.010, 0.005)	–0.003 (–0.010, 0.004)
Cov. time, intercept	σ_{01}		0.029^+ (0.021, 0.038)	0.029^+ (0.020, 0.037)	0.029^+ (0.020, 0.037)
Cov. COVID, intercept	σ_{02}		$–0.063^+$ (–0.112, –0.015)	$–0.068^+$ (–0.117, –0.020)	$–0.069^+$ (–0.117, –0.021)
Var. residual	σ_ϵ^2	0.325^+ (0.290, 0.365)	0.244^+ (0.212, 0.281)	0.244^+ (0.212, 0.281)	0.244^+ (0.212, 0.281)
Model fit					
–2 log likelihood		13,844.773	12,955.978	12,931.460	12,924.680
Sample					
Observations		7,230	7,230	7,230	7,230
Number of groups		662	662	662	662

Notes: Outcome: $LogitShareSpendOnline_{jt}$. Models were analyzed with robust standard errors. z-statistics are reported in parentheses for fixed effects; *p < .10, **p < .05, ***p < .01. 95% confidence intervals are reported in parentheses for variance components; $^+$ indicates a significant variance or covariance.

2.6.2 Altered Criteria for Inclusion in Sample

In the second robustness test, I alter the criteria for inclusion of a commuting zone in the sample. Instead of requiring that a commuting zone has at least 6 observations, I require that the commuting zone has an observation for all of the 11 years of the study period. This is due to the expectation that zones that do not have a record for every year likely have very few households contributing shopping information in the years that *do* have data for the commuting zone, making it unlikely that the data provided is representative of the behavior of the zone's population of consumers. With this restriction, the sample now consists of a balanced panel of 7,051 commuting zone by year observations across 641 commuting zones. Results are reported in Table 2.6.

For H2 and H2alt, aligned with the primary analysis, Model 3 reports a significant negative covariance between the random intercept and *COVID* (σ_{02} = –0.057, CI: [–0.090, –0.025]), *indicating support for H2alt and a lack of support for H2*. Looking next at Model 4, contrary to previous analyses, there is a significant negative interaction between *Time* and $\overline{Rural}$ (γ_{60} = –0.004, z-statistic = –2.12, $p < 0.05$, *which supports H1—thus, not supporting H1alt*—and indicates that urban zones were adopting e-commerce at a quicker rate from 2010-2019 than rural zones. Specifically, the increase in the odds of adoption over time from 2010-2019 decreased by 0.40% (Allison, 2012) for each one-unit increase in $\overline{Rural}$. As it relates to H3 and H3alt, consistent with the primary analysis, Model 4 reports a significant negative interaction between *COVID* and $\overline{Rural}$ (γ_{60} = –0.024, z-statistic = –2.18, $p < 0.05$). The coefficient indicates that the increase in the odds of adoption following the onset of COVID-19 decreased by 2.37% (Allison, 2012) per one-unit increase in $\overline{Rural}$. Accordingly, *H3 is supported, while H3alt is not supported*.

Table 2.6 Robustness Test Results: Removing Commuting Zones with Less than 11 Observations

	Label	Model 1	Model 2	Model 3	Model 4
Fixed effects					
Intercept	γ_{00}	–3.491*** (–160.37)	–3.597*** (–156.67)	–3.603*** (–157.72)	–3.600*** (–159.73)
Time	γ_{10}	0.059*** (19.11)	0.042*** (13.10)	0.042*** (12.76)	0.042*** (13.15)
COVID	γ_{20}		0.414*** (19.44)	0.412*** (19.28)	0.413*** (19.52)
Amazon	γ_{30}			0.055*** (3.28)	0.017 (0.99)
$\overline{\text{Rural}}$	γ_{40}			–0.029*** (–3.11)	–0.041*** (–3.33)
Time × $\overline{\text{Rural}}$	γ_{50}				–0.004** (–2.12)
COVID × $\overline{\text{Rural}}$	γ_{60}				–0.024** (–2.18)
Variance components					
Var. intercept	σ_0^2	0.169^+ (0.140, 0.204)	0.287^+ (0.240, 0.343)	0.279^+ (0.232, 0.336)	0.279^+ (0.231, 0.336)
Var. time	σ_1^2		0.005^+ (0.004, 0.006)	0.005^+ (0.004, 0.006)	0.005^+ (0.004, 0.006)
Var. COVID	σ_2^2		0.094^+ (0.051, 0.174)	0.094^+ (0.051, 0.173)	0.091^+ (0.048, 0.173)
Cov. time, COVID	σ_{12}		–0.005 (–0.010, 0.001)	–0.005 (–0.010, 0.001)	–0.005 (–0.010, 0.000)
Cov. time, intercept	σ_{01}		0.023^+ (0.017, 0.029)	0.022^+ (0.016, 0.029)	0.022^+ (0.016, 0.028)
Cov. COVID, intercept	σ_{02}		-0.054^+ (–0.086, –0.021)	-0.057^+ (–0.090, –0.025)	-0.058^+ (–0.090, –0.026)
Var. residual	σ_ϵ^2	0.212^+ (0.189, 0.237)	0.145^+ (0.126, 0.168)	0.145^+ (0.126, 0.168)	0.145^+ (0.126, 0.168)
Model fit					
–2 log likelihood		10,529.078	9,279.336	9,262.676	9,246.346
Sample					
Observations		7,051	7,051	7,051	7,051
Number of groups		641	641	641	641

Notes: Outcome: $LogitShareTripsOnline_{jt}$. Models were analyzed with robust standard errors. z-statistics are reported in parentheses for fixed effects; *p < .10, **p < .05, ***p < .01. 95% confidence intervals are reported in parentheses for variance components; $^+$ indicates a significant variance or covariance.

2.7 POST HOC ANALYSIS

Given that results regarding H1 in the second robustness test—which dropped all commuting zones with less than 11 observations—contradict the results from the primary analysis, I conducted a post hoc analysis to further explore the moderating effect of ruralness on adoption of e-commerce over time. Instead of operationalizing the measure related to whether the commuting zone is rural or urban as a continuous variable, I operationalize the measure as a categorical variable consisting of three categories: Urban, Med, and Rural. Doing so enables a better understanding of what is driving the results regarding the moderating effect, thereby establishing more specific boundary conditions (Makadok et al., 2018). The categories are assigned based on percentiles of the weighted rural-urban continuum for each commuting zone, with the related rural-urban continuum values as shown in Table 2.7.

Table 2.7 Rural-Urban Continuum Values Based on Percentiles

Category	Rural-urban continuum values included
Urban	1.00-3.44
Med	3.45-6.07
Rural	6.08-9.00

With this operationalization, I now enter the measures related to rural-urban as dummy variables. *Rural* is equal to 1 for all zones in the highest 33rd percentile[5] on the rural-urban continuum and 0 otherwise. *Med* is equal to 1 for all zones in the middle 33rd percentile on the rural-urban continuum and 0 otherwise. *Urban* is equal to 1 for all zones in the bottom 33rd percentile and zero otherwise and serves as the reference group. There are no additional random effects, and I present only the composite model in Equation 17:

[5] Recall that higher values on the Rural-Urban Continuum (which ranges from 1-9) indicate an area is more rural (U.S. Department of Agriculture, 2019).

$$
\begin{aligned}
&LogitShareTripsOnline_{it} \\
&= [\lambda_{00} + \lambda_{10}TIME_{it} + \lambda_{20}COVID_{it} + \lambda_{30}Amazon_{it} + \lambda_{40}RURAL_{i} + \lambda_{50}MED_{i} \\
&+ \lambda_{60}(TIME_{it} \times RURAL_{i}) + \lambda_{70}(TIME_{it} \times MED_{i}) + \lambda_{80}(COVID_{it} \times RURAL_{i}) \\
&+ \lambda_{90}(COVID_{it} \times MED_{i})] + [\xi_{0i} + \xi_{1i}TIME_{it} + \xi_{2i}COVID_{it} + \epsilon_{it}] \qquad (17)
\end{aligned}
$$

As in the last robustness test, I remove any commuting zones which have less than 11 observations. Results from the post-hoc analysis are presented in Table 2.8. Firstly, Model 1 indicates *a lack of support for H2 and support for H2alt* with a significant negative covariance between the random intercept and *COVID* (σ_{02} = –0.057, CI: [–0.089, –0.024]). This indicates that areas which had lower levels of adoption as of 2019 experienced the greatest increase in adoption in 2020.

Table 2.8 Post-Hoc Results: Urban-Rural as a Categorial Variable

	Label	Model 1	Model 2
Fixed effects			
Intercept	λ_{00}	–3.531*** (–154.55)	–3.524*** (–167.65)
Time	λ_{10}	0.042*** (12.75)	0.045*** (16.63)
COVID	λ_{20}	0.412*** (19.28)	0.486*** (32.75)
Amazon	λ_{30}	0.061*** (3.77)	0.037** (2.41)
Rural	λ_{40}	–0.125*** (–2.73)	–0.184*** (–3.01)
Med	λ_{50}	–0.093*** (3.29)	–0.051 (–1.30)
Time × Rural	λ_{60}		–0.017** (–2.06)
Time × Med	λ_{70}		0.006 (1.01)
COVID × Rural	λ_{80}		–0.101* (–1.79)
COVID × Med	λ_{90}		–0.119*** (–3.49)
Variance components			
Var. intercept	φ_0^2	0.281^+ (0.234, 0.338)	0.280^+ (0.232, 0.338)
Var. time	φ_1^2	0.005^+ (0.004, 0.006)	0.005^+ (0.004, 0.006)
Var. COVID	φ_2^2	0.094^+ (0.051, 0.173)	0.091^+ (0.048, 0.172)
Cov. time, COVID	φ_{12}	–0.005 (–0.010, 0.001)	–0.005 (–0.010, 0.000)
Cov. time, intercept	φ_{01}	0.023^+ (0.016, 0.029)	0.022^+ (0.016, 0.029)
Cov. COVID, intercept	φ_{02}	-0.057^+ (–0.089, –0.024)	-0.056^+ (–0.088, –0.024)
Var. residual	φ_ϵ^2	0.145^+ (0.126, 0.168)	0.145^+ (0.126, 0.168)
Model fit			
–2 log likelihood		9,265.843	9,248.871
Sample			
Observations		7,051	7,051
Number of groups		641	641

Notes: Outcome: $LogitShareTripsOnline_{jt}$. Models were analyzed with robust standard errors. z-statistics are reported in parentheses for fixed effects; **p* < .10, ***p* < .05, ****p* < .01. 95% confidence intervals are reported in parentheses for variance components; $^+$ indicates a significant variance or covariance.

Turning to Model 2, as it relates to the moderating effect of ruralness on the pre-COVID slope (H1 and H1alt), results indicate that the interaction between *Time* and the rural-urban nature of the commuting zone is significant for the most rural category (i.e., between *Time* and *Rural*) (λ_{60} = –0.017, z-value = –2.06, p < 0.05) but not for the middle category (i.e., between *Time* and *Med*) (λ_{70} = 0.006, z-value = 1.01, p > 0.10). Thus, only the most rural zones had a significantly lower rate of adoption of e-commerce from 2010-2019 than urban zones—with a magnitude of

1.69% lower odds (Allison, 2012)—providing *some support for H1 and no support for H1alt*. Lastly, as it relates to the discontinuity in 2020, which has supported H3 in previous analyses, the post-hoc analysis shows that both interactions (*COVID x Rural* and *COVID x Med*) are at least marginally significant and negative (λ_{80} = –0.101, z-value = –1.79, p < 0.10; λ_{90} = –0.119, z-value = –3.49, p < 0.01). While the coefficient for the interaction between *COVID* and *Med* is larger in magnitude than that between *COVID* and *Rural*, the two interactions—which represent 11.22% and 9.61% (Allison, 2012) lower increases in odds of e-commerce adoption at the onset of COVID-19, as compared to the most urban zones, respectively—are not significantly different.[6] This indicates that both the most rural zones (Rural) and zones in the middle of the rural-urban continuum (Med) experienced a smaller jump in e-commerce adoption at the onset of COVID-19 than the most urban zones (Urban). This indicates overall *support for H3 and a lack of support* for H3alt. Conclusions for each hypothesis across the main analysis, robustness tests, and post-hoc analysis are summarized in Table 2.9.

Table 2.9 Summary of Results

Hypothesis	**Main analysis**	**Robustness 1:** Proportion spend as dependent variable	**Robustness 2:** Remove zones with < 11 observations	**Post-hoc:** Rural/urban as a categorical variable
H1	Not supported	Not supported	Supported	Supported
H1alt	Not supported	Not supported	Not supported	Not supported
H2	Not supported	Not supported	Not supported	Not supported
H2alt	Supported	Supported	Supported	Supported
H3	Supported	Supported (marginal)	Supported	Supported
H3alt	Not supported	Not supported	Not supported	Not supported

[6] This was tested by constraining the parameters λ_{80} and λ_{90} to equality and finding a nonsignificant chi-square value.

2.8 DISCUSSION

2.8.1 Theoretical Contributions

This research contributes to theory in three primary ways. Firstly, I contribute to the literature exploring spatial determinants of e-commerce adoption focusing on the differences in adoption for urban versus rural consumers. I contribute to this literature firstly by building upon the present literature and approaching the study differently from a methodological perspective to reconcile existing insights. In particular, conflicting conclusions have been drawn from the existing literature, finding support for both the innovation diffusion perspective (e.g., Beckers et al., 2021; Cao et al., 2013; Farag et al., 2006a; Zhou & Wang, 2014) and the efficiency perspective (e.g., Cao et al., 2013; Farag et al., 2003; Kirby-Hawkins et al., 2019). While there are some inconsistencies in terms of significance of the innovation diffusion perspective across the various tests employed in the present study, I generally find support for this perspective, and I find no significant results in support of the efficiency perspective. Thus, this research contributes by building upon the study of urban versus rural areas as a boundary condition (Makadok et al., 2018) of e-commerce adoption and finding some support for the hypothesis that urban areas adopted at a quicker rate from 2010-2019—and especially to a greater degree (see γ_{40} in Table 2.4 Model 4 and further discussion in Section 2.8.2)—ultimately concluding that urban consumers stand to benefit more from e-commerce adoption across the stages of shopping (Gauri et al., 2021).

Second, I contribute theoretically by taking a longitudinal perspective and looking at how e-commerce adoption patterns change—or do not change—over time, which has been lacking in the literature, especially as it relates to considering the effects for urban versus rural areas. I do so by employing growth modeling techniques, which have not been commonly employed in the SCM literature (Miller, 2017; Miller et al., 2018). In particular, I focus on how the shock of the COVID-

19 pandemic, which impacted consumer shopping behavior to a large degree (e.g., Eger et al., 2021; Guthrie et al., 2021; Sheth, 2020), affected adoption patterns. As it relates to the moderating effect of the degree of urban or ruralness, I find that urban consumers increased adoption to a greater degree than rural consumers, as urban commuting zones experienced the greatest percentage jump in adoption in 2020. Rural-urban categorization aside, there was also a significant negative relationship between pre-COVID levels of adoption in 2019 and the jump in adoption at the onset of the pandemic, indicating that adoption shifted in 2020.

Third, there has been a call for greater focus on consumer-centric SCM research (Esper et al., 2020). Consumer-centric SCM prioritizes consumer behavior and preferences when designing supply chain services and activities (Esper et al., 2020). This research contributes to the consumer-centric SCM literature by understanding consumer behavior as it relates to e-commerce adoption. In turn, the findings regarding consumer behavior inform SCM-related decisions for retailers and last mile providers, as will be discussed in the following managerial implications (see Section 2.8.2). Additionally, by exploring consumers' adoption of e-commerce over time, not only in general (H2 and H2alt) but with a focus on consumers in urban versus rural areas (H1, H1alt, H3, and H3alt), this research responds to a call to understand how SCM services can be tailored to different consumer segments (Esper & Peinkofer, 2017). In particular, I find that there is greater adoption of e-commerce in urban than rural areas, thus establishing the degree of urban or ruralness as a boundary condition (Makadok et al., 2018) of e-commerce adoption.

2.8.2 Managerial Implications

This research also generates practical insights for retailers as well as last mile delivery providers. First, as it relates to the divide between urban and rural areas, this study generates some evidence that urban consumers adopt e-commerce at a faster rate and, especially, that urban areas have shown higher levels of adoption, as γ_{40} in Table 2.4 (Model 4) indicates that the odds of e-commerce adoption overall decreased by 3.34% (Allison, 2012) per one-unit increase in $\overline{Rural}$ (i.e., a one-unit increase on the rural scale of 1-9). Further and importantly, a quicker rate of adoption in urban areas was displayed following the onset of the COVID-19 pandemic. Based on these findings, it is important for retailers to continue building out their e-commerce networks near urban areas—especially in the case of retailers like Amazon, for example, which is shifting to a regional distribution model (Garland, 2023)—to ensure continued adequate service. Similarly, last mile providers should focus their resources and networks on urban areas—aligned with FedEx, for example, in its move to cut back weekend deliveries to rural areas (Black, 2023; Matthews, 2022)—to serve urban populations as adoption continues. A focus on urban areas will likely continue to be important given that cities are not seeing residents leaving like in 2020 (Overberg et al., 2023), indicating sustained demand.

Related to this, the finding that rural consumers adopt e-commerce at a less rapid rate and, especially, to a lower degree provides important implications for retailers. While retailers should focus on expanding e-commerce in urban areas, findings suggest that retailers may focus on expanding brick-and-mortar offerings in rural areas, and it points to the opportunity to open more physical stores to serve rural communities. One retailer that has successfully focused on expansion in rural areas is Dollar General, which has become one of the quickest growing retailers in the U.S. (Wall Street Journal, 2021). Similarly, Petco has also recently started expanding to rural areas (Jansen, 2022). Additionally, to enhance product offerings in areas that adopt e-commerce to a lesser degree, retailers may consider adopting strategies similar to those such as Tractor Supply Co., which focuses on rural areas and has enabled shoppers to access computer kiosks in stores to shop via the online channel (Stock, 2018).

Next, the challenges to serve urban areas are unique as compared to the challenges to serve rural areas from a last-mile perspective (Bretzke, 2013; Rose et al., 2016; Rose et al., 2020a), as urban areas face higher real estate costs (Gibson et al., 2018), greater congestion, and routing constraints (Rose et al., 2020a; Rosenbush & Stevens, 2015), while rural areas face higher costs and longer delivery times due to less concentrated delivery stops (Roberson, 2021). Consequently, the finding that consumers in urban areas adopt e-commerce to a greater degree and, to some extent, more rapidly has important implications. Specifically, as retailers and last mile delivery providers look for areas to improve efficiency as it relates to e-commerce—such as by focusing the offerings of last mile delivery services (Black, 2023; Matthews, 2022) or adopting technologies such as drones (Daleo, 2023)—this finding points them in a direction as to where to innovate and make changes; retailers and last mile providers should look to make improvements to address the challenges of serving urban consumers, given that such consumers are most likely to utilize e-commerce. For example, there is an opportunity to build out parcel locker delivery options (Ranjbari et al., 2023) in urban areas to encourage more consumers to complete the final leg of the last mile journey for online shopping, thereby reducing some of the complexity and congestion (Rose et al., 2020a; Rosenbush & Stevens, 2015) that last mile providers endure. Additionally, to overcome high real estate costs (Gibson et al., 2018) yet serve urban consumers quickly, retailers can utilize stores in urban areas as micro fulfillment centers to fulfill online orders (Young, 2022).

Lastly, I consider managerial insights as it relates to the support of H2alt. Regarding general patterns of adoption—controlling for rural and urban categorization—I found that areas which had a lower level of e-commerce adoption in 2019 experienced the greatest increase in adoption at the onset of COVID-19 in 2020. Thus, e-commerce adoption patterns changed. This indicates the need for retailers to now continuously evaluate their networks and offerings with real time data to adjust to evolving consumer trends (Briedis et al., 2020; McCarthy et al., 2022). The insights from this can also be extended to other services and beyond the COVID-19 context. In the case of potential future changes in demand for services—for example, buy online pickup in store (Kohan, 2023)—service providers cannot expect that previous patterns of adoption will necessarily hold. Thus, it is important to build out networks to provide such services beyond just those areas or populations that have adopted most rapidly in the past and be prepared to expand offerings based on new adoption patterns as a result of events creating discontinuities.

2.8.3 Limitations

As with any research, there are limitations to be noted with this study. Firstly, there are some limitations as it relates to the data. In particular, with the way that the transaction data is reported and the lack of specific information pertaining to what is captured in each channel, it is not clear whether all online shopping is properly captured in the online channel. However, given that the model-free evidence (Davis-Sramek et al., 2023) in Figures 2.1, 2.2, and 2.3 shows the expected general upward trend from 2010-2019 and jump in 2020, it indicates that any potential bias in the data is consistent across years and, accordingly, should not impact conclusions. Second, given that e-commerce activity declined following the spike in 2020 at the onset of COVID-19 (Alcedo et al., 2022), there is a possibility that adoption trends changed after 2020. However, I am unable to explore this based on the availability of data at the time of this study. Lastly, I am unable to capture perfectly the environment in which a household shops[7] in terms of urban-rural composition, making the measure for Rural imperfect. Given that commuting zones capture where

[7] While some store-level location data is available in the Consumer Panel Dataset which could give some additional insight into the location of a household's brick-and-mortar shopping trips, the data is missing for many stores/trips and, when it is available, is only given at the three-digit zip code level.

people live and work (U.S. Department of Agriculture, 2019), however, it is likely to adequately capture the shopping environment.

2.8.4 Future Research

There are also opportunities for future research to build upon this study. First, there is an opportunity to extend this study to future time periods to understand trends of adoption patterns as COVID-19 progressed and, eventually, after the end of the pandemic. Given that the volume of online shopping dipped after a peak in 2020 (Alcedo et al., 2022), it is possible that adoption trends changed, lending to additional insights regarding the longer-term impact of COVID-19 on behavior. Second, future research can adopt the methodological approach employed in the present study and utilize growth models to further understand adoption of e-commerce from the perspective of other predictors, such as other consumer characteristics that retailers could tailor to or additional geographic variables that impact the networks of retailers and last mile providers. Similarly, research could explore whether predictors vary across product categories and/or types of online retailers. Doing so would extend our understanding of e-commerce adoption by establishing additional boundary conditions (Makadok et al., 2018).

NOTE: Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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CHAPTER 3 – STOCKPILING: UNDERSTANDING CONSUMER BEHAVIOR DURING SHORTAGES OF PERISHABLE PRODUCTS

3.1 INTRODUCTION

The COVID-19 pandemic induced many changes and discussions regarding consumer shopping behavior in the United States (U.S.), such as an increase in online shopping (Brewster, 2022), a decrease in department store spend (Bhattarai, 2021), and an increase in reliance on big box retailers (Nassauer & Maloney, 2020) to name a few. There was also attention surrounding a previously less commonly discussed behavior: stockpiling. Also known as panic buying (Taylor, 2021), stockpiling is defined as "consumers buying large quantities of products" (Ahmadi et al., 2022b, p. 57; Blattberg & Neslin, 1990), oftentimes in fear of something bad happening (Cambridge Dictionary, 2023). Such behaviors can have negative effects such as unavailability of goods and inflated prices (Dammeyer, 2020). Early in the pandemic (i.e., late winter and early spring 2020), there was a flood of industry articles in the U.S. focused on consumers' stockpiling of products, with the largest focus on toilet paper (Taylor, 2020) but also discussion regarding items like hand sanitizer and cleaning products (Knoll, 2020). The stockpiling behavior was especially triggered by and aligned with the initial outbreak of COVID-19 cases and stay-at-home orders across the country (Rattner, 2020).

The presence of stockpiling behavior during disruptions has been supported by literature in various contexts (e.g., Pan et al., 2020, 2023), and there are a number of recent studies exploring stockpiling during widespread disruptions like COVID-19 (e.g., Omar et al., 2021; Papagiannidis et al., 2023). However, the existing stockpiling literature has focused on non-perishable products (e.g., Prentice et al., 2022; Yoshizaki et al., 2020) or both perishable and non-perishable products together (e.g., Keane & Neal, 2021; Papagiannidis et al., 2023), thus lacking a focus on stockpiling of perishable products. It is important to build upon the existing research by studying stockpiling in the context of perishable products during disruptions for two primary reasons. First, given the nature of perishable products and the challenges related to storing them, different behavior may transpire when considering disruptions to the supply of perishable products as compared to the non-perishable products commonly studied. Second, the exploration of stockpiling behavior during disruptions is still in its infancy and there have been calls for deeper exploration (Pan et al., 2023), which is especially important given the expectation of a continued environment of disruption (Flynn et al., 2021).

To explore stockpiling behavior of perishable products during disruptions, I utilize the context of another issue that followed shortly thereafter the initial discussion of stockpiling at the start of COVID-19 but received relatively less attention in both the popular press and literature: disruptions related to COVID-19 in the U.S. meat supply chain. Between April and June 2020, over 80 meat packing plants experienced confirmed COVID-19 cases, with roughly 10% of pork and beef plant employees testing positive (Cowley, 2020). This caused widespread closures, with nearly half of the plants that experienced outbreaks closing for a period of time (Cowley, 2020). The spread of cases also resulted in slower operations—both at plants that did and did not close—as a result of precautionary measures (e.g., social distancing) (Cowley, 2020). Accordingly, the potential for shortages of fresh meat products at grocery stores (Gallagher & Kirkland, 2020)—as well as actual shortages—were reported (Gellerman, 2020). The issues surrounding the meat supply chain lead to an interest in understanding potential stockpiling behavior in this context given the perishability of fresh meat and limited freezer space available to many consumers (Food Logistics, 2020), which may make stockpiling behavior counterintuitive.

Accordingly, this research aims to extend the nascent literature studying stockpiling during widespread disruptions in several ways. First, unlike previous studies (e.g., Prentice et al., 2022;

Yoshizaki et al., 2020), this research focuses on a perishable product, which is important given the possibility of heterogeneous behavior depending on product type. Second, this study takes a longitudinal perspective to understand the evolution of stockpiling, which is important given that disasters evolve over time (Ahmadi et al., 2022b). Lastly, given that there have been conflicting findings regarding the impact of income as it relates to stockpiling behavior, this study seeks to reconcile these conflicting findings and explores the role of household income as a boundary condition (Makadok et al., 2018). This moderating effect is important to understand as retailers can tailor inventory management decisions based on the demographics of their stores' customers. Further, there has been a call to understand how different consumers behave in relation to supply chain topics (Esper & Peinkofer, 2017). Accordingly, this essay addresses the following research questions: *How did consumers respond—in terms of the presence of or lack of stockpiling behavior—to COVID-19 related issues in the fresh meat supply chain? How did the behavior differ across income levels?*

Drawing on scarcity literature (e.g., Bell, 1982; Lynn, 1991), I hypothesize regarding the expected presence of stockpiling both during the disruption to the meat supply chain in April to June 2020, as well as in the period immediately following the disruption as the supply chain was recovering. I also focus on the moderating effect of income level, looking at three income cohorts: low, middle, and high income. To test my hypotheses, I employ a difference-in-differences (DiD) design. DiD is appropriate given that data is available for both treated and control products both before and after an exogeneous shock—which affected only the treated products—occurred. This allows me to compare the change in shopping behavior for three fresh meat product categories to the change in spend in four control product categories that were not affected by the disruption in the meat supply chain. To employ the DiD design, I rely on consumer transaction data from

NielsenIQ's Consumer Panel Dataset and employ Consumer Price Indices (CPI) (U.S. Bureau of Labor Statistics, 2022) to control for inflation.

The results reveal that consumers did stockpile fresh meat products during the initial disruptions (i.e., outbreak of COVID-19 cases) in the U.S. meat supply chain, as compared to before the outbreak. Among the three income cohorts studied, there is evidence that the low income cohort stockpiled to a greater degree than the high income cohort. Additionally, after the shortage risk subsided (i.e., after the meat supply chain began to return to normal operations and the risk of shortages was declining), stockpiling behavior did not follow and also subside; instead, consumers continued to stockpile as compared to before the outbreak, even after the scarcity threat was reduced.

There are several theoretical and managerial implications that result from this research. From a theoretical perspective, I first contribute to literature regarding stockpiling during disruptions by focusing on a previously unexplored product category—perishable goods—which is important as the existence of stockpiling was uncertain given the nature of the products and that the effects of scarcity can be heterogeneous (Shi et al., 2020). Second, I contribute to the scarcity literature by responding to a call to draw on multiple theories to understand the impacts of scarcity (Shi et al., 2020). Combining both commodity theory (Brock, 1998; Lynn, 1991) and regret theory (Bell, 1982; Loomes & Sugden, 1982) helps to more fully understand the mechanisms (Astbury & Leeuw, 2010) postulated to drive stockpiling behavior. Third, I contribute to both the stockpiling and scarcity literatures by taking a longitudinal perspective, which is important given that disruptions can evolve over time (Ahmadi et al., 2022b) and that the scarcity literature focused on consumer behavior has largely studied reactions during a scarcity encounter (e.g., Devlin et al., 2007; Peinkofer et al., 2016; Verhallen & Robben, 1994), thereby not providing guidance on behavior after scarcity is subsiding. Lastly, I contribute to both areas of literature by establishing income as a boundary condition (Makadok et al., 2018) of consumer stockpiling behavior as it relates to scarcity of perishable products.

This research also provides important implications for managers. During a shortage, understanding that consumers do stockpile perishable products allows both retailers and their suppliers to more accurately forecast demand. Further, given that low income households stockpiled to a larger degree, retailers can better allocate their limited inventory across their network of stores, and suppliers can similarly allocate their inventory to their retail customers. As the supply chain is recovering and the disruption subsides, the finding that consumers still continue stockpiling indicates that the influence is lasting, and retailers and their suppliers must be aware of this challenge as they aim to rebuild their inventories.

This essay is organized as follows. The next section reviews the relevant stockpiling literature. Next, I review the theory and develop hypotheses, followed by the research design, including the data, variable construction, and methodological approach. Then, I describe my econometric approach and results, followed by robustness tests. Lastly, I provide theoretical and managerial contributions, limitations, and future research directions.

3.2 LITERATURE REVIEW

Stockpiling[8] literature has traditionally focused on understanding consumers' stockpiling behavior during promotions (e.g., Ailawadi & Neslin, 1998; Bell et al., 1999; Chandon & Wansink, 2002; Ching & Osborne, 2020; Gupta, 1988; Huchzermeier et al., 2002; Mela et al., 1998). More

[8] While sometimes incorrectly used interchangeably, stockpiling is distinct from hoarding in that hoarding is instead focused on accumulating possessions to an excessive extent and having trouble discarding them due to a perceived need to hold onto them (American Psychiatric Association, 2013; Taylor, 2021). Further, stockpiling is sporadic (i.e., typically lasts for a short period of time), while hoarding is a more persistent behavior (i.e., lasts for years) (Taylor, 2021). While these behaviors could occur together, this research is concerned with and observes only what happens in the retail store and is thus focused on stockpiling behavior.

recently, the stockpiling literature has studied stockpiling behavior during disruptions or crises, focusing on contexts such as economic downturns (e.g., Pan et al., 2023), natural disasters (e.g., Beatty et al., 2019; Pan et al., 2020), and the COVID-19 pandemic (e.g., Brizi & Biraglia, 2021; Dammeyer, 2020; Papagiannidis et al., 2023). These studies have confirmed the presence of stockpiling behavior during disruptions and have demonstrated that stockpiling behavior is heterogeneous across product types (e.g., Kassas & Nayga, 2021; Micalizzi et al., 2021) as well as situational (e.g., Prentice et al., 2020, 2022) and consumer characteristics (e.g., Beatty et al., 2019; Fischer et al., 2021). A sample of the empirical literature studying stockpiling during disruptions is presented in Table 3.1.

Table 3.1 Empirical Literature Studying Stockpiling During Disruptions

Citation	Context	Stockpiling data source	Perspective	Product(s)	Stockpiling predictors	Key findings
Non-COVID related studies						
Hori and Iwamoto (2014)	Natural disaster	Daily purchase data	Longitudinal	Both; nondurables	City size; family size; age; occupation; education; standard of living; infant in household	Households stockpiled a wide variety of items at random.
Beatty et al. (2019)	Natural disaster	Scanner data (NielsenIQ)	Longitudinal	Non-perishable (bottled water, batteries, flashlights)	Income; race; proximity to coast; education	The purchase of emergency items increased with a hurricane threat. Most stockpiling occurs before landfall but still continues after the hurricane. Wealthier (poorer) areas experienced more stockpiling before (after) the hurricane.
Dong and Klaiber (2019)	Product ban	Weekly sales data	Longitudinal	Non-perishable (light bulbs)	Announcement and onset of ban	Stockpiling occurred following the ban of production and import of incandescent lights.
Hansman et al. (2020)	Export policy change	Consumer panel data and scanner data (NielsenIQ)	Longitudinal	Non-perishable (rice)	Prices	Consumers stockpile ahead of expected price increases.

Table 3.1 (cont'd)

Pan et al. (2020)	Natural disaster	Scanner data	Longitudinal	Non-perishable (bottled water)	Retailer characteristics; product variety; consumer risk perception; hurricane experience; income	There is a primarily positive relationship between income and stockpiling. Stockpiling most often occurs at drug stores prior to the onset of a hurricane.
Pan et al. (2023)	Economic downturn	Consumer panel data (NielsenIQ)	Longitudinal	Non-perishable (diapers)	Economic conditions (consumer confidence)	Both consumers that do and do not typically stockpile reduce consumption rates when consumer confidence is low.
COVID related studies						
Dammeyer (2020)	COVID-19	Survey	Cross-sectional	Both (no specific product)	Personality traits; demographics (e.g., age, gender); attitude toward pandemic	Stockpiling is related to consumers' personality traits and opinions.
Garbe et al. (2020)	COVID-19	Survey	Cross-sectional	Non-perishable (toilet paper)	Personality traits; perceived COVID-19 threat	Stockpiling is heterogeneous across consumers based on personality and perceived COVID-19 threat.
Prentice et al. (2020)	COVID-19	Posts on Twitter; retail revenue	Longitudinal	Both (general stockpiling)	Government restrictions	Government measures affect consumer stockpiling, with travel bans having minimal impact and lockdowns having a larger impact.
Wang et al. (2020)	COVID-19	Survey	Cross-sectional	Perishable (fresh food)	Willingness to pay; previous behavior; consumer characteristics (e.g., age, gender, income)	High income, female, and educated consumers are more likely to stockpile.
Yoshizaki et al. (2020)	COVID-19	Store-level transactions (3 retailers)	Longitudinal	Non-perishable (toilet paper)	Income per capita	There is a positive relationship between stockpiling and per capita income.
Ben Hassen et al. (2021)	COVID-19	Survey	Cross-sectional	Both (general stockpiling)	Income; household composition; gender; age; concern of shortage; concern of price increases; emotions	Stockpiling is heterogeneous across consumers and relates to factors such as negative emotion and sociodemographic variables.
Brizi and Biraglia (2021)	COVID-19	Survey	Cross-sectional	Both (food in general)	Need for closure; perception of lack of food; gender; country	Consumers with high levels of need for cognitive closure tend to stockpile more due to a perception of lack of food.

Table 3.1 (cont'd)

Fischer et al. (2021)	COVID-19	Online experiment	Cross-sectional	Both (food in general, but pasta in stimulus)	Scarcity level; personality traits	Honesty-humility related to negatively to stockpiling, while victim sensitivity and emotionality related positively to stockpiling. Scarcity did not moderate the effects.
Herjanto et al. (2021)	COVID-19	Survey	Cross-sectional	Both (general stockpiling)	Perceived risk; situation ambiguity; thinking style; information overload	Situational ambiguity and perceived risk led to stockpiling.
Kassas and Nayga (2021)	COVID-19	Survey	Cross-sectional	Both (variety of items)	Income; household size and composition; ethnicity; views on COVID-19; product type	Stockpiling behavior is heterogeneous across product types and consumers' demographics and behavioral traits.
Keane and Neal (2021)	COVID-19	Google search data	Longitudinal	Both (various foods)	Internal restrictions; stimulus; travel restrictions; COVID-19 cases	Stockpiling was widespread across countries in March 2020, but timing and severity differed between countries. Internal restrictions and other factors induced stockpiling.
Lehberger et al. (2021)	COVID-19	Survey	Cross-sectional	Non-perishable	Norms; attitudes; perceived control; neuroticism; fear of future unavailability	Attitudes, fears, and norms drive stockpiling.
Micalizzi et al. (2021)	COVID-19	Survey	Cross-sectional	Both (variety of items)	Political affiliation; concern with COVID-19; gender; education; size of household	Stockpiling varied by individual factors and item; toilet paper was most commonly stockpiled.
Omar et al. (2021)	COVID-19	Survey	Cross-sectional	Both (groceries in general)	Uncertainty; perceived severity; perceived scarcity; anxiety	Anxiety mediates the relationships between stockpiling and both uncertainty and scarcity.

Table 3.1 (cont'd)

Ahmadi et al. (2022a)	COVID-19	Mobility data	Longitudinal	Both (general stockpiling)	Uncertainty avoidance; long-term orientation; indulgence; individualism	Countries whose residents have higher levels of uncertainty avoidance and individualism and lower levels of long-term orientation and indulgence stockpiled more.
Ahmadi et al. (2022b)	COVID-19	Survey	Cross-sectional (stockpiling behavior); longitudinal (motives)	Both (general stockpiling)	Fear; shortage expectations; trust in government	Fear and shortage expectations positively relate to stockpiling.
Amaral et al. (2022)	COVID-19	Survey	Cross-sectional	Both (general stockpiling)	Job security; health risk; locus of control	Individuals with internal locus of control engage in less stockpiling than those with external locus of control. Health risk is positively related to stockpiling.
Papagiannidis et al. (2023)	COVID-19	Survey	Cross-sectional	Both (food in general)	Perceived scarcity; vulnerability; self-efficacy; response efficacy; perceived stockpiling costs; social norms; delivery slots; stockouts; health and safety guidelines	Stockpiling helped minimize anxiety and fear, while increasing wellbeing, during COVID-19 lockdowns.
Prentice et al. (2022)	COVID-19	Survey	Cross-sectional	Non-perishable (hand sanitizer, toilet paper, food staples)	Government measures; peer influence; media influence; fear of missing out	Government measures and influence from peers and media drove stockpiling.

There are four primary observations from the disruption-related stockpiling literature relevant to this study. Firstly, the literature has focused primarily on the stockpiling of non-perishable products—such as bottled water (Beatty et al., 2019; Pan et al., 2020) or toilet paper (Prentice et al., 2022; Yoshizaki et al., 2020)—or a mixture of both perishable and non-perishable goods with little to no distinction made among product groups (e.g., Keane & Neal, 2021; Papagiannidis et al., 2023). In this research, I contribute to the stockpiling literature by exploring behavior in response to a disruption-related shortage of fresh, perishable products. This is important as consumer purchasing behavior may differ depending on product type given that scarcity has heterogeneous effects depending on the context (Shi et al., 2020).

Second, much of this literature has utilized cross-sectional data (e.g., Amaral et al., 2022; Herjanto et al., 2021; Omar et al., 2021), thus looking at consumer behavior at a point in time during a disaster. While these studies have yielded valuable insights, given that disruptions evolve over time it is important to take a longitudinal perspective (Ahmadi et al., 2022b) to generate more actionable insights for retailers and their suppliers. The existing studies which *have* taken a longitudinal perspective differ from the present study in important ways. First, these studies have mostly focused on disruptions which have restricted scope and most heavily impact a limited geographical area, such as hurricanes (e.g., Beatty et al., 2019; Pan et al., 2020). Second, the longitudinal studies which have focused on more widespread disruptions have suffered from two limitations: (1) As previously noted, such studies have focused only on non-perishable products (Yoshizaki et al., 2020) or have combined perishable and non-perishable products with little to no distinction among product types (Ahmadi et al., 2022b; Keane & Neal, 2021; Prentice et al., 2020), thus limiting the ability to draw conclusions given the heterogeneous effects of scarcity (Shi et al., 2020). (2) Additionally, such studies have tended to examine stockpiling behavior during a time when *all* grocery products were likely to be affected by stockpiling (i.e., panic buying) and shortages at the start of COVID-19 (Knoll, 2020), restricting the ability of researchers to establish causal identification. By exploring the impact of COVID-19 related issues after the initial surge of panic buying that took place at the start of the pandemic, I build upon the existing insights by establishing causal identification, thus generating additional insights for retailers and their suppliers.

Third, in addition to the cross-sectional nature of the data utilized, the primary data source in the disruption-related stockpiling literature—especially that exploring widespread disruptions like COVID-19—has been surveys of consumers. In doing so, the measure of stockpiling has been mostly perceptual and/or retrospective in nature. For example, Kassas and Nayga (2021) measured consumers' view of the importance of panic buying (i.e., stockpiling), and Omar et al. (2021, p. 6) adapted measures that asked participants to rate their agreement with statements related to the behavior such as, "While shopping for groceries, I have bought more products than what I intended to buy." Retrospective measures such as that used by Omar et al. (2021) may suffer from recall bias (Tax et al., 1998). Thus, I build upon the literature by utilizing transaction data to measure actual consumer behavior.

Lastly, there has been some exploration regarding the impact of consumer or household income on stockpiling behavior in both the COVID (Ben Hassen et al., 2021; Kassas & Nayga, 2021; Wang et al., 2020; Yoshizaki et al., 2020) and non-COVID contexts (Beatty et al., 2019; Pan et al., 2020). However, conflicting conclusions have been drawn. Research has found both positive (Beatty et al., 2019; Pan et al., 2020; Wang et al., 2020; Yoshizaki et al., 2020) and negative relationships (Beatty et al., 2019; Kassas & Nayga, 2021) between income and stockpiling. A lack of relationship has also been identified (Ben Hassen et al., 2021). I aim to settle these inconsistent findings by establishing causal identification. In doing so, this research seeks to contribute to further understanding the relationship between consumer characteristics and stockpiling behavior by establishing income as a boundary condition (Makadok et al., 2018) in the context of fresh, perishable products.

3.3 THEORY AND HYPOTHESIS DEVELOPMENT

To support this research, I draw from facets of the scarcity literature, particularly that which studies how scarcity impacts consumer shopping behavior. As summarized by Shi et al. (2020), scarcity research is primarily approached from the perspective of four theories: commodity theory (Brock, 1998; Lynn, 1991), regret theory (Bell, 1982; Loomes & Sugden, 1982), conformity theory (Bernheim, 1994; Jones, 1984), and reactant theory (Lessne & Notarantonio, 1988). The two perspectives relevant in the current context are commodity theory and regret theory. The commodity theory perspective highlights that limited availability of a product increases the consumer perceived attractiveness and value of a product (Brock, 1998; Lynn, 1991). Regret theory highlights that consumers make decisions to avoid future regret (Bell, 1982; Loomes & Sugden, 1982); accordingly, consumers will value a product more if future availability is questionable.

I first explain why I expect that the risk of the shortage of fresh meat products will increase consumers' stockpiling behavior. In general, product scarcity should lead to stockpiling for two primary reasons. First, in line with commodity theory and its perspective of the scarcity effect, limited availability of a product increases the attractiveness of such product (Lynn, 1991). In the present context, during the incidence of COVID-19 cases spreading through the U.S. meat supply chain, the potential for shortages (Gallagher & Kirkland, 2020) and actual shortages were reported (Gellerman, 2020), thus indicating limited availability. Therefore, it is expected that consumers will perceive limited availability of fresh meat, thus leading to an increased attractiveness (Lynn, 1991). This is further supported by research indicating that scarcity increases purchase intentions (Aggarwal et al., 2011).

Second, in line with regret theory and its expectations regarding the scarcity effect, people tend to make decisions in order to avoid regret to the greatest degree possible (Bell, 1982). In this case, there is potential for future regret, which I expect will influence consumers to stockpile. In particular, in the face of a shortage risk of fresh meat, consumers will likely be worried about the possibility of not being able to obtain the desired product in the future as the disruption continues. If consumers do not act and increase their purchase volumes of meat (i.e., stockpile) while they are able, there is then the potential for regret (Abendroth & Diehl, 2006). It is accordingly expected that consumers aware of a scarcity of fresh meat products will stockpile to avoid the potential regret (Bell, 1982) that could ensue if they are not able to obtain the desired products in the future.

While the expectation of stockpiling during a shortage or disruption is supported by theory, such behavior might seem counterintuitive in the present context. Specifically, the perishability of fresh meat and limited freezer space available to many consumers (Food Logistics, 2020) may prevent consumers from stockpiling. However, I do still expect the scarcity effect to hold and drive stockpiling behavior. This is because consumers do not always behave rationally (Simon, 2000)—especially in the face of uncertainty (Jung & Kellaris, 2004), which is likely to be present during disruptions (Altig et al., 2020)—and thus, may not consider such practicalities of storage space and perishability. Additionally, stockpiling in general is often driven by fear (Cambridge Dictionary, 2023), further indicating lack of rationality associated with the behavior. Therefore, I expect that the scarcity effects will prevail even with perishable meat products. Accordingly, I hypothesize:

H1: The risk of a shortage of fresh meat products will lead to an increase in purchase volume.

I next focus on the expected moderating effect of household income on stockpiling behavior. As it relates to this relationship, there may be more than one mechanism in action, so I derive two competing hypotheses (H2 and H2alt) given the theoretical ambiguity. Further, existing research has uncovered conflicting findings regarding the impact of income on stockpiling behavior in various contexts (Beatty et al., 2019; Ben Hassen et al., 2021; Kassas & Nayga, 2021; Pan et al., 2020), again contributing to the theoretical ambiguity.

I first explicate why I expect that high income households will stockpile fresh meat to a greater degree than low income households. Consider that there are two types of scarcity: product scarcity and resource scarcity (Hamilton et al., 2019). While product scarcity is defined as "a real or perceived lack of goods and services available to the consumer," resource scarcity is a "real or perceived lack of various forms of capital (i.e., financial, social, cultural) or other production inputs (i.e., time) that the consumer invests in order to acquire and use goods and services" (Hamilton et al., 2019, p. 533). In the case of a shortage risk of fresh meat products, all consumers seeking to obtain such products face the issue of product scarcity; however, low income consumers also likely face resource scarcity in terms of limited financial capital (Hamilton et al., 2019). This is supported by behaviors of low income households, who typically spend money more carefully (Hamilton et al., 2019; Shah et al., 2012) and consider tradeoffs when making spending decisions (Shah et al., 2015; Spiller, 2011). While as detailed in H1 I expect that product scarcity in general will lead to increased stockpiling behavior, I expect that for low income households, resource scarcity may counteract—at least to a degree—the expected effects of product scarcity.

Accordingly, given a lesser chance for the presence of resource scarcity, high income households will be more likely than low income households to be influenced by the effects of product scarcity, thereby viewing the scarce products as attractive (Lynn, 1991) and purchasing the products in order to minimize the potential for regret (Bell, 1982). This, in turn, should lead to a larger degree of stockpiling for the high income households given that scarcity increases purchase intentions (Aggarwal et al., 2011). This logic aligns with findings that high income households take advantage of promotions to a larger degree due to such households having more financial flexibility to do so (Teunter, 2002), indicating a lack of resource scarcity (Hamilton et al., 2019). I accordingly hypothesize:

H2: High income households will increase purchase volumes of fresh meat products to a greater degree during the shortage risk than low income households.

Alternatively, I expect that the presence of resource scarcity (Hamilton et al., 2019) may actually drive *greater* stockpiling for low income households as compared to high income households. First, consider that low income households are more price sensitive (Wakefield & Inman, 2003). Given that consumers tend to expect impending price increases when product availability is low (Lynn & Bogert, 1996), low income households may participate in pre-emptive buying in the face of a shortage risk. Additionally, low income households are often farther away from large retailers and have access to fewer options (Moore & Diez Roux, 2006), leading to fewer alternatives and greater repercussions if they cannot purchase the desired products in the future if they delay the purchase. Further, high income households may have more opportunities to engage with independent farmers directly to purchase a bulk order of meat (Jiang, 2020), leading to less of a need to stockpile in the traditional grocery store channel. This leads to an expectation that low income households face the possibility of greater potential regret if they do not stockpile while they are able to do so (i.e., before inventory is exhausted).

Thus, in order to minimize regret (Bell, 1982), I expect that low income households will be *more* likely to stockpile during the disruptions in the meat supply chain. This logic is supported by Shi et al. (2020, p. 388), who state, “Those consumers who have a greater need to avoid future regret choose to buy a product not because of its utility but, rather, because they are concerned that they won't be able to buy it in the future." This expectation also aligns with Laroche et al. (2001) who found that high income shoppers are less likely to engage in promotions and stockpiling behavior, given they were well-off enough to not need deals. Taken together, I hypothesize:

H2alt: High income households will increase purchase volumes of fresh meat products to a lesser degree during the shortage risk than low income households.

Finally, I hypothesize regarding how stockpiling behavior will evolve as the shortage risk subsides. The initial inclination may be that stockpiling behavior would subside given that scarcity is eliminated, and scarcity was driving the enhanced attractiveness (Lynn & Bogert, 1996) and desire to avoid regret (Bell, 1982). However, the scarcity literature has lacked a longitudinal perspective, as much of the literature has utilized behavioral experiments to study consumer reactions during a scarcity encounter (e.g., Devlin et al., 2007; Peinkofer et al., 2016; Verhallen & Robben, 1994). Driven by uncertainty about future scarcity, I expect that stockpiling behavior will continue for some time even after product availability concerns abate. While the scarcity was reduced or eliminated, there was likely still uncertainty around whether the scarcity would occur again. Thus, the threat of a potential scarcity would again make the product more attractive (Lynn, 1991) and increase purchases (Aggarwal et al., 2011). This is supported by the finding that uncertainty (Papagiannidis et al., 2023) or a desire to avoid uncertainty (Jung & Kellaris, 2004) can drive stockpiling and that there was high uncertainty during the time period of COVID-19 issues within the meat supply chain (Altig et al., 2020).

H3: As the shortage risk of fresh meat products subsides, volume purchased will remain above pre-shortage levels.

3.4 RESEARCH DESIGN

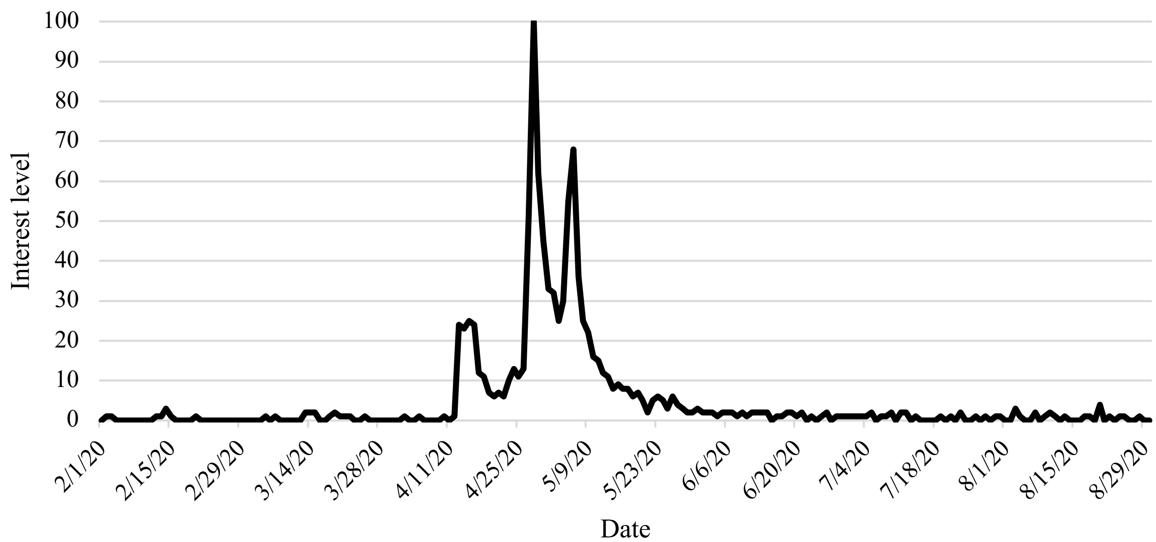
3.4.1 Methodological Approach and Causal Identification

With the onset of COVID-19 issues in the meat supply chain in late spring and early summer 2020, I used this setting to implement a difference-in-differences (DiD) approach to establish causal identification by identifying product groups that were affected by the outbreak of COVID-19 cases in meat plants (treatment products) as well as product groups that were not affected by this outbreak (control products). By looking at two time periods after the onset of the treatment—both during the outbreak and after the outbreak is subsiding—I can compare (1) the change in the outcome of interest for the treated products before and after the onset of COVID-19 issues in the meat supply chain to the change in the outcome of interest for the control products before and after the onset (Scott et al., 2021) to test H1, H2, and H2alt, and (2) the change in the outcome of interest for the treated products before the outbreak of cases within the meat supply chain and after the outbreak begins to subside to the change in the outcome of interest for the control products before the outbreak of cases and after the outbreak begins to subside to test H3.

3.4.2 Timeline

Given the DiD design of this study, it was essential to determine an appropriate timeline for the study to test my hypotheses regarding the outbreak period—during which the meat supply chain was experiencing COVID-19 related closures and slowdowns—and after the outbreak risk was subsiding. I rely on several sources of information to determine the timeline. Firstly, I utilized information from the USDA (U.S. Department of Agriculture, 2021), which stated, "Partial plant closures and increased social distancing protocols were implemented at meatpacking plants across the country starting in late April 2020 through early June" 2020. The start of this general timeline was corroborated by popular press sources such as an article from April 21, 2020, which stated that meat processing plants in at least eight states had been forced to temporarily close due to COVID-19 cases (Gray Television, 2020) and an April 26, 2020, article which reported that (Gallagher & Kirkland, 2020) meat packaging and processing plants had faced outbreaks leading to closures. The timeline of late April to early June was also supported by data from Google trends (Google Trends, 2020). Conducting a search for the term "meat shortage" in the U.S. from February through August 2020 showed a peak in interest on April 28, and by June 7, the prominence of searches regarding "meat shortage" was down to just 2% of the peak, as plotted in Figure 3.1 (Google Trends, 2020).

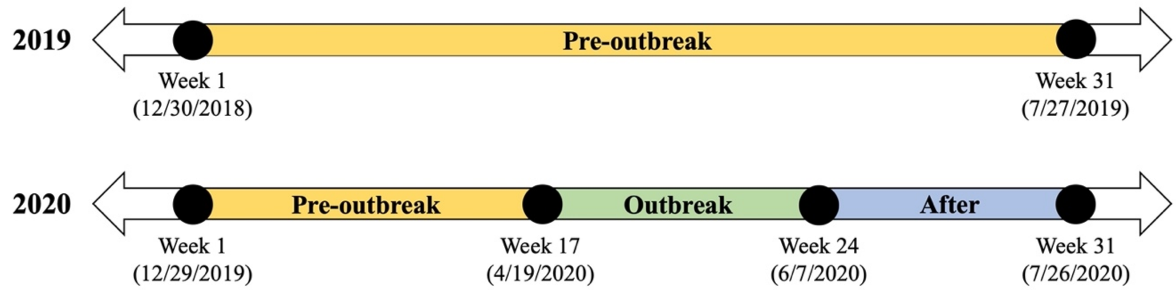
Figure 3.1 "Meat Shortage" Internet Searches in the U.S. from February Through August 2020



Notes: The interest level is represented as relative to the peak popularity over the timeframe. Therefore, 100 represents peak popularity over the time period, while a value of 50, for example, represents that on that date the search term was half as popular as the peak.

Based on the aforementioned information, I determined the timeline as shown in Figure 3.2. Firstly, the Outbreak period is assigned to April 19, 2020, through June 6, 2020 (Week 17 – Week 23 of 2020). The After period includes the same number of weeks as the Outbreak period (7 weeks) and is assigned to June 7, 2020, through July 25, 2020 (Week 24 – Week 30 of 2020). Note that the NielsenIQ Consumer Panel Dataset is available for each year starting on the Sunday of the prior year, so I assigned the count to each week as starting on a Sunday. For the Pre-outbreak phase, I include all weeks in 2020 prior to the Outbreak (Week 1 – Week 16). I also include in the Pre-outbreak phase the same full 30-week timeframe (Week 1 – Week 30) for 2019 to control for seasonality, as will be described later.

Figure 3.2 Study Timeline



3.4.3 Data Sources

I will first briefly introduce the data sources upon which this research relies and will discuss more regarding how I use each data source in a later section. The first data source is NielsenIQ Consumer Panel Dataset. Utilized in recent supply chain and operations management research including Lim et al. (2021), Pan et al. (2023), and Sodero (2022), the Consumer Panel Dataset provides a panel of approximately 60,000 households in the U.S. It includes data regarding household characteristics, shopping trips made throughout the year (e.g., when, where, total cost, etc.), and the products purchased. Data is available at a product-level for fast-moving consumer goods, making this dataset appropriate for understanding shopping patterns regarding grocery items. From the NielsenIQ Consumer Panel Dataset, I sourced transaction data (item purchased, price paid, date) and household income. In addition, I utilized the U.S. Bureau of Labor Statistics to source Consumer Price Indices (CPI) (U.S. Bureau of Labor Statistics, 2022) to control for inflation.

3.4.4 Unit of Analysis and Sample

Based on the data coverage, the unit of analysis for this research becomes the product category × income cohort × year × week of year.[9] The sample consists of seven product categories, three income cohorts, two years (2019, 2020), and 30 weeks per year (Week 1 – Week 30). Therefore, the sample consists of a total of 1,260 observations for the main analysis. For the product categories, I identified three treated categories—fresh beef, fresh turkey, and fresh pork—and four control categories—refrigerated dairy milk, fresh lettuce, fresh carrots, and frankfurters (franks). While many of the closures during the studied time period affected beef and pork plants (Cowley, 2020), including turkey in the treatment group—which was still affected (McCarthy & Danley, 2020) but perhaps to a somewhat lesser degree—makes results conservative. The control product categories were chosen as they are perishable products with short shelf lives for milk, lettuce, and carrots, similar to that of fresh meat. Franks were included as they are also a meat product, indicating some similarity to the treated categories, but their longer shelf life as compared to fresh meat (FoodSafety.gov, 2021) means that franks would have been affected to a far lesser degree by the outbreak of COVID-19 cases in the meat supply chain.

For the income cohorts, I utilize three groups based on the annual income for the household in the panel: low income ($0–$49,999), middle income ($50,000–$99,999), and high income ($100,000+). I base the low income category on the fact that in the U.S., households of three making less than $52,000 a year are considered low income (Walrack, 2023). As the household income data is available in ranges, the closest cutoff was $49,999. The high income cohort begins at $100,000 as this is the highest bound provided by NielsenIQ (i.e., all households making $100,000 or higher are coded as belonging to the same income group in the data).

[9] A household level of analysis was of interest. However, there was not enough data coverage at a weekly frequency at the household level and isolating the outbreak timing was important.

3.4.5 Variables

3.4.5.1 Dependent Variable

For notational purposes, I utilize i to index each product category, j to index the income cohort, t to index the year, and k to index the week of the year. To operationalize stockpiling, the dependent variable is the natural log of the dollars spent[10] in a product category × income cohort × year × week of year, which I denote as $LnSpend_{ijtk}$. The natural log transformation was applied to improve interpretability by reporting results in percentage form. I retrieved the dollars spent at a shopping trip × item level. I linked each of the following: (a) each shopping trip to the respective household to determine the income cohort, (b) the date of each trip to the appropriate year and week, and (c) each item to the appropriate product category.

3.4.5.2 Independent Variables

There are four focal predictors. The first is labeled $Outbreak$ and represents the outbreak period—during which the meat supply chain was affected by COVID-19 related plant closures and process slowdowns—for the *treated products* (i.e., meat products, which experienced the outbreak of COVID-19 cases within the supply chain during this time period). Thus, $Outbreak$ is assigned a value of 1 during the outbreak period (Week 17 – Week 23 of 2020) for the products that were affected by the outbreak. For products in the control group and for the treated products during all other times of the study, this variable is assigned a value of 0. The second focal predictor is labeled $After$ and represents the period that COVID-19 cases in the meat supply chain were improving (i.e., the after period) for the *treated products*. Thus, $After$ is assigned a value of 1 during the after period (Week 24 – Week 30 of 2020) for the products that were affected by the outbreak in

[10] I also explored the option of utilizing quantity purchased as the dependent variable, but the reporting of sizes differed, making quantities difficult to compare across SKUs (e.g., some were reported in ounces while others were reported in counts without a unit).

the meat supply chain. For products in the control group and for the treated products during all other times of the study, the variable is assigned a value of 0.[11]

Next are two moderating variables that correspond to income cohorts and enable the testing of H2 and H2alt. With three income cohorts, the low income cohort is treated as the reference group. Therefore, the first moderating variable is $HighIncome$, which is assigned 1 for the high income cohort and 0 otherwise. The second is $MidIncome$, which is assigned a 1 for the middle income cohort and 0 otherwise.

3.4.5.3 Control Variables

Several control variables are included in the model. First, given that the dependent variable is measured in dollars, I control for inflation by including the natural log of the Consumer Price Index (CPI)[12] for a given product category × year × week of year. This variable is labeled $LnCPI$ and is sourced from the U.S. Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2022). The CPI data is available on a monthly basis at the product type level. I therefore map the monthly data to the respective weeks in my dataset.[13] The CPI data is sourced on a not seasonally adjusted basis and is adjusted to set January 2019 equal to 100. The natural log transformation is applied to improve interpretability.

I also include several fixed effects. First, I include product × week of year fixed effects (γ_{ik}) to account for seasonality within a product group. Franks, for example, showed an increase in sales around Memorial Day (week 22) and the Fourth of July (week 27) in both 2019 and 2020.[14]

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[11] The operationalization for the key predictors is aligned with Scott et al. (2023) for example.

[12] While pricing data is also available from NielsenIQ, it was not feasible to utilize to adjust for inflation/pricing given the product types of interest, as many fresh products are not assigned a barcode and therefore are not linked to prices in the dataset.

[13] For weeks that occur over two months, I assign the CPI data on a weighted basis (e.g., if the new month begins on a Tuesday, the earlier month's CPI is weighted 2/7 and the new month's CPI is weighted 5/7).

[14] I confirmed that the week of the year in which Memorial Day and the Fourth of July fell was the same in both 2019 and 2020.

Second, I include product × income cohort fixed effects (λ_{ij}) to account for any stable purchasing patterns within an income group and product category. If, for example, one income cohort in general tends to purchase more fresh beef overall than other income cohorts, this fixed effect will control for that behavior. Lastly, I include year × week of year fixed effects (α_{tk}) to control for any average effects across all product groups during a given week. For example, this accounts for the general stockpiling behavior at the start of COVID-19 in March 2020. Table 3.2 contains descriptive statistics.

Table 3.2 Descriptive Statistics

Variable	Mean	Standard deviation	Minimum	Maximum
Weekly spend				
Fresh beef	\$24,440.79	\$4,342.75	\$18,490.11	\$37,412.71
Fresh turkey	\$7,211.73	\$1,201.51	\$4,933.05	\$10,639.97
Fresh pork	\$1,917.35	\$371.85	\$1,318.84	\$3,229.97
Dairy milk	\$61,603.40	\$4,837.60	\$53,651.53	\$78,362.50
Fresh lettuce	\$12,072.68	\$1,091.20	\$10,154.92	\$14,529.86
Fresh carrots	\$9,048.18	\$1,316.25	\$7,120.69	\$13,393.12
Franks	\$15,645.65	\$4,779.07	\$9,637.84	\$28,866.37
All products	\$131,939.77	\$13,728.30	\$109,254.39	\$172,364.94
CPI	103.42	5.18	95.15	127.68

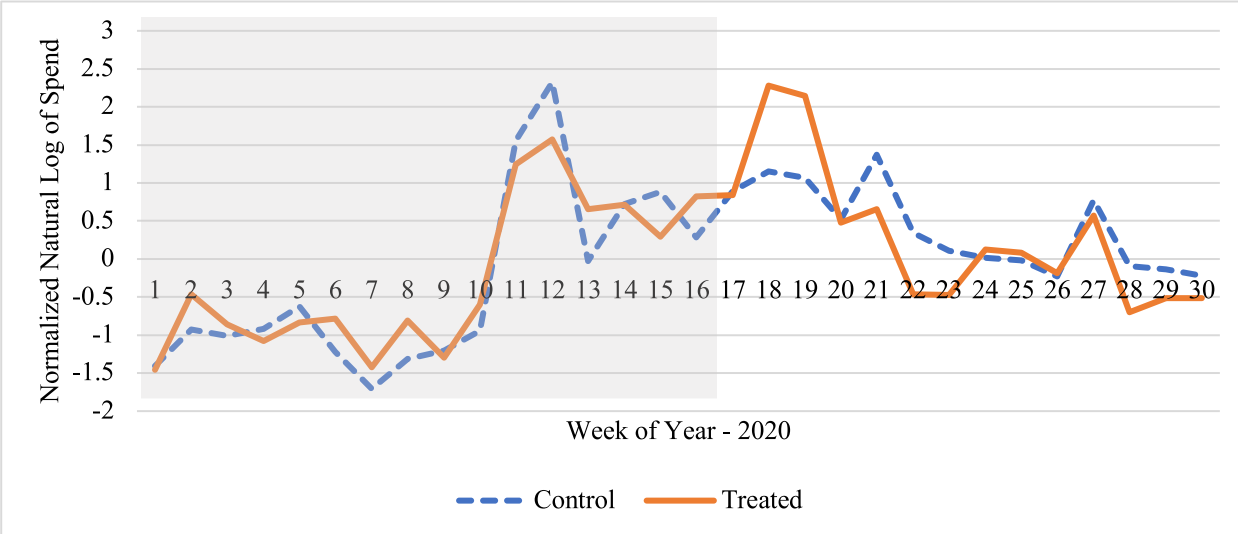
Note: Descriptive statistics are reported before natural log transformation.

3.4.6 DiD Assumptions

To establish causal identification via DiD, several assumptions must be met. First, the treatment must be exogeneous to other factors that could impact the outcome (Scott et al., 2021). Other factors that may impact the outcome of spend in a product category are, for example, sales/promotions and holidays (e.g., Memorial Day may influence consumers to purchase more meat for barbecues). However, COVID-19 issues in the meat supply chain are exogenous to both of these, as sales/promotions in the retail store would not influence the spread of COVID-19 in processing or packaging facilities upstream. Further, while there is sometimes an expectation of increased spread of COVID-19 cases following holidays (Scott, 2022), the treatment is seen to be exogenous to holidays in this setting as COVID-related issues in meat plants were starting to decline by early June 2020 (U.S. Department of Agriculture, 2021) when an increase would have been expected following the Memorial Day holiday.

It is also important that the parallel trends assumption is met, which states that in the absence of the treatment, the treated product group would have evolved in the same way over time as the control product group (Angrist & Pischke, 2009). Accordingly, I looked at the trends prior to the treatment onset to evaluate whether the trends for the treatment and control groups move in parallel. To evaluate the parallel trends assumption visually, the natural log of spend by treated versus control products for the study period—normalized (Draca et al., 2011) by the total spend over weeks 1 through 30 in 2020—is plotted in Figure 3.3. The period prior to the treatment—of interest for evaluating the parallel trends assumption—is shaded. Prior to the treatment onset, Figure 3.3 illustrates that the trends were evolving in the same way. Importantly, both groups experienced a spike in spend around March 2020, when the initial onset of COVID-19 in the U.S. occurred. Note also that after the onset of the COVID-19 cases in the meat supply chain (i.e., starting during week 17), there is a spike in spend in the treated category, supporting predictions via model-free evidence (Davis-Sramek et al., 2023).

Figure 3.3 Normalized Spend by Week for 2020



Additionally, I evaluate the parallel trends assumption statistically by fitting two panel regression models to determine whether there are different time trends for treated versus control product groups. Specifically, I examine whether dummies representing time trends in Weeks 1 – 16 (pre-outbreak) interact with a dummy variable indicating that a product grouping is part of the treated category (meat products). Aligned with Scott et al. (2021), I utilize the Parks estimator (Parks, 1967), and results are reported in Table 3.3. As can be seen, interactions added in Model 2 between the week dummies and treated dummy are nonsignificant. Further, conducting a likelihood ratio test indicates that the model fit does not improve by adding the interactions (likelihood ratio $\Delta\chi^2 = 7.95$, df = 15, p = 0.926), indicating that there are not significantly different time trends for treated versus control products prior to the onset of the outbreak of COVID-19 cases in the meat supply chain. This provides additional support for the parallel trends assumption.

Table 3.3 Panel Regression Models to Evaluate the Parallel Trends Assumption

Outcome: LnSpend	**Model 1**	**Model 2**
Intercept	10.005*** (286.23)	10.070*** (214.95)
Week 2	0.060 (1.41)	0.053 (0.97)
Week 3	0.037 (0.86)	0.048 (0.89)
Week 4	0.019 (0.45)	0.052 (0.96)
Week 5	0.056 (1.31)	0.104* (1.91)
Week 6	–0.010 (–0.23)	0.024 (0.44)
Week 7	–0.046 (–1.16)	–0.044 (–0.81)
Week 8	0.004 (0.09)	–0.018 (–0.33)
Week 9	–0.014 (–0.32)	0.013 (0.24)
Week 10	0.046 (1.08)	0.062 (1.14)
Week 11	0.300*** (7.09)	0.318*** (5.89)
Week 12	0.387*** (9.14)	0.389*** (7.20)
Week 13	0.169*** (3.98)	0.177*** (3.27)
Week 14	0.231*** (5.45)	0.225*** (4.17)
Week 15	0.223*** (5.27)	0.244*** (4.51)
Week 16	0.197*** (4.66)	0.186*** (3.43)
Week 2 × Treated		0.016 (0.20)
Week 3 × Treated		–0.027 (–0.32)
Week 4 × Treated		–0.076 (–0.92)
Week 5 × Treated		–0.112 (–1.35)
Week 6 × Treated		–0.077 (–0.94)
Week 7 × Treated		–0.012 (–0.14)
Week 8 × Treated		0.051 (0.62)
Week 9 × Treated		–0.062 (–0.75)
Week 10 × Treated		–0.037 (–0.45)
Week 11 × Treated		–0.042 (–0.51)
Week 12 × Treated		–0.005 (–0.06)
Week 13 × Treated		–0.018 (–0.22)
Week 14 × Treated		0.013 (0.16)
Week 15 × Treated		–0.048 (–0.59)
Week 16 × Treated		0.027 (0.33)
Product fixed effects	Included	Included
Log likelihood	124.995	128.972
Observations (product × week)	112	112

Notes: z-statistic in parentheses. Standard errors are calculated via the Parks (1967) method. Reference week is Week 1. **p* < .10, ***p* < .05, ****p* < .01.

3.5 ANALYSIS AND RESULTS

3.5.1 DiD Model

To test my hypotheses, I employed a series of DiD models. To test H1 and H3, I employ the following model:

$$LnSpend_{ijtk} = \beta_0 + \gamma_{ik} + \lambda_{ij} + \alpha_{tk} + \beta_1 Outbreak_{itk} + \beta_2 After_{itk} + \beta_3 LnCPI_{itk} + \varepsilon_{ijtk} \quad (1)$$

where the variables are as described previously, and ε_{ijtk} is the residual of $LnSpend_{ijtk}$. H1 predicts that β_1 is positive and significant, while H3 predicts that β_2 is positive and significant.

Next, to test H2, I add interaction terms between the outbreak period and income cohort groups as follows:

$$\begin{aligned} LnSpend_{ijtk} = {} & \beta_0 + \gamma_{ik} + \lambda_{ij} + \alpha_{tk} + \beta_1 Outbreak_{itk} + \beta_2 After_{itk} + \beta_3 LnCPI_{itk} \\ & + \beta_4 (Outbreak_{itk} \times HighIncome_j) + \beta_5 (Outbreak_{itk} \times MidIncome_j) \\ & + \varepsilon_{ijtk} \qquad (2) \end{aligned}$$

where H2 predicts that β_4 and β_5 will be positive and significant, and H2alt predicts that β_4 and β_5 will be negative and significant.

3.5.2 Main Results

Results from the main analysis are presented in Table 3.4. The models were estimated using panel data estimation with fixed effects in Stata 16.1. Model 1 evaluates Equation 1, excluding $LnCPI$, which results in an unconditional DiD model. Model 1 provides initial support for both H1 and H3. As it relates to H1, consistent with expectations, β_1 is statistically significant and positive (β_1 = 0.196, p < 0.01). The coefficient indicates that the change in spend between the pre-outbreak and outbreak periods was 19.6% higher for the treated product groups as compared to the control groups. Related to H3, consistent with expectations, β_2 is statistically significant and positive (β_1 = 0.171, p < 0.01). The coefficient indicates that the change in spend between the pre-outbreak and after periods was 17.1% higher for the treated product groups as compared to the control groups. However, given a concern with changes in prices over time (Bureau of Economic Analysis, 2023), Model 2 includes the control of $LnCPI$. Though the size of the coefficients decreases, indicating the importance of the control, results still hold.

Table 3.4 Main Results

Outcome: LnSpend	Label	Model 1	Model 2	Model 3
Intercept	β_0	8.250*** (214.64)	4.226*** (7.31)	4.226*** (7.30)
Outbreak	β_1	0.196*** (9.80)	0.139*** (6.56)	0.143*** (5.39)
After	β_2	0.171*** (8.52)	0.114*** (5.38)	0.114*** (5.37)
LnCPI	β_3		0.864*** (6.98)	0.864*** (6.97)
Outbreak × High income	β_4			–0.001 (–0.05)
Outbreak × Mid income	β_5			–0.008 (–0.30)
Product × week of year fixed effects	γ_{ik}	Included	Included	Included
Product × income cohort fixed effects	λ_{ij}	Included	Included	Included
Year × week of year fixed effects	α_{tk}	Included	Included	Included
Observations		1,260	1,260	1,260

Notes: z-statistic in parentheses. *$p < .10$, **$p < .05$, ***$p < .01$.

Concerning H1, consistent with expectations, the coefficient β_1 in Model 2 is statistically significant and positive (β_1 = 0.139, p < 0.01), indicating that the change in spend between the pre-outbreak and outbreak periods was 13.9% higher for the treated product groups as compared to the control groups. Thus, *H1 is supported*, as the findings present evidence of stockpiling behavior after the onset of COVID-related issues in the fresh meat supply chain. Similarly, concerning H3, consistent with expectations, the coefficient β_2 is statistically significant and positive (β_2 = 0.114, p < 0.01) and indicates that the change in spend between the pre-outbreak and after periods was 11.4% higher for the treated product groups as compared to the control groups. Therefore, *H3 is also supported*, as there is evidence of stockpiling behavior continuing even after the shortage risk was subsiding. It is also evident from Model 2 that including CPI is important, as the coefficient indicates a near constant elasticity. As the CPI increases by 1%, there is a 0.86% increase in spend.

Lastly, I add the interactions in Model 3 (Equation 2). In looking at the coefficients on both interaction terms (β_3 and β_4), neither coefficient is significant ($\beta_4 = -0.001$, $p > 0.10$; $\beta_5 = -0.008$, $p > 0.10$). Therefore, there was no difference in the change in spend for the high or middle income groups as compared to the low income group during the outbreak period as compared to pre-outbreak. Therefore, *H2 and H2alt are not supported* in the primary analysis, indicating that each income cohort stockpiled to the same degree.

3.6 ROBUSTNESS TESTING

3.6.1 Income Cohort × Year × Week of Year Fixed Effects

I conducted several robustness tests. In the first, I include income cohort × year × week of year fixed effects. Doing so controls for factors that could have affected a particular income cohorts' purchasing patterns across all products at a given time. For example, government stimulus was dispersed in 2020, with the amount distributed to a given household dependent on income (Cochrane & Stolberg, 2020), leading to potential different effects on one income group as compared to another (i.e., one income cohort may be more likely to stockpile products overall at particular times). In adding income cohort × year × week of year fixed effects, the year × week of year fixed effects employed in the main analysis fall out. Results are presented in Table 3.5.

Table 3.5 Results from Robustness Test with Cohort × Year × Week of Year Fixed Effects

Outcome: LnSpend	Label	Model 1	Model 2
Intercept	β_0	4.285*** (7.64)	4.281*** (7.67)
Outbreak	β_1	0.139*** (6.77)	0.186*** (6.47)
After	β_2	0.114*** (5.55)	0.114*** (5.58)
LnCPI	β_3	0.864*** (7.20)	0.864*** (7.23)
Outbreak × High income	β_4		–0.107*** (–3.07)
Outbreak × Mid income	β_5		–0.032 (–0.91)
Product × week of year fixed effects	γ_{ik}	Included	Included
Product × income cohort fixed effects	λ_{ij}	Included	Included
Year × week of year fixed effects	α_{tk}		
Income cohort × year × week of year fixed effects	η_{jtk}	Included	Included
Observations		1,260	1,260

Notes: z-statistic in parentheses. *$p < .10$, **$p < .05$, ***$p < .01$.

In Model 1, which includes $LnCPI$, the coefficients for Outbreak and After do not change as compared to Model 2 in Table 3.4 given that these coefficients are not indexed by income and therefore are unaffected by the inclusion of income cohort × year × week of year fixed effects. Therefore, both *H1 and H3 are supported*. To evaluate H2, Model 2 adds the interaction terms between the Outbreak period and income cohorts. Like in the main analysis, β_5 is not significant (β_5 = –0.032, p > 0.10). However, β_4 is now negative and significant (β_4 = –0.107, p < 0.01). The coefficient indicates that the change in spend between the pre-outbreak and outbreak periods for the treated product groups compared to control products was 10.7% lower for the high income cohort than the low income cohort. Thus, the high income cohort stockpiled the affected products to a lower degree than the low income cohort, providing *partial support for H2alt*.

3.6.2 Normalized Spend as the Dependent Variable

The second robustness test employs an alternative dependent variable. Instead of utilizing the natural log of the dollars spent, the dependent variable becomes the spend normalized by product × income cohort. The alternative dependent variable is used given that the standard deviation of spend differs across products, and those with higher standard deviations influence the model to a larger degree (Wooldridge, 2013). Results are presented in Table 3.6, with Models 1 and 2 replicating the main results but with the normalized outcome variable, and Models 3 and 4 replicating the first robustness test (see Section 3.6.1) but with the normalized outcome variable.

Table 3.6 Results from Robustness Test with Normalized Spend as the Outcome Variable

Outcome: Normalized spend	**Label**	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Intercept	β_0	–18.797*** (–6.02)	–18.797*** (–6.02)	–18.565*** (–5.96)	–18.583*** (–5.99)
Outbreak	β_1	0.350*** (3.05)	0.468*** (3.28)	0.350*** (3.06)	0.615*** (3.86)
After	β_2	0.278** (2.42)	0.278** (2.43)	0.278** (2.43)	0.278** (2.44)
LnCPI	β_3	4.131*** (6.18)	4.131*** (6.18)	4.131*** (6.20)	4.131*** (6.23)
Outbreak × High income	β_4		–0.286* (–1.94)		–0.627*** (–3.24)
Outbreak × Mid income	β_5		–0.067 (–0.46)		–0.167 (–0.86)
Product × week of year fixed effects	γ_{ik}	Included	Included	Included	Included
Product × income cohort fixed effects	λ_{ij}	Included	Included	Included	Included
Year × week of year fixed effects	α_{tk}	Included	Included		
Income cohort × year × week of year fixed effects	η_{jtk}			Included	Included
Observations		1,260	1,260	1,260	1,260

Notes: z-statistic in parentheses. *$p < .10$, **$p < .05$, ***$p < .01$.

As seen in Table 3.6 Models 1 and 3, the same conclusions hold for H1 and H3 as previously established, but results are now interpreted in terms of standard deviations. *Supporting H1*, the coefficient β_1 is statistically significant and positive (β_1 = 0.350, p < 0.01) and indicates that the change in spend between the pre-outbreak and outbreak periods was 0.350 standard deviations greater for the treated product groups as compared to the control groups. Similarly, *in support of H3*, the coefficient β_2 is statistically significant and positive ($\beta_2 = 0.278$, $p < 0.05$) and indicates that the change in spend between the pre-outbreak and after periods was 0.278 standard deviations greater for the treated product groups as compared to the control groups.

Next, concerning H2 and H2alt, results are generally aligned with findings from previous tests. While β_5 is not significant in either Model 2 (year × week of year fixed effects; $\beta_5 = -0.067$, $p > 0.10$) or Model 4 (income cohort × year × week of year fixed effects; $\beta_5 = -0.167$, $p > 0.10$) with normalized spend as the outcome, β_4 is negative and marginally statistically significant (Model 2; $\beta_4 = -0.286$, $p < 0.10$) or significant (Model 4; $\beta_4 = -0.627$, $p < 0.01$) depending on the use of fixed effects. Given that there is reason to believe different overall stockpiling behavior across income cohorts as previously explained, which is accounted for in Model 4, the negative and significant β_4 thus provides *partial support for H2alt* and indicates the change in spend between the pre-outbreak and outbreak periods for the treated product groups compared to control products was 0.627 standard deviations lower for the high income cohort than the low income cohort. Accordingly, the low income cohort stockpiled to a greater degree than the high income cohort.

3.6.3 Altered Period Start Dates

3.6.3.2 Earlier Start Date for the Outbreak Period

The final robustness tests account for the concern that it is difficult to isolate when consumers became aware of the issues in the meat supply chain initially and when the issues were starting to subside. Therefore, given that Google Trends (2020) indicated a spike in interest—albeit smaller than that for the original start of the outbreak period—by April 13, I move up the start date of the outbreak period by one week, assigning the outbreak period to April 12, 2020, through June 6, 2020 (Week 16 – Week 23 of 2020). The after period still starts on June 7, 2020, but is extended by one week to match the duration of the outbreak period, thus ending on August 1, 2020 (Week 24 – Week 31 of 2020). The overall study period is therefore extended by two weeks (one week per year), resulting in a total of 1,302 observations (7 product categories, 3 income cohorts, 2 years, 31 weeks per year).

Results are presented in Table 3.7. Analysis is conducted with the original dependent variable (LnSpend) and with both year × week of year fixed effects (Models 1 and 2) and income cohort × year × week of year fixed effects (Models 3 and 4). Conclusions are consistent with previous analysis, indicating *support for H1 and H3*, and *partial support for H2alt*.

Table 3.7 Results from Robustness Test with Earlier Start Date for Outbreak Period (Weeks 16 – 23)

Outcome: LnSpend	**Label**	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Intercept	β_0	5.403*** (8.96)	5.403*** (8.96)	5.417*** (9.26)	5.413*** (9.31)
Outbreak	β_1	0.177*** (9.30)	0.190*** (7.88)	0.177*** (9.59)	0.231*** (8.78)
After	β_2	0.112*** (5.38)	0.112*** (5.37)	0.112*** (5.54)	0.112*** (5.58)
LnCPI	β_3	0.604*** (4.68)	0.604*** (4.68)	0.604*** (4.83)	0.604*** (4.86)
Outbreak × High income	β_4		–0.015 (–0.58)		–0.117*** (–3.56)
Outbreak × Mid income	β_5		–0.025 (–0.99)		–0.047 (–1.44)
Product × week of year fixed effects	γ_{ik}	Included	Included	Included	Included
Product × income cohort fixed effects	λ_{ij}	Included	Included	Included	Included
Year × week of year fixed effects	α_{tk}	Included	Included		
Income cohort × year × week of year fixed effects	η_{jtk}			Included	Included
Observations		1,302	1,302	1,302	1,302

Notes: z-statistic in parentheses. *$p < .10$, **$p < .05$, ***$p < .01$.

3.6.3.2 Delayed Start Date for the After Period

Lastly, I delay the start date of the after period to Week 26 of 2020 to address that consumers' awareness that issues were subsiding may have been delayed. As in the primary analysis, the outbreak period starts with week 17 of 2020 (Gallagher & Kirkland, 2020; Google Trends, 2020; Gray Television, 2020), but instead of only extending for seven weeks through week 23, I use nine weeks as the outbreak period (Week 17 – Week 25, April 19 – June 20, 2020) to delay the start of the after period by two weeks. I also then extend the after period to an equal timeframe of nine weeks (Week 26 – Week 34, June 21 – August 22, 2022). With the extension of the study period, the sample now consists of 34 weeks per year, resulting in a total of 1,428 observations (7 product categories, 3 income cohorts, 2 years, 34 weeks per year).

Results are presented in Table 3.8. Like with the previous robustness test, analysis is conducted with the original dependent variable (LnSpend) and with both year × week of year fixed effects (Models 1 and 2) and income cohort × year × week of year fixed effects (Models 3 and 4). Conclusions are consistent with previous analysis, indicating *support for H1 and H3*, and *partial support for H2alt*.

Table 3.8 Results from Robustness Test with Delayed Start Date for the After Period (Weeks 26 – 34)

Outcome: LnSpend	Label	Model 1	Model 2	Model 3	Model 4
Intercept	β_0	4.065*** (7.23)	4.065*** (7.23)	4.063*** (7.53)	4.059*** (7.57)
Outbreak	β_1	0.127*** (6.55)	0.139*** (5.82)	0.127*** (6.83)	0.183*** (7.14)
After	β_2	0.086*** (4.62)	0.086*** (4.62)	0.086*** (4.82)	0.086*** (4.85)
LnCPI	β_3	0.878*** (7.31)	0.878*** (7.31)	0.878*** (7.62)	0.878*** (7.67)
Outbreak × High income	β_4		–0.007 (–0.29)		–0.115*** (–3.71)
Outbreak × Mid income	β_5		–0.031 (–1.26)		–0.055* (–1.78)
Product × week of year fixed effects	γ_{ik}	Included	Included	Included	Included
Product × income cohort fixed effects	λ_{ij}	Included	Included	Included	Included
Year × week of year fixed effects	α_{tk}	Included	Included		
Income cohort × year × week of year fixed effects	η_{jtk}			Included	Included
Observations		1,428	1,428	1,428	1,428

Notes: z-statistic in parentheses. *p < .10, **p < .05, ***p < .01.

3.7 DISCUSSION

3.7.1 Theoretical Contributions

This research contributes to theory by extending the disruption-related stockpiling literature and scarcity literature in four primary ways. First, given that the literature regarding stockpiling during disruptions—especially during widespread disruptions that are not limited to a relatively small geographical area—is still in its early stages, I contribute by extending beyond its existing focus. In particular, I expand beyond the primary focus on only non-perishable products (e.g., Pan et al., 2023; Prentice et al., 2022; Yoshizaki et al., 2020) or both perishable and non-perishable goods with little to no distinction made among product groups (e.g., Keane & Neal, 2021; Papagiannidis et al., 2023) by focusing on a perishable product group, fresh meat. I find that consumers did stockpile during the disruptions of the meat supply chain in the U.S., indicating that scarcity increased the attractiveness of the products (Lynn, 1991) and led to consumers taking action to avoid future regret (Bell, 1982). This behavior was not necessarily certain *ex ante* given the perishability of the product and limited freezer space to store extra stockpiled goods (Food Logistics, 2020). While the general presence of stockpiling during disruptions has been supported in various contexts (e.g., Omar et al., 2021; Pan et al., 2020), confirming its presence even with perishable products contributes to the literature by exploring the applicability of theory to a different context (Makadok et al., 2018) and broadening the insights provided by the stockpiling literature.

Second, I contribute to the scarcity literature by responding to a call to draw on multiple theories to understand the impacts of scarcity (Shi et al., 2020). To do so, I explore the applicability of two perspectives: (1) the commodity theory perspective which highlights that limited availability of a product increases the attractiveness and value of a product (Brock, 1998; Lynn, 1991), and (2) the regret theory perspective which highlights that consumers make decisions to avoid future regret (Bell, 1982; Loomes & Sugden, 1982). In doing so, I explain how these perspectives combined explain the observed behavior of stockpiling of perishable products during and following a disruption; not only is the presence of scarcity itself important to drive interest, but also the desire to avoid regret, which is particularly important for low income households who have fewer alternative purchasing options (Moore & Diez Roux, 2006) and are more price sensitive (Wakefield & Inman, 2003) than high income households. Combining these theories helps to more fully understand the mechanisms (Astbury & Leeuw, 2010) behind the stockpiling behaviors I theorize and observe.

Third, as it relates to both the stockpiling during disruptions and the scarcity literature, I contribute by taking a longitudinal perspective. This contributes to the stockpiling literature given that the literature focused on stockpiling during widespread disruptions (e.g., COVID-19) has mostly taken a cross-sectional approach (e.g., Amaral et al., 2022; Herjanto et al., 2021; Omar et al., 2021). Taking a longitudinal perspective is important because (a) it allows causal identification to be established and (b) disruptions can evolve over time (Ahmadi et al., 2022b). Additionally, much of the scarcity literature exploring consumer behavior has studied behavior *during* a scarcity encounter by using behavioral experiments (e.g., Devlin et al., 2007; Peinkofer et al., 2016; Verhallen & Robben, 1994). Therefore, it was not clear how consumers would behave after the scarcity was subsiding and whether a scarcity would have longer-term impacts. I contribute to both areas by focusing on how a disruption-related scarcity of perishable products impacts consumer behavior not only during the scarcity but also afterwards and find that consumers continue displaying stockpiling behavior after a scarcity risk is subsiding. This is aligned with the theoretical mechanism (Astbury & Leeuw, 2010; Makadok et al., 2018) I suggested whereby the uncertainty (Altig et al., 2020) surrounding whether a scarcity would recur led to stockpiling (Papagiannidis et al., 2023) in order to guard against the uncertainty (Jung & Kellaris, 2004).

I lastly contribute to both the stockpiling and scarcity literatures by exploring the moderating impact of household income, which has shown conflicting results in the past (e.g., Ben Hassen et al., 2021; Kassas & Nayga, 2021; Wang et al., 2020). Thus, I contribute by reconciling these differences by establishing causal identification. In doing so, this research identifies income as a boundary condition (Makadok et al., 2018) of consumer stockpiling behavior as it relates to perishable products. Specifically, I find that low income households stockpiled in response to the scarcity to a greater degree, which I theorized was due to low income households being more price sensitive (Wakefield & Inman, 2003) and having less access to alternatives (Moore & Diez Roux, 2006), thereby seeking to avoid future regret (Bell, 1982) due to price increases (Lynn & Bogert, 1996) or unavailability.

3.7.2 Managerial Implications

This research also results in practical insights for retailers as well as their suppliers, both during a shortage of perishable products and during the recovery as the risk is subsiding. Such insights are important given an expectation of a continued environment of disruptions moving forward (Flynn et al., 2021). During the shortage risk, I firstly found that consumers do indeed stockpile; such behavior is not unique to only the commonly studied and discussed non-perishable products such as bottled water (Beatty et al., 2019; Pan et al., 2020) or toilet paper (Prentice et al., 2022; Yoshizaki et al., 2020). For both retailers and their suppliers, this knowledge is important to allow them to forecast demand more accurately with an impending or occurring disruption causing a shortage of perishable products. Further, for suppliers, this indicates the importance of avoiding or mitigating such disruption given the negative impact that consumer stockpiling can have such as unavailability of goods and inflated prices (Dammeyer, 2020).

Additionally, the finding that low income households stockpile to a greater degree than high income households also results in implications for retailers and their suppliers. For retailers, such an awareness of consumer behavior allows the retailer to more appropriately allocate and manage inventory within their network of stores. In particular, stores in low income areas may be more susceptible to stockpiling behavior given the larger presence of low income shoppers. For suppliers, similarly, they can better allocate inventory to their retail customers based on the demographics that the retailers serve. Such an understanding of consumer behavior will help both retailers and suppliers predict the challenges they will face throughout their network, thus enabling them to plan to mitigate the risks when a shortage of perishable products occurs.

After the shortage risk is subsiding, this research also points to key managerial implications. The finding that consumers do still tend to stockpile goods after the shortage risk subsides, which, to my knowledge, has not been previously demonstrated, informs retailers and suppliers regarding their path to recovering from the disruption. While not hypothesized, I tested for the ad hoc interaction effects between income and the after period. Aligned with the outbreak period, there is evidence that the low income cohort stockpiled to a greater degree as the risk subsided than the high income cohort. Again, like in the disruption period during the shortage, the awareness of stockpiling behavior as the disruption is subsiding will help retailers and their suppliers better plan ahead and forecast demand as they recover from the disruption. Specifically, it helps inform their rebuilding of inventory to know that demand will be heightened for some time, which prolongs their recovery to normal inventory conditions.

3.7.3 Limitations

As with any research, there are limitations to the present study. The first two relate to data limitations. Firstly, the transaction data is not consistent in reporting of the size of items purchased by consumers (e.g., some line items area reported in number of ounces purchased for example, while others lack units and are only reported as a count). Therefore, I was not able to utilize product quantity as an outcome given a lack of comparability among products within a product category. However, I was able to correct for this issue by including each product category's CPI, thereby controlling for changes in inflation over time. Second, I was unable to control for the presence of stockouts, which likely affected the volume of fresh meat products purchased during the study period given the meat supply chain disruptions (Law, 2020). However, if stockouts did reduce the volume of fresh meat purchases, the results would be conservative, and I would expect the magnitude of the effects to only have increased if I were able to control for the presence of stockouts. Thus, I am confident in the conclusions despite this limitation. The third limitation is that the timing for the outbreak and after periods cannot be definitively determined, as it is not possible to isolate exactly when consumers were aware of the shortage risk and when they perceived it to dissipate. I reduce the threat that this poses to conclusions by corroborating the start of the outbreak across several sources (Gallagher & Kirkland, 2020; Google Trends, 2020; Gray Television, 2020) and by conducting robustness tests related to the timeline (see Section 3.6.3).

3.7.4 Future Research

To build upon this study, there are several avenues for future research. First, while the outbreak of COVID-19 cases in the meat supply chain occurred after the initial COVID-19 disruption which impacted consumer behavior with an increase in spend across grocery items—as evidenced in Figure 3.3—the present study still took place in the context of a larger disruption given the pandemic was still ongoing (Katella, 2021). Thus, there is an opportunity to pursue further research regarding stockpiling behavior of perishable products outside of a larger environment of disruption. Second, researchers may explore stockpiling behavior across other perishable product categories given the effect of scarcity is heterogeneous (Shi et al., 2020). For the same reason, researchers may also consider studying additional predictors of stockpiling behavior in this context, including both consumer and retailer characteristics. Doing so would offer additional insights and establish additional boundary conditions—beyond income—of stockpiling behavior (Makadok et al., 2018). Third, it would be fruitful to explore how to influence stockpiling behavior—such as through methods of 'nudging' via communication (Kim et al., 2020)—in the context of perishable products during a shortage risk. Lastly, researchers could examine how other behaviors, such as the purchase of substitutes or changes in consumption, are affected by shortages of perishable products.

NOTE: Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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CHAPTER 4 – DOES THE CAUSE OF A STOCKOUT MATTER? INVESTIGATING THE IMPACT ON CONSUMER TRUST AND REPURCHASE INTENTIONS

4.1 INTRODUCTION

Over half of online shoppers in the United States (U.S.) indicate that stockouts affect their shopping behavior (Yltaevae, 2022), and in 2020, the cost of out-of-stocks in the U.S. was $225.7 billion (IHL Group, 2022). Recent heightened supply chain challenges during the COVID-19 pandemic—including upstream manufacturing disruptions (Naughton & Hufford, 2020), import transportation capacity challenges (Maiden, 2021), and changes in consumer shopping patterns (Bureau of Economic Analysis, 2022; Internal Revenue Service, 2021; Nassauer & Kapner, 2020)—have brought the topic of stockouts into more prevalent discussion and popular press coverage (e.g., Maloney & Terlep, 2022; Nassauer & Terlep, 2021; Scott & Kapner, 2021; Thomas, 2021). This increased prevalence, coupled with other recent consumer-facing supply chain issues such as long and/or uncertain e-commerce delivery times during the COVID-19 pandemic (Corkery & Maheshwari, 2020; Gandel, 2020), has led to consumers having a heightened awareness of supply chain issues (Shih, 2022).

Some retailers, therefore, have started to inform online consumers about the causes of stockouts. For example, Patagonia shared the following message with a backordered item: “Unfortunately, production and shipping delays due to the COVID-19 pandemic are temporarily affecting our inventory. Thank you for being patient as we work through these issues safely and with compassion for the people who make our clothes.” Additionally, Wayfair.com displayed the following message with an out-of-stock item: “We’re Sorry! We could not find a supplier that could fulfill your order…” Such actions lead to an interest in understanding whether such disclosures impact consumer reactions to stockouts. A nuanced understanding of the efficacy of such disclosures online is especially important given the expectation of a continued environment of disruption (Flynn et al., 2021), which leads to an expectation for continued stockout prevalence, making it important for retailers to understand how to mitigate negative effects.

The literature exploring how disclosing the cause of a stockout impacts consumer reactions to stockouts (e.g., Anderson et al., 2006; Kumar et al., 2021; Pizzi & Scarpi, 2013) is in its infancy and has explored limited stockout causes. The literature has also fallen short in providing insights regarding *why* consumers react in particular ways as it relates to disclosing the stockout cause, leading to a need to understand the underlying mechanisms (Astbury & Leeuw, 2010) as well as boundary conditions (Makadok et al., 2018). Accordingly, in this research I aim to understand how the disclosure of various stockout causes in an online retail setting affects consumers' repurchase intentions (RPI). This is important to explore given the increased expense associated with gaining online customers (Deighton, 2022), thus necessitating that retailers retain customers.

To understand the underlying mechanisms (Astbury & Leeuw, 2010) I explore trust as a mechanism given the importance of trust in the online retail setting (Gefen, 2000; Grabner-Kraeuter, 2002). Further, I study how gender moderates the effect of stockout cause on trust and subsequent RPI. The moderating effect is explored because (1) there has been a call in the supply chain management (SCM) literature to take a consumer-focused approach to SCM research to understand how different segments of consumers behave (Esper & Peinkofer, 2017) and (2) online trust is heterogeneous across individuals (Lee & Turban, 2001; Urban et al., 2009). Gender in particular is explored because retailers aim to tailor advertisements and information to individual consumers (Halzack, 2015), and demographic information is rather easy for retailers to capture—and subsequently tailor to—given the ability of online retailers to access consumers' browsing history (Morrison, 2019). In summary, the present study addresses the following research questions: *How does the disclosure of the cause of an online stockout influence consumer* *sentiments and behavior—trust and repurchase intentions (RPI), respectively? How does the effect vary by gender?*

Drawing on signaling theory (Connelly et al., 2011; Spence, 1973) and the impression formation literature (Kim et al., 2006; Paruchuri et al., 2021), I collected primary data via a series of four scenario-based experiments (Experiments 1, 2, 3A, and 3B) to explore the impact of various stockout causes—related to retailers' capabilities (Paruchuri et al., 2021) to varying degrees—on consumer trust and subsequent RPI. Scenario-based experiments are appropriate for this research given the desire to understand the why and how (Rungtusanatham et al., 2011) behind consumers' sentiments and behaviors. In Experiment 1, I examine the impact of disclosing two stockout causes—supplier issues and high demand, identified as upstream and downstream causes, respectively—as compared to not disclosing a stockout cause. Further, I explore whether the stockout cause signal is interpreted heterogeneously based on consumer gender.

Given the breadth of potential stockout causes (Moussaoui et al., 2016), in Experiment 2, I build upon Experiment 1 and the literature exploring the impact of broad stockout causes by examining the impact of four more contextualized operational (Moussaoui et al., 2016) stockout causes, as compared to not disclosing a stockout cause. Specifically, in Experiment 2, I focus on causes located upstream and at the focal firm to generate detailed insights for retailers. Lastly, in Experiments 3A and 3B, I look at the development of trust as a process (Urban et al., 2009) by measuring change in trust as the mechanism. While Experiments 1, 2, and 3A utilize a fictitious online retailer in the stockout scenario, representing a small, startup retailer (Peinkofer & Jin, 2023), Experiment 3B replicates Experiment 3A by looking at the effect for two well-known retailers. Doing so establishes external validity and an understanding of whether the effects are different for established retailers of different reputations, thus establishing reputation of the retailer as a boundary condition for the effect of disclosing stockout causes (Makadok et al., 2018).

This research contributes to both theory and practice. As it relates to theory, I build upon two streams of literature: (1) retail literature exploring how information disclosure can influence consumer reactions to stockouts and (2) literature studying trust in online retailing. Second, this research contributes by developing middle range theory (Pawson, 2000; Stank et al., 2017) by building upon signaling theory (Connelly et al., 2011; Spence, 1973) with impression formation literature (Kim et al., 2006; Paruchuri et al., 2021) and illustrating that signals have heteroengeous effects: in this case, the effect differs based on the stockout cause disclosed. Doing so helps create a foundation to understand other aspects of consumer behavior based on impressions and signals that relate to supply chain information. Lastly, this research contributes to theory by taking a consumer-focused approach to SCM research in order to understand how different segments of consumers behave, responding to a call from Esper and Peinkofer (2017) by exploring the role of gender as a potential boundary condition (Makadok et al., 2018).

As it relates to managerial implications, this research generates several insights for retailers—both small or startup retailers and established retailers. The findings reveal that there are opportunities to influence levels of trust and change in trust over time, subsequently influencing RPI in both positive and negative ways, by disclosing upstream and focal firm stockout causes to consumers. On the other hand, disclosing a downstream cause does not impact trust and subsequent RPI. In general, upstream causes pose an opportunity to generate higher levels of trust and subsequent RPI as compared to not disclosing a stockout cause, while focal firm causes can negatively impact trust levels and RPI. As it relates to change in trust over time, however, small or startup retailers as well as established retailers with positive reputations have opportunities to *build* trust even with focal firm-related causes; however, established retailers with weaker reputations can build trust or risk diminishing trust depending on the type of stockout cause disclosed. Lastly, a lack of significant results regarding the moderating role of gender indicates retailers do not need to cater their stockout cause disclosure strategy based on consumer gender.

The structure of this essay is as follows. I first review the relevant literature regarding influencing stockout reactions via retailer information disclosure as well as trust in an online setting. Second, I introduce the theoretical foundations, followed by the development of the hypotheses for Experiment 1. Next, I introduce Experiment 1, including the method, analysis, and results. Then, Experiment 2 hypotheses, methods, analysis, and results are presented, followed by Experiments 3A and 3B. Finally, this research concludes with theoretical and practical implications, limitations, and future research suggestions.

4.2 LITERATURE REVIEW

4.2.1 Consumer Responses to Stockouts and Mitigation Strategies

For many years, scholars have empirically studied how consumers react to stockouts in the retail environment (e.g., Emmelhainz et al., 1991; Peinkofer et al., 2015; Zinn & Liu, 2001). It is well-established that stockouts negatively impact immediate satisfaction (Kim & Lennon, 2011; Pizzi & Scarpi, 2013) and behaviors, leading consumers to, for example, switch stores or brands or delay their purchase (e.g., Campo et al., 2000; Zinn & Liu, 2001). Stockouts can also impact longer-term outcomes such as order amounts (Son et al., 2019), customer retention, or repurchase intentions (Dadzie & Winston, 2007; Fitzsimons, 2000; Jing & Lewis, 2011).

Due to the impact that stockouts can have on consumer perceptions and behaviors, scholars have examined how retailers can mitigate the negative repercussions of stockouts, such as through information disclosures (e.g., Breugelmans et al., 2006; Kumar et al., 2021; Peinkofer et al., 2016), offering a discount (Anderson et al., 2006; Kim & Lennon, 2011), suggesting substitutes (Breugelmans et al., 2006; Hoang & Breugelmans, 2022), or offering different fulfillment options in response to in-store stockouts (Peinkofer et al., 2022). As it relates to information disclosures, the literature has found, for example, that retailers can influence reactions to stockouts based on whether they disclose inventory availability information (Peinkofer et al., 2016), by altering the timing at which a stockout is disclosed (Breugelmans et al., 2006; Kim & Lennon, 2011), or by disclosing the cause of the stockout (Anderson et al., 2006; Ezhilkumar, 2020; Kumar et al., 2021; Peterson et al., 2020; Pizzi & Scarpi, 2013). I aim to contribute by building upon the literature which explores the effect of disclosing the stockout cause. Table 4.1 summarizes key literature.

Table 4.1 Key Literature Studying the Impact of Disclosing Stockout Cause

Citation	Stockout disclosure	Outcome variable(s)	Other key variables	Primary findings
Anderson et al. (2006)	• A supplier problem • Extreme popularity	• Short run impact (final disposition for out-of-stock item: canceled, shipped late but returned, shipped not returned) • Long run impact (4 measures of demand in 13 months post-stockout period)	• No mechanisms formally tested; anticipated length of delay informally examined • Moderators: price, store brand vs. not, consumers' prior purchase, other items in the order	• Disclosing popularity positively impacted short run outcomes • More favorable long run impacts were experienced for the high popularity condition, but mostly long run outcomes could not conclusively be studied
Pizzi and Scarpi (2013)	• High number of customer requests • Retailer's stock exhausted	• Decision satisfaction • Repatronage intention	• Other predictor (in addition to stockout cause): timing of stockout disclosure • Moderator: gender	• Accepting stockout responsibility via the stock exhaustion condition can neutralize the negative impacts of stockouts, if informed *ex ante* • No effect of gender
Ezhilkumar (2020)	• High consumer demand • Retailer's stock exhausted	• Product behavioral intent • Store behavioral intent	• Moderator: product type • Mechanisms: perceived uniqueness of product, consumption risk	• For utilitarian (hedonic) products, behavioral intent was higher when the stockout reason was consumer demand (retailer stock) related than retailer stock (consumer demand) related
Peterson et al. (2020)	Worded as: • "out-of-stock" • "sold out" • "unavailable"	• Behavioral intent • Perceived cause of outage • Perceived duration of outage • Product disappointment • Website disappointment • Bad luck • Visit website in future	• N/A; no mechanisms or moderators explored	• Framing of a stockout impacts consumer perceptions but not behavioral intentions • More positive perceptions result when the product is disclosed as being "sold out" rather than "out-of-stock"
Kumar et al. (2021)	• High demand • Short supply	• Product behavioral intent • Store behavioral intent	• Other predictor (in addition to stockout cause): sales level • Moderator: product type • Mechanisms: negative affect, perceived popularity, perceived uniqueness	• The impact on behavioral outcomes of revealing a stockout cause differs based on product type and whether sales information is provided • Revealing the stockout cause reduces intention to share negative word of mouth

I would like to highlight two key observations from Table 4.1 to detail how the present study builds upon the extant literature. First, the existing studies either do not explicitly state but infer the cause of the stockout (Peterson et al., 2020) or only explore a limited set of stockout causes. The causes explored are typically quite broad, such as short supply (Kumar et al., 2021), a supplier problem (Anderson et al., 2006), or high demand/popularity (Anderson et al., 2006;

Ezhilkumar, 2020; Kumar et al., 2021; Pizzi & Scarpi, 2013). The present study builds upon the literature by not only looking at the effects of some of the commonly explored stockout causes but also incorporating other more contextualized operational (Moussaoui et al., 2016) causes to generate more nuanced insights for retailers. In particular, I incorporate disruption-related causes at both the supplier and retailer levels given the expectation for continued supply chain disruptions moving forward (Flynn et al., 2021).

Second, as shown in Table 4.1, underlying mechanisms have only been formally examined in two of the key studies (Ezhilkumar, 2020; Kumar et al., 2021), and they tend to focus on product-related attributes (e.g., uniqueness, popularity). Additionally, though not formally testing the presence of mechanisms, a few studies within this literature have theorized that consumer responses are due to the effects of scarcity (e.g., Anderson et al., 2006; Ezhilkumar, 2020; Kumar et al., 2021). With the limited focus on exploring mechanisms, this literature has not successfully explored the "how" and "why" (Astbury & Leeuw, 2010) behind consumers' behavior in reaction to the disclosure of stockout causes. There are likely other mechanisms, beyond product attributes or scarcity, that can aid in understanding consumer responses to the disclosure of stockout causes. I, therefore, build upon the literature by exploring another factor—namely, trust—and its role as a mechanism. Exploring trust in particular contributes to the retail literature given the importance of trust in the online retail setting (Gefen, 2000; Grabner-Kraeuter, 2002), as further discussed in the next section.

4.2.2 Trust in Online Retailing

Trust is defined as existing "when one party has confidence in an exchange partner's reliability and integrity" (Morgan & Hunt, 1994, p. 23). The importance of trust in an online retail setting has been clearly established by several studies (e.g., Gefen, 2000; Grabner-Kraeuter, 2002;

Jin & Park, 2006; Li et al., 2012; van der Heijden et al., 2003). Specifically, trust has been shown to impact consumers' purchase intentions in an online setting (Schlosser et al., 2006). While trust is important in general in retail settings (e.g., Zboja & Voorhees, 2006), there is a special importance in the online setting given that consumers are unable to visually inspect a product for its quality prior to purchase (Chen & Dibb, 2010; Kim & Krishnan, 2015; Lee & Turban, 2001; Pavlou et al., 2007). Additionally, compared to brick-and-mortar shopping, it is easier for online consumers to switch to an alternate online retailer (Bhalla, 2020); by establishing trust, online retailers may help prevent switching.

Given the importance of trust in an online setting, the literature has explored how to influence trust in online retailing. Much of this literature has focused on how various website design features (e.g., Bart et al., 2005; Jin & Park, 2006) and company reputation can influence trust (e.g., Koufaris & Hampton-Sosa, 2004; Yoon, 2002). There has been a limited amount of literature also focused on how SCM information can influence trust. In particular, Wang et al. (2004) focus in part on return policy leniency and find that there is not a significant effect on trust, while Bart et al. (2005) explore the impact of order fulfillment experience and find that it is influential in some online shopping scenarios. Lastly, Peinkofer and Jin (2023) look at how disclosing fulfillment information influences trust in an online setting. The current research contributes to the online retailing trust literature by expanding the exploration of how SCM information can impact consumers' trust in an online retailer, with a specific focus on disclosing the cause of a stockout.

4.3 THEORETICAL FOUNDATION

To support this research, I draw from tenets of signaling theory (Connelly et al., 2011; Spence, 1973) and the impression formation literature (Kim et al., 2006; Paruchuri et al., 2021).

Signaling theory has been adopted in several supply chain and operations management studies (e.g., Mollenkopf et al., 2022; Rao et al., 2018; Wallenburg et al., 2021) and has also been supported by impression formation literature more recently, with calls for additional research doing so (Mollenkopf et al., 2022). Signaling theory, which has had key constructs and concepts developed in the economics and finance disciplines (Connelly et al., 2011; Riley, 2001), is concerned with how to reduce information asymmetry that exists between two parties (Spence, 1973, 2002): the signaler and the receiver (Connelly et al., 2011). The signaler has information that they may decide to share or not share—via a signal—with the receiver, who stands to benefit from receiving such information (Connelly et al., 2011). The current study looks at how the retailer (i.e., the signaler) can disclose the cause of a stockout (i.e., the signal) to provide information to the consumer (i.e., the receiver) to influence the consumer's reactions to stockouts, and whether the signal is interpreted differently based on a consumer characteristic, gender.

Disclosing of information (i.e., signaling) can impact the impression that, in this case, consumers form about a retailer (Elsbach, 1994). Thus, I augment the signaling theory literature with the impression formation literature, which notes that there are two fundamental dimensions upon which consumers will form impressions: the capability and integrity dimensions (Connelly et al., 2016; Mishina et al., 2012; Park & Rogan, 2019; Paruchuri et al., 2021). In this research, I look at how disclosing various stockout causes relating to various degrees to the retailer's capability dimension—which is related to an "ability to perform" (Paruchuri et al., 2021, p. 563)—impacts stockout reactions. Capabilities are important for establishing and impacting trust in a party (Kim et al., 2006; Paruchuri et al., 2021), and trust related to a party's capabilities is especially impactful in affecting purchase intentions (Schlosser et al., 2006).

4.4 THE INFLUENCE OF DISCLOSING STOCKOUT CAUSE: EXPERIMENT 1 HYPOTHESES

There are countless reasons that a stockout may occur (Ehrenthal & Stölzle, 2013; Moussaoui et al., 2016). Moussaoui et al. (2016) in particular delineate five types of drivers of product unavailability: behavioral, coordination, managerial, operational, and systemic. In this research, I focus on operational drivers, which encompass the vast majority of stockout causes (Moussaoui et al., 2016) and include factors such as upstream failures, backroom operation issues, and demand-supply mismatches. While operational stockout causes can be classified in many ways, I delineate causes based on where within the supply chain they are communicated to have occurred or been caused: upstream, at the focal firm (i.e., retailer), or downstream. I first focus on upstream and downstream events in Experiment 1 then expand to include focal firm issues as the stockout cause, focusing on focal firm and upstream causes in Experiments 2, 3A, and 3B.

I first explain the expected effect of disclosing the stockout as due to an upstream cause (e.g., a supplier issue) as compared to not disclosing the cause of the stockout. In general, when a stockout occurs, there is the presence of information asymmetry (Spence, 1973, 2002) as the retailer may be aware of the cause of the stockout (Moussaoui et al., 2016), but such information is not typically available to the consumer. A retailer can choose to disclose the reason, thereby reducing information asymmetry, which can impact consumer perceptions (Connelly et al., 2011). By disclosing a cause that is due to an issue at an upstream supply chain partner, the consumer receives information signaling that the cause of the stockout is due to a capability failure of the supplier, not the retailer. In comparison, when no stockout cause is disclosed, consumers may think that the stockout is due to a capability failure at the retailer. While based on the concept of chain liability the retailer will still be held responsible to some degree for failures upstream (Hartmann et al., 2022; Hartmann & Moeller, 2014), the reflection on the retailer's capabilities is expected to be lower in the case of disclosing the cause as due to an upstream issue as compared to not disclosing the cause of the stockout.

Given that capabilities are a key dimension upon which impressions are formed (Connelly et al., 2016; Mishina et al., 2012; Park & Rogan, 2019; Paruchuri et al., 2021) and upon which trust is based (Kim et al., 2006; Paruchuri et al., 2021), I expect that the consumer's trust in the retailer will be of a higher level for those that encounter a stockout indicated as due to an upstream issue as compared to no reason. This is expected because the upstream cause reflects less negatively on the retailer's capabilities. Based on a higher level of trust, I expect subsequent higher RPI given the importance of trust in an online retailing setting (e.g., Gefen, 2000; Grabner-Kraeuter, 2002; Jin & Park, 2006; Li et al., 2012; van der Heijden et al., 2003) and that trust is an important predictor of behavior whereby high levels of trust result in positive consumer behaviors (e.g., Eastlick & Lotz, 2011; Gefen, 2000). Therefore, I hypothesize:

H1: As compared to not disclosing the stockout cause, disclosing an upstream stockout cause will result in higher RPI via higher levels of trust.

Next, I focus on the effect of disclosing the stockout as due to a downstream cause (e.g., high demand) as compared to not disclosing the cause of the stockout. As in the development of H1, in the face of a stockout a retailer can choose to disclose the stockout cause, thereby reducing information asymmetry, which can impact consumer perceptions (Connelly et al., 2011). When disclosing a stockout as due to a downstream cause, consumers again receive information signaling that the cause of the stockout is not due to a capability failure at the retailer, but rather a failure/issue further down the supply chain. On the other hand, when no stockout cause is disclosed, consumers may infer that the stockout is due to a capability failure at the retailer. Such positive impact from disclosing a stockout as due to a downstream cause is supported by Anderson et al. (2006) who find that disclosing product popularity (i.e., high demand) as a stockout cause positively impacted short- and long-term outcomes.

Again, as capabilities are important for impacting trust (Kim et al., 2006; Paruchuri et al., 2021), I expect that the consumer's trust in the retailer will be of a higher level for those that encounter a stockout indicated as due to a downstream issue as compared to no reason disclosed since the downstream cause will not reflect negatively on the retailer's capabilities. As in H1, based on higher trust, I expect subsequent higher RPI given the importance of trust in an online retailing setting (e.g., Gefen, 2000; Grabner-Kraeuter, 2002; Jin & Park, 2006; Li et al., 2012; van der Heijden et al., 2003) and that high levels of trust result in positive consumer behaviors (e.g., Eastlick & Lotz, 2011; Gefen, 2000). Accordingly, I hypothesize:

H2: As compared to not disclosing the stockout cause, disclosing a downstream stockout cause will result in higher RPI via higher levels of trust.

Lastly for Experiment 1, I explain the expected moderating effect of consumer gender, with a focus on how gender moderates the relationship between stockout cause and trust, ultimately impacting RPI. This is important to explore to understand whether the stockout cause signals are interpreted differently by different consumers, which may be catered to by online retailers (Halzack, 2015; Morrison, 2019). There are two reasons why I expect gender will moderate the relationship between stockout cause disclosure and trust. Firstly, the presence of trust in an online setting is heterogeneous across individuals (Lee & Turban, 2001; Urban et al., 2009). Second, males and females process information differently (Chang, 2007; Meyers-Levy, 1989; Meyers-Levy & Maheswaran, 1991; Meyers-Levy & Sternthal, 1991). For example, men process information selectively, while women integrate details (Chang, 2007; Meyers-Levy &

Maheswaran, 1991). Thus, I expect males and females will interpret the signal of stockout cause disclosure differently.

Combining the expectation that males and females will interpret signals differently and the fact that trust is heterogeneous across individuals (Lee & Turban, 2001; Urban et al., 2009), I expect that the relationship between stockout cause disclosure and trust will be moderated by gender. In particular, the literature has shown that females tend to be less trusting than males (Rodgers & Harris, 2003). Accordingly, I anticipate a negative moderating effect in that the impact of disclosing the stockout cause on trust will be more negative for females as compared to males. Thus, I hypothesize:

H3: The association between stockout cause—as either an (a) upstream stockout cause or (b) a downstream stockout cause—and RPI will be lower for females as compared to males via lower levels of trust in the retailer.

4.5 RESEARCH METHODOLOGY INTRODUCTION

4.5.1 Development of Experimental Scenario

To develop the scenario for the experiments, I followed the process prescribed by Rungtusanatham et al. (2011). During the pre-design stage, I examined several retailers' websites to identify current practices in terms of disclosing stockout causes. Further, based on the extant literature, I identified measures and selected a product—a tablet—for the scenario. Electronics account for one of the most shopped-for products in an online setting (Shopify, 2022) and is plagued by high stockout rates (Adobe Communciations Team, 2021), making electronics a suitable product category in line with previous studies (e.g., Peinkofer et al., 2015, 2016, 2022; Pizzi & Scarpi, 2013). In the next stage, I developed the common and experimental cue modules (Rungtusanatham et al., 2011). The common module simulated an out-of-stock encounter while shopping online for a tablet at fictitious retailer Electronics.com (Experiments 1, 2, and 3A, to control for brand effects) or one of two real online retailers (Experiment 3B), while the experimental module incorporated the manipulations of the stockout causes.

4.5.2 Experimental Procedures Common Across All Experiments

Several procedures remained consistent across all experiments. Firstly, I recruited participants using Amazon Mechanical Turk (MTurk) via CloudResearch (formerly known as TurkPrime; Litman et al., 2017). I used MTurk based on the precedent of its use in consumer research in SCM (e.g., Peinkofer et al., 2022; Tokar et al., 2020) and based on research which has indicated utilizing MTurk results in samples of equal to or higher quality than when students are utilized (Kees et al., 2017) and of higher quality than when other consumer panels are utilized[15] (Berry et al., 2022; Kees et al., 2017). Within CloudResearch, I specified several criteria to be met for workers to qualify for participation. First, I required that workers be located in the U.S. and, following recommendations from Chandler et al. (2015), did not allow workers to participate in more than one of the experiments for this study. Further, workers needed to have participated in at least 100 approved HITs[16], have a HIT approval rate of at least 96% (Peer et al., 2014), and be indicated as a Cloud Research approved participant (Berry et al., 2022), meaning that they have achieved CloudResearch's measure requirements for attention and engagement (Litman et al., 2022). I also blocked duplicate IP addresses and any workers with suspicious geocodes (Berry et al., 2022).

In each experiment, participants received compensation of $1.00 for their time. Participants were randomly assigned to only one condition and were exposed to the scenario before variables

[15] To obtain higher quality responses than professional panels, vetted MTurk participants must be used that are approved by CloudResearch (Berry et al., 2022), as is done in this study.
[16] HITs (human intelligence tasks) are tasks on MTurk (Litman et al., 2017).

of interest were collected. Then, manipulation checks were included. Realism of the scenario was also assessed using two seven-point Likert scale questions to understand how realistic the scenario was and to what degree participants could imagine themselves in the situation. Two attention checks were included in each experiment, one at the beginning and one at the end of the experiment. Table 4.2 summarizes the experimental procedures for each of the experiments.

Table 4.2 Experimental Procedures

	Experiment 1	Experiment 2	Experiment 3A	Experiment 3B
Compensation	\$1.00	\$1.00	\$1.00	\$1.00
Average completion time	7 minutes 15 seconds	5 minutes 16 seconds	7 minutes 37 seconds	8 minutes 43 seconds
Initial sample	174	232	227	440
Removed due to failed attention check	6	8	6	21
Removed due to failed manipulation check	20	21	20	37
Final sample size	148	203	201	382
Mean age (range)	39 years (21 – 72)	40 years (20 – 73)	41 years (22 – 79)	44 years (20 – 85)
Gender (female / male / other)	47% / 53% / 1%	36% / 64% / 0%	49% / 51% / 0%	57% / 43% / 1%
Median household income	\$50,000 – \$59,999	\$40,000 – \$49,999	\$50,000 – \$59,999	\$50,000 – \$59,999
% with some college	90.5%	88.2%	88.6%	89.8%
Realism checks ratings range	6.37 – 6.47	5.70 – 6.63	5.84 – 6.75	5.90 – 6.70

4.6 EXPERIMENT 1

4.6.1 Manipulations

Experiment 1 involved a 3-level (stockout cause: upstream, downstream, no cause disclosed) between-subjects design. Appendix A contains an overview of the scenario and manipulations for Experiment 1. As the stockout causes for the upstream and downstream scenarios, I adapted causes explored in previous literature including a supplier problem (e.g., Anderson et al., 2006), specifically "reduced shipments from suppliers" as the upstream cause and a consumer driven cause (e.g., Pizzi & Scarpi, 2013) of "high consumer demand" as the downstream cause. To check the stockout cause manipulation, I followed Wang et al. (2022) and incorporated a single-item question inquiring about the cause of the stockout. I conducted a two-way contingency table analysis to check the manipulation, and, per Miller et al. (2002), the validity of the stockout cause manipulation was supported ($\chi^2 = 229.95$, $p < 0.001$, Cramer's V = 0.83). Participants who did not correctly identify their respective experimental condition were removed to ensure data was of high quality (Abbey & Meloy, 2017).

4.6.2 Measures and CFA

Table 4.3 contains a summary of the scale items used in this research. As the dependent variable, I adopted a measure of repurchase intentions (RPI) from Hui et al. (2004) consisting of three 7-point semantic differentials to assess whether the consumer would consider patronizing the online retailer in the future. Trust in the online retailer (Trust), the mediating variable, was assessed using a measure adopted from Sirdeshmukh et al. (2002) consisting of four 7-point semantic differentials assessing consumers' perceptions of the retailers' dependability and competence. The moderator, Gender, was collected via a multiple-choice question in which participants indicated gender as male, female, or other. In the tests including the moderator, Gender was coded as 0 for male participants and 1 for female participants.[17]

[17] Responses for participants who indicated gender as 'other' were excluded from the analysis for the moderating effect of gender.

Table 4.3 Summary of Scale Items

Measure	Items	Used in Experiments
Trust in online retailer (Trust)[18]	I feel that Electronics.com (Retailer B, Retailer C) is: Trust1: 1 = very undependable; 7 = very dependable Trust2: 1 = very incompetent; 7 = very competent Trust3: 1 = of very low integrity; 7 = of very high integrity Trust4: 1 = very unresponsive to customers; 7 = very responsive to customers	1, 2, 3A, 3B
Repurchase intention (RPI)	Overall, how likely are you to buy from the website of the retailer Electronics.com (Retailer B, Retailer C) again: RPI1: 1 = unlikely; 7 = likely RPI2: 1 = definitely no; 7 = definitely yes RPI3: 1 = not inclined to; 7 = inclined to	1, 2, 3A, 3B
Loyalty	*1 = strongly disagree; 7 = strongly agree* Loyal1: I seldom switch from Retailer B(C) to another retail website. Loyal2: As long as the present service continues, I doubt that I would switch from Retailer B(C) to a different retail website. Loyal3: I try to use Retailer B(C) whenever I need to make a purchase. Loyal4: Whenever I need to make a purchase, Retailer B(C) is my first choice. Loyal5: I like using Retailer B(C). Loyal6: To me Retailer B(C) is the best retail website to do shopping with. Loyal7: I believe that Retailer B(C) is my favorite retail website.	3B
Quality	In comparison to other retail websites' products, I believe that the products sold on Retailer B(C) are: Quality1: 1 = low quality; 7 = high quality Quality2: 1 = inferior; 7 = superior Quality3: 1 = bad; 7 = good Quality4: 1 = worse than others; 7 = better than others	3B

I conducted a confirmatory factor analysis (CFA) using Mplus with a two-factor model consisting of RPI and Trust. Results are shown in Table 4.4. The CFA fit statistics support the two-factor model (Hu & Bentler, 1999): $\chi^2 = 17.877$, $df = 13$, CFI = 0.995, RMSEA = 0.050 (90% confidence interval: 0.000; 0.102), and SRMR = 0.021. Supporting convergent validity, each factor's average variance extracted (AVE) exceeds 0.5 (Fornell & Larcker, 1981), and supporting construct validity, each factor's composite reliability exceeds 0.7 (Fornell & Larcker, 1981). I find support for discriminant validity by comparing the AVE for each of the factors to the phi-square correlation (ϕ^2) of the pair of factors. With a correlation coefficient of 0.653 (RPI with Trust), the

[18] In Experiments 3A and 3B, trust is measured both before and after the stockout scenario and operationalizes change in trust (ΔTrust).

AVE for each factor exceeded ϕ^2 for the pair (Fornell & Larcker, 1981). Aligned with best practices, I utilized Mplus to extract factor scores for the constructs (Calantone et al., 2017) and included the factor scores in the analysis. Including factor scores is preferred over including the average of scale items for each construct as factor scores represent a weighted aggregate of the scale items, whereas including a simple average treats all items as weighted equally (Aiken & West, 1991; Edwards & Wirth, 2009).

Table 4.4 Experiment 1 CFA Results

Measure	Measure Description	Standardized Path Loading	Mean	SD	Composite Reliability	AVE
Trust	Trust1	0.805	3.743	1.305	0.903	0.701
	Trust2	0.870	4.182	1.341		
	Trust3	0.809	4.399	1.051		
	Trust4	0.862	4.068	1.184		
RPI	RPI1	0.924	4.169	1.583	0.958	0.884
	RPI2	0.944	4.061	1.467		
	RPI3	0.953	4.088	1.615		

χ^2 = 17.877, *df* = 13, CFI = 0.995, RMSEA = 0.050 (90% confidence interval: 0.000; 0.102), and SRMR = 0.021

4.6.3 Analysis and Results

To test the hypotheses for each experiment, I used PROCESS in SPSS, which are macros based on ordinary least squares (OLS) regression path analysis (Hayes, 2022). Each hypothesis was tested with 20,000 bootstrap samples (Hayes, 2022). To test H1 and H2, I used PROCESS Model 4 (Hayes, 2022) with stockout cause as the predictor, RPI as the outcome, and trust as the mediator. Stockout cause was entered as a multicategorical predictor with the control group (i.e., no stockout cause disclosed) as the reference group (i.e., coded as 0).

Full regression results for Experiment 1 are presented in Table 4.5. The construction of the indirect effects for H1 and H2 is presented in Table 4.6. *Lacking support for H1*, results indicate a nonsignificant indirect effect on RPI through trust of disclosing the stockout as due to an upstream cause (i.e., reduced shipments from suppliers) versus not disclosing a stockout cause (effect size = 0.122, CI = [–0.189, 0.399]). Also *lacking support for H2*, results indicate that there is not a significant indirect effect on RPI via trust of disclosing the stockout as due to a downstream cause (i.e., high consumer demand) versus not disclosing a cause (effect size = –0.084, CI = [–0.298, 0.141]). Taken together, when retailers disclose an upstream or downstream stockout cause, it neither positively nor negatively impacts RPI via trust as compared to not disclosing the cause.[19]

Table 4.5 Experiment 1 Regression Results

	Panel A: PROCESS Model 4			
	Model 1		**Model 2**	
	Trust	**p-value**	**RPI**	**p-value**
Intercept	0.028 (0.132)	0.833	0.078 (0.096)	0.419
Upstream cause	0.159 (0.198)	0.422	–0.106 (0.145)	0.468
Downstream cause	–0.119 (0.186)	0.522	–0.125 (0.136)	0.360
Trust			0.705 (0.061)	0.000
F-value(df)	0.993(2)	0.373	45.918(3)	0.000
R^2	0.014		0.489	
	Panel B: PROCESS Model 8			
	Model 1		**Model 2**	
	Trust	**p-value**	**RPI**	**p-value**
Intercept	0.038 (0.183)	0.834	0.059 (0.134)	0.663
Upstream cause	0.213 (0.269)	0.429	–0.156 (0.197)	0.429
Downstream cause	–0.059 (0.263)	0.826	–0.039 (0.193)	0.841
Gender	–0.022 (0.266)	0.934	0.041 (0.195)	0.833
Upstream cause × Gender	–0.176 (0.406)	0.666	0.123 (0.297)	0.679
Downstream cause × Gender	–0.118 (0.376)	0.755	–0.173 (0.275)	0.531
Trust			0.705 (0.062)	0.000
F-value(df)	0.478(5)	0.792	22.515(6)	0.000
R^2	0.017		0.491	

Note: standard errors are reported in parentheses.

[19] Necessary sample size to detect mediation effects was determined according to guidelines in Fritz and MacKinnon (2007).

Table 4.6 Experiment 1 Indirect Effects

X (Stockout condition)	$\theta_{(X_i \to Trust)} = a_1$	$\theta_{(Trust \to RPI)} = b_1$	Indirect effect size $(a_1 * b_1)$	Bootstrapped confidence interval	Significant
Upstream cause (H1)	0.159	0.705	0.112	(–0.189, 0.399)	No
Downstream cause (H2)	–0.119	0.705	–0.084	(–0.298, 0.141)	No

H3 was tested using PROCESS Model 8 (Hayes, 2022) with stockout cause as the predictor, RPI as the outcome, and trust as the mediator. Gender was entered as a first stage moderator, with male coded as the reference group. The indices of moderated mediation are presented in Table 4.7. *Lacking support for H3*, the indices of moderated mediation are not significant in the case of either the stockout as due to an upstream (index of moderated mediation = –0.124, CI = [–0.762, 0.472]) or downstream cause (index of moderated mediation = –0.083, CI = [–0.524, 0.372]). This indicates that the association between stockout cause and RPI via trust is not significantly different for females versus males.

Table 4.7 Experiment 1 Indices of Moderated Mediation

X (Stockout condition)	$\theta_{(X_i * Gender \to Trust)} = a_1$	$\theta_{(Trust \to RPI)} = b_1$	Index of moderated mediation $(a_1 * b_1)$	Bootstrapped confidence interval	Significant
Upstream cause (H3a)	–0.176	0.705	–0.124	(–0.762, 0.472)	No
Downstream cause (H3b)	–0.118	0.705	–0.083	(–0.524, 0.372)	No

4.6.4 Experiment 1 Discussion

Overall, Experiment 1 built on the existing literature by looking at how the disclosure of a stockout cause impacts RPI by way of trust, as the mediating role of trust has not yet been examined and is of importance for online retailers (e.g., Gefen, 2000; Grabner-Kraeuter, 2002; Jin & Park, 2006; Li et al., 2012; van der Heijden et al., 2003). In summary, I do not find support for the hypotheses in Experiment 1, which were all tested utilizing a fictious retailer representing a small or startup online retailer. Firstly, in either situation—an upstream or downstream stockout cause—disclosing the cause of a stockout did not serve as a signal (Connelly et al., 2011) to differentially impact consumers' behavioral reactions to stockouts by way of impacting the level of trust in the retailer, as compared to not disclosing a cause. Thus, I find that there was not a significant impact on impressions formed by the consumers as a result of the signaling of capabilities from the retailer. This finding is important given that retailers have been implementing the practice of disclosing the stockout cause in some situations. Though it did not positively benefit the retailer, I find that it also did not negatively influence the sentiments and behaviors of consumers, indicating that the act of stockout cause disclosure was not harmful in the proposed stockout scenarios (reduced shipments and high demand).

While some previous studies have found that different framing of stockouts impacts behavioral intentions (e.g., Anderson et al., 2006; Ezhilkumar, 2020; Kumar et al., 2021), this may indicate that stockout disclosure can serve as a signal of scarcity but may not serve as a signal of trust—at least with the stockout causes explored in Experiment 1. However, it is possible that other stockout causes may impact consumers' trust and subsequent RPI. In particular, both reduced shipments from suppliers (i.e., the upstream cause explored) and high consumer demand (i.e., the downstream cause explored) might be seen as within a retailer's influence to some degree, resulting in a lack of differentiable reflection upon the retailer's capabilities as compared to the control scenario (no cause disclosed). For instance, supplier issues could be affected by retailer-supplier relationships (Lee et al., 2000), while stockouts due to high demand could result from a retailer's poor forecasting (Moussaoui et al., 2016). Therefore, as will be explained, in Experiment 2 I explore stockout causes that may be more clearly related to a particular party's capabilities.

In addition to the general finding that disclosing the stockout cause as due to an upstream or downstream cause did not impact RPI via trust as compared to not disclosing the cause, I also find that the interpretation of the stockout cause and its impact on trust did not differ across genders (i.e., females and males interpreted the stockout signals in the same way). This result aligns with findings from Pizzi and Scarpi (2013) regarding the lack of moderating effects of gender. Thus, both female and male consumers are forming impressions of the retailer's capabilities in the same way. This is an important finding for retailers as it indicates that there is not a difference in the way the different consumer segments (Esper & Peinkofer, 2017) should be catered to in regard to disclosing stockout causes.

4.7 EXPERIMENT 2

Given the lack of significant findings in Experiment 1, I explore four more contextualized stockout causes in Experiment 2. In doing so, I devise middle range theory (Pawson, 2000; Stank et al., 2017) via top-down theoretical contextualization (Craighead et al., 2016). Specifically, drawing on elements of signaling theory (Connelly et al., 2011; Spence, 1973) and impression formation literature (Kim et al., 2006; Paruchuri et al., 2021), I contextualize (Johns, 2006) these theories to the setting of stockouts in online retailing. Given the lack of significant results as it relates to the moderating effect of gender, I do not focus on or hypothesize about such effects in Experiments 2, 3A, or 3B, but as will be noted, I test for the ad hoc effects of gender (see Appendix B).

First, I consider two additional upstream stockout causes: (a) an earthquake in Asia and (b) COVID-19 cases at the supplier's warehouse. While there was a lack of significance for H1 which examined an upstream stockout cause as well (reduced shipments from suppliers), I expect that both of the upstream scenarios in Experiment 2 will indicate an even lesser degree of relation to the retailer's capabilities. This is because both earthquakes and infectious disease cases are to a large degree beyond the retailer's influence, whereas, as mentioned, supplier issues could be affected by retailer-supplier relationships (Lee et al., 2000). Thus, I expect a lower degree of chain liability (Hartmann et al., 2022; Hartmann & Moeller, 2014) assigned to the retailer as a result of signaling the stockout reason as due to either an earthquake or upstream COVID-19 cases, as compared to not disclosing the stockout cause. Therefore, based on the lack of tie to the retailer's capabilities, the impression (Connelly et al., 2016; Mishina et al., 2012; Park & Rogan, 2019; Paruchuri et al., 2021) and subsequent trust (Kim et al., 2006; Paruchuri et al., 2021) placed in the retailer by the consumer should be more positive in the case of disclosing such stockout causes. This trust should accordingly lead to higher RPI (e.g., Eastlick & Lotz, 2011; Gefen, 2000) as explained in the argument for H1. Therefore, I hypothesize:

H4: As compared to not disclosing the stockout cause, disclosing the stockout cause as due to either (a) an earthquake in Asia or (b) COVID-19 cases at the supplier will result in higher RPI via higher levels of trust.

Next, instead of focusing on downstream causes as in H2, which are unlikely to become more detailed than a variation of "high consumer demand" (e.g., high popularity, a large number of customer requests), I focus on the impact of disclosing two stockout causes which reside within the focal firm and are highly relevant in a disruption context: (a) a labor shortage at the retailer's warehouse and (b) COVID-19 cases at the retailer's warehouse. As previously noted, disclosing a stockout cause allows the retailer to reduce information asymmetry (Spence, 1973, 2002), but I expect that disclosing a cause that is due to an event at the focal firm will reflect poorly on the retailer's capabilities (i.e., will indicate a capability failure), as compared to not disclosing the stockout cause. Thus, disclosing a cause at the focal firm should negatively impact the impressions that consumers form of the retailer (Paruchuri et al., 2021). Aligned with previous reasoning, such view of the retailer's capabilities is expected to impact trust (Kim et al., 2006; Paruchuri et al., 2021), in this case negatively as compared to not disclosing the stockout cause. Based on a lower level of trust, I expect subsequent lower RPI for the same reasons as argued previously: trust is an important predictor of behavior whereby low levels of trust result in negative consumer behaviors (e.g., Eastlick & Lotz, 2011; Gefen, 2000). Accordingly, I hypothesize:

H5: As compared to not disclosing the stockout cause, disclosing the stockout cause as due to either (a) a labor shortage at the retailer's warehouse or (b) COVID-19 cases at the retailer's warehouse will result in lower RPI via lower levels of trust.

4.7.1 Manipulations

Experiment 2 involved a 5-level (stockout reason: earthquake, COVID-19 cases at the supplier's warehouse, a labor shortage at the retailer's warehouse, COVID-19 cases at the retailer's warehouse, no cause disclosed) between-subjects design. Appendix C contains an overview of the scenario and manipulations for Experiments 2, 3A, and 3B. To check the stockout cause manipulation, I again followed Wang et al. (2022) and incorporated a single-item question inquiring about the cause of the stockout. I conducted a two-way contingency table analysis to check the manipulation, and, per Miller et al. (2002), the validity of the stockout cause manipulation was supported ($\chi^2 = 709.78$, $p < 0.001$, Cramer's V = 0.89). Participants who did not correctly identify their respective experimental condition were removed to ensure data was of high quality (Abbey & Meloy, 2017).

4.7.2 Measures and CFA

Experiment 2 contained the same measures as in Experiment 1: the dependent variable (RPI) was adopted from Hui et al. (2004), the mediator (Trust) was adopted from Sirdeshmukh et al. (2002), and Gender was measured using a multiple choice question in which participants indicated gender as male, female, or other. Consistent with Experiment 1, I conducted a CFA using

Mplus with a two-factor model consisting of RPI and Trust. Results are presented in Table 4.8. The CFA fit statistics support the two-factor model (Hu & Bentler, 1999): $\chi^2 = 28.410$, $df = 13$, CFI = 0.990, RMSEA = 0.076 (90% confidence interval: 0.038; 0.115), and SRMR = 0.027. Convergent validity, construct validity, and discriminant validity[20] were all supported (Fornell & Larcker, 1981). Again aligned with best practice, factor scores for the constructs (Calantone et al., 2017) were extracted from Mplus and included in the analysis.

Table 4.8 Experiment 2 CFA Results

Measure	Measure Description	Standardized Path Loading	Mean	SD	Composite Reliability	AVE
Trust	Trust1	0.843	4.030	1.328	0.926	0.757
	Trust2	0.909	4.291	1.386		
	Trust3	0.867	4.448	1.325		
	Trust4	0.859	4.409	1.315		
RPI	RPI1	0.975	4.680	1.567	0.961	0.892
	RPI2	0.928	4.562	1.448		
	RPI3	0.930	4.557	1.725		

$\chi^2 = 28.410$, $df = 13$, CFI = 0.990, RMSEA = 0.076 (90% confidence interval: 0.038; 0.115), and SRMR = 0.027

4.7.3 Analysis and Results

Experiment 2 hypotheses were tested in the same way as Experiment 1 via PROCESS in SPSS using PROCESS Model 4 (Hayes, 2022). Full regression results for Experiment 2 are presented in Table 4.9, and the construction of the indirect effects is presented in Table 4.10. *In support of H4a*, results indicate a significant positive indirect effect on RPI through trust of disclosing the stockout cause as due to an earthquake versus not disclosing a cause (effect size = 0.379, CI = [0.105, 0.696]). Similarly, *in support of H4b*, results show a significant positive indirect effect on RPI via trust of disclosing the stockout cause as due to COVID-19 cases at the

[20] With a correlation coefficient of 0.718 (RPI with Trust), the AVE for each factor exceeded ϕ^2 for the pair (Fornell & Larcker, 1981).

supplier versus not disclosing a cause (effect size = 0.275, CI = [0.018, 0.534]). Taken together, *support for H4a and H4b* indicates that when retailers disclose the stockout as due to an upstream cause of either an earthquake or COVID-19 cases at the supplier, it has a significant positive impact on RPI—thereby benefitting the retailer—via higher trust, as compared to not disclosing the cause of the stockout.

Table 4.9 Experiment 2 Regression Results

	PROCESS Model 4			
	Model 1		Model 2	
	Trust	**p-value**	**RPI**	**p-value**
Intercept	–0.091 (0.138)	0.512	0.057 (0.100)	0.570
Earthquake	0.522 (0.196)	0.008	–0.045 (0.144)	0.755
COVID-19 cases at supplier	0.379 (0.198)	0.057	0.069 (0.145)	0.637
Labor shortage	–0.561 (0.194)	0.004	–0.226 (0.143)	0.115
COVID-19 cases at warehouse	0.198 (0.198)	0.320	0.007 (0.144)	0.960
Trust			0.727 (0.051)	0.000
F-value(df)	9.517(4)	0.000	53.182(5)	0.000
R^2	0.161		0.574	

Note: standard errors are reported in parentheses.

Table 4.10 Experiment 2 Indirect Effects

$\boldsymbol{X}$ (Stockout condition)	$\boldsymbol{\theta}_{(X_i \rightarrow Trust)} = \boldsymbol{a_1}$	$\boldsymbol{\theta}_{(Trust \rightarrow RPI)} = \boldsymbol{b_1}$	Indirect effect size ($\boldsymbol{a_1} * \boldsymbol{b_1}$)	Bootstrapped confidence interval	Significant
Earthquake (H4a)	0.522	0.727	0.379	(0.105, 0.696)	Yes
COVID-19 cases at supplier (H4b)	0.379	0.727	0.275	(0.018, 0.534)	Yes
Labor shortage (H5a)	–0.561	0.727	–0.408	(–0.703, –0.136)	Yes
COVID-19 cases at retailer (H5b)	0.198	0.727	0.144	(–0.118, 0.402)	No

Turning next to H5a, results indicate a significant negative indirect effect on RPI through trust of disclosing the stockout as due to a labor shortage at the retailer's warehouse as compared to not disclosing the stockout cause (effect size = –0.408, CI = [–0.703, –0.136]). This indicates that when retailers disclose the stockout as being due to a focal firm issue of a labor shortage, it has negative implications for retailers of lower RPI via lower levels of trust, as compared to not disclosing the cause of the stockout. *Lacking support for H5b*, the results indicate that there is no significant indirect effect on RPI through trust of disclosing the stockout as being due to a focal firm issue of COVID-19 cases at the retailer's warehouse versus not disclosing a cause (effect size = 0.144, CI = [–0.118, 0.402]). This indicates that when retailers disclose the stockout cause, if it is due to a focal firm issue related to COVID-19 cases, it neither benefits nor harms retailers as compared to not disclosing the stockout cause.

Although I did not hypothesize regarding the moderating effect of gender (given the nonsignificant results from Experiment 1), I tested for first-stage moderating effects in Experiment 2. Like in Experiment 1, there were not significant moderated mediation effects (see Appendix B).

4.7.4 Experiment 2 Discussion

By exploring more contextualized stockout causes, several notable insights were generated in Experiment 2. Firstly, in both cases of upstream stockout causes (an earthquake in Asia and COVID-19 cases at the supplier), the retailer positively benefited—as compared to not disclosing the stockout cause—via higher levels of trust and subsequent higher levels of RPI; thus, the stockout cause disclosure served as a signal (Connelly et al., 2011) and generated positive impressions via a lack of negative reflection upon the retailer's capabilities (Paruchuri et al., 2021). This indicates that it can be beneficial for retailers to be specific in disclosing upstream causes that indicate a lack of connection to the retailers' capabilities. Recall that no effect was found for the upstream cause in Experiment 1—reduced shipments from suppliers—indicating such disclosure did not serve as a signal (Connelly et al., 2011). This may be due to consumers being more apt to relate reduced shipments from suppliers to an underlying cause that could be influenced to a greater degree by the retailer's capabilities (as compared to the causes explored in Experiment 2, an earthquake in Asia and COVID-19 cases at the supplier) (Kim et al., 2006; Paruchuri et al., 2021), thus attributing greater chain liability (Hartmann et al., 2022; Hartmann & Moeller, 2014) and having a greater influence on impressions (Paruchuri et al., 2021) and subsequent trust (Kim et al., 2006; Paruchuri et al., 2021).

As it relates to the causes at the focal firm, as predicted, in the case of a labor shortage at the retailer's warehouse (H5a), lower levels of trust—as compared to the control of no cause disclosed—resulted in lower RPI. This indicates that disclosing such cause served as a signal (Connelly et al., 2011) and reflected a capability failure (Paruchuri et al., 2021), leading to the conclusion that retailers should not disclose such issues in an effort to positively influence consumers' stockout reactions. However, the lack of support for H5b—disclosing a stockout as due to COVID-19 cases at the retailer's warehouse—indicates that such disclosure did not serve as a signal (Connelly et al., 2011) and there is no benefit nor harm associated with disclosing such cause. This is likely due to consumers perceiving COVID-19 cases as less related to retailers' capabilities than labor shortages. Taken together, based on Experiment 2, retailers should consider disclosing the cause of stockouts only when related to upstream causes, especially those that are clearly disconnected from the retailer.

4.8 EXPERIMENT 3

In Experiments 3A and 3B, I build upon Experiment 2 to understand whether there are differences in how trust is built (or diminished) over time across the stockout conditions by looking at *change* in trust as the mechanism. While Experiments 1 and 2 focused on theorizing regarding between-subject effects, Experiments 3A and 3B focus on within-subject effects. This is motivated by the idea that trust development is seen as a process (Urban et al., 2009), so considering existing trust prior to a stockout is important to fully understand the impact of disclosing the stockout cause.

My expectation is that the directions of H4 and H5 will hold but instead of having a positive or negative impact on the level of trust, the change in trust will be more positive or negative, respectively, as compared to not disclosing the stockout cause. Given the similarity to previous arguments, I briefly describe H6 and H7.

First, as it relates to disclosing the upstream stockout causes (an earthquake in Asia or COVID-19 cases at the supplier), I expect the change in trust to be more positive—as compared to not disclosing the stockout cause—given that disclosing such causes reduces information asymmetry (Spence, 1973, 2002) and should not negatively reflect on the retailer's capabilities (i.e., should not indicate a capability failure). Both of these reasons should increase trust by forming positive impressions (Kim et al., 2006; Paruchuri et al., 2021). Following such increase in trust, I expect that there will be higher RPI in the case of disclosing the upstream cause, compared to not disclosing, given that positive change in trust has been shown to relate to positive RPI (Peinkofer & Jin, 2023). I accordingly hypothesize:

H6: As compared to not disclosing the stockout cause, disclosing the stockout cause as due to either (a) an earthquake in Asia or (b) COVID-19 cases at the supplier will result in higher RPI via an increase in trust.

Lastly, I hypothesize regarding the expected effect of disclosing focal firm causes (a labor shortage or COVID-19 cases at the retailer's warehouse) on change in trust and subsequent RPI, as compared to not disclosing a stockout cause. In accordance with H5, I expect disclosing such causes will negatively reflect upon retailers' capabilities, as compared to not disclosing the cause, thus negatively impacting impressions formed of the retailer (Paruchuri et al., 2021) and reducing trust (Kim et al., 2006; Paruchuri et al., 2021). I expect such decrease in trust will reduce RPI (Peinkofer & Jin, 2023), and I hypothesize:

H7: As compared to not disclosing the stockout cause, disclosing the stockout cause as due to either (a) a labor shortage at the retailer's warehouse or (b) COVID-19 cases at the retailer's warehouse will result in lower RPI via a decrease in trust.

4.8.1 Experiment 3A

4.8.1.1 Manipulations

Experiment 3A involved a mixed factorial design with a 5-level between-subjects manipulation of stockout cause (stockout reason: earthquake, COVID-19 cases at the supplier's warehouse, a labor shortage at the retailer's warehouse, COVID-19 cases at the retailer's warehouse, no cause disclosed) and a within-subjects measure of change in trust (ΔTrust). Aligned with Peinkofer and Jin (2023), to collect the initial measure of Trust, participants were first presented with a baseline scenario which contained the retailer's logo and were asked to assess their feelings toward that retailer (Electronics.com). After collecting the initial measure for Trust, participants were randomly assigned to one experimental condition. To check the stockout cause manipulation, I incorporated a single-item question inquiring about the cause of the stockout (Wang et al., 2022). I conducted a two-way contingency table analysis to check the manipulation, and, per Miller et al. (2002), the validity of the stockout cause manipulation was supported (χ^2 = 706.90, $p < 0.001$, Cramer's V = 0.89). Participants who did not correctly identify their respective experimental condition were removed to ensure data was of high quality (Abbey & Meloy, 2017).

4.8.1.2 Measures and CFA

Experiment 3A contained the same dependent and moderator variables as in Experiments 1 and 2: RPI (Hui et al., 2004) and Gender, respectively. The mediating variable is now change in trust (ΔTrust), which is still adopted from Sirdeshmukh et al. (2002) but measured both before and after encountering the stockout scenario. To assess change in trust, I employed a difference score

($\Delta Trust_{jt} = Trust_{j2} - Trust_{j1}$) (Allison, 1990; Peinkofer & Jin, 2023). Results from a two-factor CFA model of RPI and ΔTrust are presented in Table 4.11. The CFA fit statistics support the two-factor model (Hu & Bentler, 1999): $\chi^2 = 57.916$, $df = 13$, CFI = 0.986, RMSEA = 0.093 (90% confidence interval: 0.069; 0.118), and SRMR = 0.014. Convergent validity, construct validity, and discriminant validity[21] were all supported (Fornell & Larcker, 1981). Factor scores for the constructs (Calantone et al., 2017) were extracted from Mplus and included in the analysis.

Table 4.11 Experiment 3A CFA Results

Measure	Measure Description	Standardized Path Loading	Mean	SD	Composite Reliability	AVE
ΔTrust	ΔTrust1	0.879	4.371	1.371	0.938	0.790
	ΔTrust2	0.917	4.687	1.420		
	ΔTrust3	0.890	4.699	1.299		
	ΔTrust4	0.869	4.607	1.360		
RPI	RPI1	0.968	4.070	1.735	0.970	0.915
	RPI2	0.942	4.065	1.562		
	RPI3	0.959	3.945	1.777		

$\chi^2 = 57.916$, $df = 13$, CFI = 0.986, RMSEA = 0.093 (90% confidence interval: 0.069; 0.118), and SRMR = 0.014

4.8.1.3 Analysis and Results

Full regression results for Experiment 3A, tested using PROCESS Model 4 (Hayes, 2022), are presented in Table 4.12, and the construction of the indirect effects is presented in Table 4.13. *In support of H6a*, results indicate a significant positive indirect effect on RPI through change in trust of disclosing the stockout cause as due to an earthquake versus not disclosing a cause (effect size = 0.260, CI = [0.082, 0.474]). This indicates that retailers have an opportunity to *increase* or *build* a consumer's trust by disclosing such upstream cause. However, *lacking support for H6b*, there is not a significant indirect effect on RPI through change in trust of disclosing the stockout

[21] With a correlation coefficient of 0.532 (RPI with ΔTrust), the AVE for each factor exceeded ϕ^2 for the pair (Fornell & Larcker, 1981).

cause as due to COVID-19 cases at the supplier versus not disclosing (effect size = 0.043, CI = [–0.149, 0.222]). This indicates that there is no benefit (nor harm) from the perspective of *change* in a consumer's level of trust of disclosing the cause of COVID-19 cases upstream as compared to not disclosing the cause of the stockout.

Table 4.12 Experiment 3A Regression Results

	PROCESS Model 4			
	Model 1		**Model 2**	
	ΔTrust	**p-value**	**RPI**	**p-value**
Intercept	–0.701 (0.151)	0.000	0.114 (0.128)	0.377
Earthquake	0.608 (0.215)	0.005	0.486 (0.178)	0.007
COVID-19 cases at supplier	0.101 (0.223)	0.650	0.081 (0.180)	0.655
Labor shortage	0.054 (0.221)	0.808	–0.098 (0.179)	0.584
COVID-19 cases at warehouse	0.452 (0.218)	0.040	0.159 (0.178)	0.373
ΔTrust			0.427 (0.058)	0.000
F-value(df)	3.017(4)	0.019	16.454(5)	0.000
R^2	0.058		0.297	

Note: standard errors are reported in parentheses.

Table 4.13 Experiment 3A Indirect Effects

$\boldsymbol{X}$ **(Stockout condition)**	$\boldsymbol{\theta_{(X_i \to \Delta Trust)} = a_1}$	$\boldsymbol{\theta_{(\Delta Trust \to RPI)} = b_1}$	**Indirect effect size** $(\boldsymbol{a_1 * b_1})$	**Bootstrapped confidence interval**	**Significant**
Earthquake (H6a)	0.608	0.427	0.260	(0.082, 0.474)	Yes
COVID-19 cases at supplier (H6b)	0.101	0.427	0.043	(–0.149, 0.222)	No
Labor shortage (H7a)	0.054	0.427	0.023	(–0.181, 0.244)	No
COVID-19 cases at warehouse (H7b)	0.452	0.427	0.193	(0.029, 0.378)	Yes

Turning next to *H7a, lacking support*, there is not a significant indirect effect on RPI through change in trust of disclosing the stockout as due to labor issues at the retailer's warehouse as compared to not disclosing the stockout cause (effect size = 0.023, CI = [–0.181, 0.244]). This indicates that retailers cannot influence RPI via either building or diminishing trust by disclosing such focal firm cause. Contrary to the predictions for H7b, the results show that there is a significant but *positive* indirect effect on RPI via change in trust of disclosing the stockout cause as being due to COVID-19 cases at the retailer's warehouse versus not disclosing a cause (effect size = 0.193, CI = [0.029, 0.378]). This indicates that when retailers disclose the stockout cause, if it is due to focal firm COVID-19 cases, it positively benefits the retailer through higher RPI via an increase in trust as compared to not disclosing the cause. Thus, although this effect was not significant in Experiment 2 which examined between-subject effects and the *level* of trust, retailers can *change* (build) trust by disclosing such cause.

Again, although I did not hypothesize regarding the moderating effect of Gender (given the lack of significant results from Experiment 1), I tested for moderating effects and found no significant results (see Appendix B).

4.8.2 Experiment 3B

To generate additional insights and establish external validity, I replicated Experiment 3A with two real retailer names as an additional manipulation. Experiments 1, 2, and 3A utilized a fictitious retailer (Electronics.com), representing a startup or small online retailer (Peinkofer & Jin, 2023). Using well-known, established retailers—particularly two with differing reputations—may heed different insights given that a retailer's reputation can influence the level of trust that a consumer has in the retailer (Koehn, 2003). Therefore, I next tested H6 and H7 using well-known retailers: Retailer B and Retailer C, whose identities remain masked. As detailed in the pretest for Experiment 3B (see Appendix D), Retailer B is regarded as having more high-quality products and has more loyal customers than Retailer C. Retailer B is a largely an online retailer and is seen as a leader in e-commerce, while Retailer C is an omnichannel retailer that more recently has strengthened its online services.

4.8.2.1 Manipulations

Experiment 3B involved a mixed factorial design with a 5 (stockout reason: earthquake, COVID-19 cases at the supplier's warehouse, a labor shortage at the retailer's warehouse, COVID-19 cases at the retailer's warehouse, no cause disclosed) x 2 (retailer name: Retailer B, Retailer C) between-subjects manipulation and a within-subjects measure of change in trust (ΔTrust). Participants were randomly assigned to one experimental condition. Again aligned with Peinkofer and Jin (2023), to collect the initial measure of Trust, participants were presented with a baseline scenario which contained the retailer's logo and were asked to assess their feelings toward that retailer. To check the stockout cause manipulation, I again incorporated a single-item question inquiring about the cause of the stockout (Wang et al., 2022). Similarly, I included a single-item question inquiring about the retailer from which they were purchasing to check the retailer manipulation. I conducted two-way contingency table analyses to check the manipulations. Per Miller et al. (2002), the validity of the stockout cause ($\chi^2 = 1351.05$, $p < 0.001$, Cramer's V = 0.90) and retailer (419.00, $p < 0.001$, Phi = 1.00)[22] manipulations were supported. Participants who did not correctly identify their respective experimental condition were removed to ensure data was of high quality (Abbey & Meloy, 2017).

4.8.2.2 Measures and CFA

Experiment 3B contained the same measures as in Experiment 3A: RPI, ΔTrust, and Gender. Additionally, Loyalty and Quality (see Table 4.3) were included as controls given the use of real retailer names (Peinkofer & Jin, 2023). The CFA results are presented in Table 4.14. For Experiment 3B, a four-factor model was used consisting of RPI, ΔTrust, Loyalty, and Quality. The CFA fit statistics support the four-factor model (Hu & Bentler, 1999): $\chi^2 = 929.693$, $df = 129$, CFI

[22] In accordance with Miller et al. (2002), given that this contingency table was 2x2, Phi was used in place of Cramer's V (which is used for all that exceed the size of 2x2).

= 0.955, RMSEA = 0.090 (90% confidence interval: 0.085; 0.096), and SRMR = 0.053. Convergent validity, construct validity, and discriminant validity[23] were all supported (Fornell & Larcker, 1981). Factor scores for the constructs (Calantone et al., 2017) were extracted from Mplus and included in the analysis.

Table 4.14 Experiment 3B CFA Results

Measure	Measure Description	Standardized Path Loading	Mean	SD	Composite Reliability	AVE
ΔTrust	ΔTrust1	0.934	4.945	1.620	0.934	0.781
	ΔTrust2	0.949	5.093	1.564		
	ΔTrust3	0.793	4.637	1.556		
	ΔTrust4	0.849	4.906	1.582		
RPI	RPI1	0.978	5.026	1.718	0.977	0.934
	RPI2	0.969	4.987	1.679		
	RPI3	0.952	4.901	1.818		
Loyalty	Loyal1	0.765	3.780	1.975	0.959	0.771
	Loyal2	0.850	3.814	2.036		
	Loyal3	0.922	3.872	2.024		
	Loyal4	0.919	3.937	2.128		
	Loyal5	0.770	5.060	1.687		
	Loyal6	0.948	3.969	2.004		
	Loyal7	0.952	3.767	2.096		
Quality	Qual1	0.938	4.437	1.338	0.958	0.851
	Qual2	0.943	4.338	1.255		
	Qual3	0.898	4.764	1.341		
	Qual4	0.911	4.364	1.209		

χ^2 = 929.693, *df* = 129, CFI = 0.955, RMSEA = 0.090 (90% confidence interval: 0.085; 0.096), and SRMR = 0.053

[23] With correlation coefficients of 0.699 (RPI with ΔTrust), 0.666 (RPI with Loyalty), 0.588 (RPI with Quality), 0.588 (ΔTrust with Loyalty), 0.604 (ΔTrust with Quality), and 0.695 (Loyalty with Quality), the AVE for each factor exceeded ϕ^2 for the pair.

4.8.2.3 Analysis and Results

Full regression results for Experiment 3B, tested via PROCESS Model 4 (Hayes, 2022), are presented in Table 4.15 by retailer (Retailer B in Panel A, Retailer C in Panel B). The construction of the indirect effects is presented in Table 4.16. For the upstream stockout causes (H6a and H6b), results for both of the established, well-known retailers align with those from Experiment 3A for the fictitious retailer representing a small or startup online retailer. Specifically, there is *support for H6a* (Retailer B effect size = 0.079, CI = [0.012, 0.182]; Retailer C effect size = 0.115, CI = [0.027, 0.223]) and a *lack of support for H6b* (Retailer B effect size = 0.038, CI = [–0.036, 0.131]; Retailer C effect size = 0.055, CI = [–0.021, 0.166]) for both Retailer B and Retailer C. This indicates that RPI can be positively influenced via a larger increase in trust, as compared to not disclosing the stockout cause, by disclosing the stockout as due to an earthquake in Asia. There was, however, no effect of disclosing the stockout as due to COVID-19 cases at the supplier.

Table 4.15 Experiment 3B Regression Results

	Panel A: Retailer B PROCESS Model 4				Panel B: Retailer C PROCESS Model 4			
	Model 1		Model 2		Model 1		Model 2	
	Trust	**p-value**	**RPI**	**p-value**	**Trust**	**p-value**	**RPI**	**p-value**
Intercept	–0.642 (0.109)	0.000	–0.121 (0.116)	0.299	–0.528 (0.110)	0.000	0.219 (0.133)	0.101
Earthquake	0.351 (0.143)	0.015	0.311 (0.143)	0.031	0.404 (0.147)	0.007	0.006 (0.170)	0.972
COVID-19 cases at supplier	0.168 (0.145)	0.249	0.404 (0.143)	0.005	0.194 (0.148)	0.192	–0.174 (0.168)	0.302
Labor shortage	–0.069 (0.142)	0.627	0.299 (0.140)	0.034	–0.354 (0.143)	0.014	–0.094 (0.165)	0.569
COVID-19 cases at warehouse	0.314 (0.150)	0.037	0.239 (0.148)	0.109	–0.104 (0.151)	0.492	–0.219 (0.171)	0.203
ΔTrust			0.226 (0.072)	0.002			0.284 (0.084)	0.001
Quality	0.054 (0.066)	0.416	0.087 (0.065)	0.183	–0.160 (0.072)	0.028	0.433 (0.083)	0.000
Loyalty	0.040 (0.073)	0.583	0.601 (0.072)	0.000	0.096 (0.087)	0.270	0.352 (0.099)	0.001
F-value(df)	2.381(6)	0.031	24.362(7)	0.000	5.759(6)	0.000	20.450(7)	0.000
R^2	0.071		0.480		0.160		0.442	

Note: standard errors are reported in parentheses.

Table 4.16 Experiment 3B Indirect Effects

X (Stockout condition)	$\boldsymbol{\theta}_{(X_i \rightarrow \Delta Trust)} = \boldsymbol{a_1}$	$\boldsymbol{\theta}_{(\Delta Trust \rightarrow RPI)} = \boldsymbol{b_1}$	**Indirect effect size ($\boldsymbol{a_1 * b_1}$)**	**Bootstrapped confidence interval**	**Significant**
Earthquake (H6a)					
Retailer B	0.351	0.226	0.079	(0.012, 0.182)	Yes
Retailer C	0.404	0.284	0.115	(0.027, 0.223)	Yes
COVID-19 cases at supplier (H6b)					
Retailer B	0.168	0.226	0.038	(–0.036, 0.131)	No
Retailer C	0.194	0.284	0.055	(–0.021, 0.166)	No
Labor shortage (H7a)					
Retailer B	–0.069	0.226	–0.016	(–0.087, 0.062)	No
Retailer C	–0.354	0.284	–0.101	(–0.226, –0.016)	Yes
COVID-19 cases at warehouse (H7b)					
Retailer B	0.314	0.226	0.071	(0.004, 0.175)	Yes
Retailer C	–0.104	0.284	–0.030	(–0.139, 0.048)	No

As it relates to the focal-firm related stockout causes, differences exist between Retailers B and C. Results for Retailer B are consistent with that found in Experiment 3A for the fictitious retailer (Electronics.com). For Retailer B (that which is seen as having higher quality products and more loyal customers), *lacking support for H7a*, there is not a significant indirect effect on RPI through change in trust of disclosing the stockout as due to labor issues at the retailer's warehouse as compared to not disclosing the stockout cause (effect size = –0.016, CI = [–0.087, 0.062]). Additionally, contrary to expectations, there is a significant but *positive* indirect effect of RPI via change in trust of disclosing the stockout cause as being due to COVID-19 cases at the retailer's warehouse versus not disclosing a cause (effect size = 0.071, CI = [0.004, 0.175]). Therefore, established retailers with high quality and loyalty have an opportunity to build trust by disclosing a stockout as due to COVID-19 cases as their warehouse.

Contrary to results for Electronics.com and Retailer B, *H7a was supported for Retailer C* (effect size = –0.101, CI = [–0.226, –0.016]). Thus, established retailers viewed as having lower quality products and less loyal customers can negatively impact RPI through a decrease in trust by disclosing a stockout as due to a labor shortage as compared to not disclosing the cause. Like for Electronics.com and Retailer B, there was a *lack of support for H7b* in Experiment 3B, but instead of finding the opposite (positive) effect as in Experiment 3A, no significant indirect effect was found (effect size = –0.030, CI = [–0.139, 0.048]). This indicates there is no impact on RPI via a change in trust by disclosing a stockout cause as due to COVID-19 cases at the retailer's warehouse—as compared to not disclosing the cause—for Retailer C.

Again, although I did not hypothesize regarding the moderating effect of Gender, I tested for moderating effects and found no significant results (see Appendix B).

4.8.3 Experiment 3 Discussion

Experiments 3A and 3B built upon Experiment 2 by looking at the impact of stockout cause disclosure on RPI via *change* in trust, with Experiment 3A focused on the same fictitious retailer, representing a startup or small online retailer, while Experiment 3B replicated 3A with two well-known retailers. A summary of results from Experiments 3A and 3B is shown in Table 4.17. While results were largely consistent across the three retailers, there are a couple of key differences. First, it is noteworthy to point out that it is possible for an established retailer that is viewed as carrying lower quality products and having less loyal customers (Retailer C) to experience a decrease in trust. Thus, such retailers should be cognizant that disclosing some information as it relates to stockout causes can serve as a signal (Connelly et al., 2011) that negatively impacts consumer behaviors. This is true specifically in the scenario which disclosed a stockout cause of a labor shortage at the retailer's warehouse. This indicates that retailers of which consumers already have negative perceptions—in terms of quality and loyalty—can harm consumers' sentiments and behaviors by disclosing causes that are relatively within their influence.

Table 4.17 Summary of Results from Experiments 3A and 3B

Stockout condition	**Electronics.com** (Experiment 3A)	**Retailer B** (Experiment 3B)	**Retailer C** (Experiment 3B)
Earthquake	Positive indirect effect	Positive indirect effect	Positive indirect effect
COVID-19 cases at supplier	No indirect effect	No indirect effect	No indirect effect
Labor shortage	No indirect effect	No indirect effect	Negative indirect effect
COVID-19 cases at warehouse	Positive indirect effect	Positive indirect effect	No indirect effect

Second and in contrast, small or startup retailers (Electronics.com) as well as established retailers with better reputations (Retailer B) only stand to *increase* trust through disclosing the cause of a stockout (or have no significant indirect effect, when evaluating ΔTrust as the mediator). In particular, disclosing the stockout cause of an earthquake or COVID-19 cases at the retailer's warehouse both serve as positive signals (Connelly et al., 2011) for these retailers. Given the importance of trust in online retailing (e.g., Zboja & Voorhees, 2006) and the challenge in establishing it (Bhalla, 2020; Chen & Dibb, 2010; Kim & Krishnan, 2015; Pavlou et al., 2007), this is important as it demonstrates that these retailers can take deliberate actions to build trust with consumers. The different findings in this regard points to the importance of retailer reputation: a greater degree of chain liability (Hartmann et al., 2022; Hartmann & Moeller, 2014) is placed on established retailers with lower reputations, as compared to established retailers with better reputations and startup retailers.

4.9 IMPLICATIONS, LIMITATIONS, AND FUTURE RESEARCH

4.9.1 Theoretical Insights and Implications

This research contributes theoretically in three primary ways. First, I build upon two areas of retail literature: (1) literature exploring how retailer information disclosure can influence consumer reactions to stockouts and (2) literature studying trust in online retailing. As it relates to the former, while there is a foundation of literature studying how to influence consumer reactions to stockouts via information disclosure (e.g., Breugelmans et al., 2006; Kumar et al., 2021; Peinkofer et al., 2016), I contribute by examining more contextualized stockout causes than previously examined and by exploring how disclosing such causes can influence trust and subsequent behavioral reactions. Thus, I contribute by understanding the underlying mechanisms (Makadok et al., 2018). I find that in some cases (i.e., an upstream earthquake or COVID-19 cases at the supplier) it is beneficial for a retailer to disclose a stockout cause by resulting in higher levels of trust and subsequent RPI as compared to not disclosing the cause, while other times (i.e., in the case of a focal firm labor issue) disclosing the stockout cause negatively impacts trust levels and RPI. Additionally, retailers can build—but also break, in the case of less reputable established retailers—trust by disclosing the cause of stockouts. By examining the impacts for startup retailers versus two different established retailers with different reputations, I establish retailer reputation as a boundary condition (Makadok et al., 2018) for the effect of disclosing the stockout cause.

As it relates to the second literature stream, focused on trust in online retailing, I contribute by further exploring how trust in online retailing can be influenced by SCM information. This literature is in its infancy and has explored only limited SCM-related influences on trust, including return policy (Wang et al., 2004), order fulfillment experience (Bart et al., 2005), and fulfillment information (Peinkofer & Jin, 2023). I thus expand the literature by focusing on another aspect of SCM information, the disclosure of stockout causes. Further, I explore not only the impact of disclosing stockout cause on levels of trust but also on change in trust to further understand how retailers can develop trust over time (Peinkofer & Jin, 2023). With both literature streams, I also contribute by responding to calls to study issues that are of interest to industry (Richey & Davis-Sramek, 2022), which is evident given the focus on stockouts in the popular press (e.g., Maloney & Terlep, 2022; Nassauer & Terlep, 2021; Scott & Kapner, 2021; Thomas, 2021).

Second, this research contributes to theory by building upon signaling theory (Connelly et al., 2011; Spence, 1973) with impression formation literature (Kim et al., 2006; Paruchuri et al., 2021). Specifically, I develop middle range theory (Pawson, 2000; Stank et al., 2017) via top-down theoretical contextualization (Craighead et al., 2016) and contextualize (Johns, 2006) these theories to the setting of online stockouts. In doing so, I illustrate that signals have heteroengeous effects: in this case, the effect differs based on the stockout cause disclosed, which impact impact impressions in different ways. Further, I respond to a recent call to utilize the impression formation literature to further understand consumers' use of SCM-related signals (Mollenkopf et al., 2022). Doing so helps create a foundation to understand other aspects of consumer behavior based on impressions and signals that relate to supply chain issues.

Lastly, this research contributes to theory by taking a consumer-focused approach to SCM research in order to understand how different segments of consumers behave, thus responding to a call from Esper and Peinkofer (2017). To do so, I focus on how gender moderates the relationship between stockout causes and trust, subsequently impacting RPI. By finding no significant moderated mediation effects (see Table 4.7 and Appendix B), I find that both females and males respond to/interpret the signals related to the disclosure of stockout causes in the same way. Given that retailers in recent years have focused on tailoring advertisements and information to individual consumers (Halzack, 2015), this importantly indicates that gender is not a relevant boundary condition (Makadok et al., 2018) to consider in the case of stockout cause disclosure.

4.9.2 Managerial Implications

There are also important managerial implications resulting from this research. Firstly, the results help to inform retailers' messaging regarding online stockouts by understanding when it is beneficial and harmful to disclose the cause of a stockout. This is important given the negative effects that stockouts can have on consumer behavior (Campo et al., 2000; Kim & Lennon, 2011; Pizzi & Scarpi, 2013; Zinn & Liu, 2001); since stockouts are likely to continue being a reality given the prominence (IHL Group, 2022; Maloney & Terlep, 2022; Nassauer & Terlep, 2021; Scott & Kapner, 2021; Thomas, 2021) and an expected continued environment of disruption (Flynn et al., 2021), mitigating the harmful effects thus continues to be a priority. While the causes explored in Experiment 1 (reduced shipments from suppliers and high consumer demand) did not significantly impact RPI via trust, more contextualized causes explored in Experiments 2, 3A, and 3B do provide retailers with guidance. Specifically, less reputable established retailers should only disclose upstream stockout causes; as it relates to the causes explored in this research, less reputable established retailers stand to benefit by disclosing the cause if due to an earthquake. Start-up retailers and established retailers with better reputations (in terms of product quality and customer loyalty) should disclose not only upstream causes, but such retailers can also disclose focal firm causes that are seen to be relatively less influenced by retailers (COVID-19 cases at the retailer's warehouse, given the positive increase in trust and subsequent impact on RPI).

A second way this research contributes to practice is by identifying ways that retailers can build trust. This is of significance given the overall importance of trust in retailing (e.g., Zboja & Voorhees, 2006) and the challenges that exist in an online environment (Bhalla, 2020; Chen & Dibb, 2010; Kim & Krishnan, 2015; Pavlou et al., 2007). Further, its relevance is highlighted by a focus on developing trust in recent years (Ryan, 2021; Saxena, 2022). While established retailers with lower reputations can build trust by disclosing causes upstream (specifically, an earthquake, as found in Experiment 3B for Retailer C), startups and established retailers with better reputations can develop trust with some upstream *and* focal firm causes (as found in Experiment 3A for Electronics.com and Experiment 3B for Retailer B).

Lastly, this research contributes to practice by specifically focusing on startups or small online retailers, which helps generate actionable insights for small and medium sized retailers that are often neglected in retail SCM research. By utilizing a fictitious online retailer in Experiments 1, 2, and 3A, the insights are applicable for lesser-known retailers, and findings from Experiment 3A can be compared to those from 3B which are instead applicable for established retailers. Of special importance is the finding that small retailers do not risk decreasing a particular consumer's trust (focused on change in trust; Experiment 3A). Given that familiarity is an important determinant of trust (Koehn, 2003) and small/startup retailers will not have the benefit of familiarity, having identified that disclosing the cause of a stockout can help small retailers build trust is beneficial.

4.9.3 Limitations and Future Research

As with all research, there are limitations of the current research and opportunities for related future research. The first limitation is that this research was conducted during a time of supply chain disruption due to the continued impacts of COVID-19 (Xie, 2022) and the war in Ukraine (Noble, 2022), for example, as data was collected from May to October 2022. Thus, consumers participating are in general more accustomed to encountering stockouts given the events of the past few years, which could impact findings. Thus, there is an opportunity to replicate this research in the future to understand whether consumers still react to stockout messaging in the same way once the supply chain is back to 'normal' (Fung et al., 2022). The second limitation is that the scenarios presented in Experiments 2, 3A, and 3B are quite specific and relevant to present events (i.e., the COVID-19 pandemic). This made the scenarios realistic to the consumers participating the experiments, which is important for a scenario-based experiment. The results are also likely applicable to a variety of other related stockout causes, making them readily extended to future disruptions. Still, there is an opportunity to test additional stockout causes in the near or more distant future.

Third, given the examples of stockout disclosures online by retailers such as Patagonia and Wayfair.com, this research was set in an online context. Future research can extend this study to a brick-and-mortar shopping environment to understand whether the impact on consumer sentiments and behavior is similarly applicable. Lastly, as with any method employed in research, there are drawbacks to the use of scenario-based experiments. While internal validity was enhanced by conducting a randomized experiment, I was not able to observe, for example, actual repurchase behavior given the nature of the study. Future research, therefore, could employ other methods to develop additional insights related to disclosing the cause of a stockout.

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APPENDIX A: EXPERIMENT 1 DESIGN, CONDITIONS, AND SCENARIOS

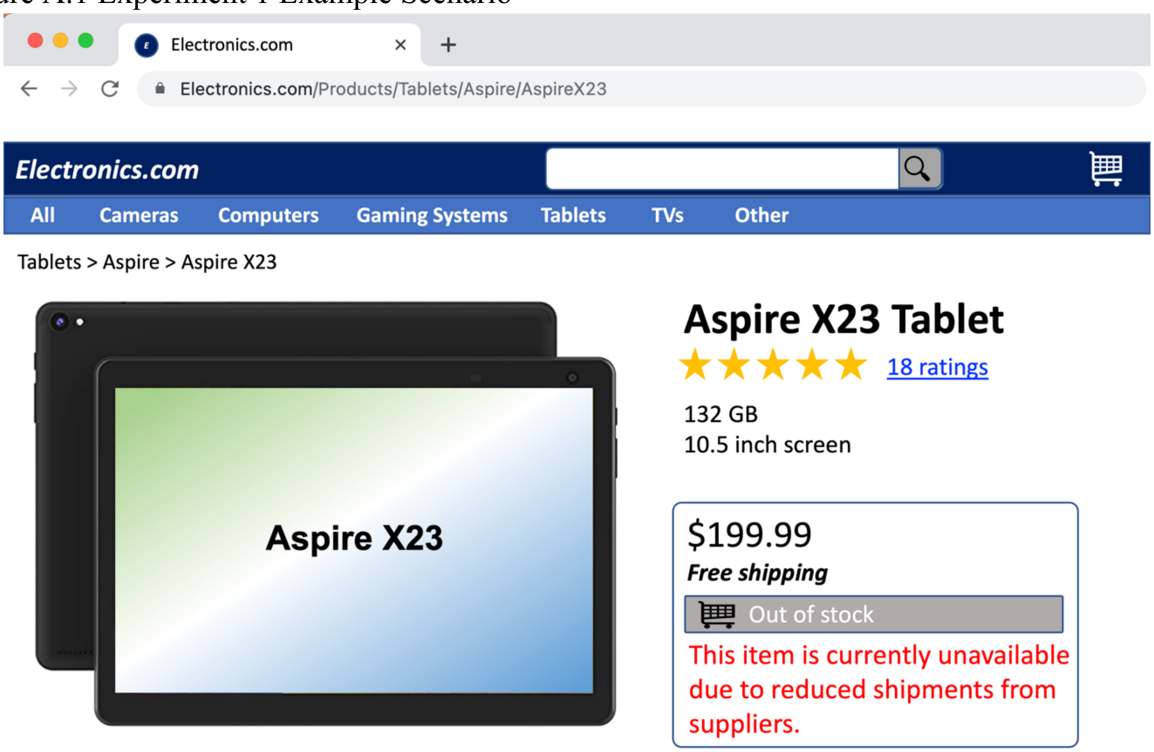
Design: 3-level (stockout cause: upstream, downstream, no cause disclosed) between-subjects.

Stockout Conditions:
- Upstream: "This item is currently unavailable ***due to reduced shipments from suppliers***."
- Downstream: "This item is currently unavailable ***due to high consumer demand***."
- Control: "This item is currently unavailable."

Example Scenario:

You have been planning to buy the new Aspire X23 tablet for yourself. Aspire is a leading brand in the electronics market and sells innovative, high-quality tablets. The Aspire X23 is sold exclusively through online retailer Electronics.com. You visit the website of Electronics.com. When you navigate to the Aspire X23, you find it is currently unavailable. You see the below message on the product's webpage: "This item is currently unavailable ***due to reduced shipments from suppliers***."

Figure A.1 Experiment 1 Example Scenario



APPENDIX B: RESULTS REGARDING THE IMPACT OF GENDER FOR EXPERIMENTS 2, 3A, AND 3B

While Gender as a moderator was not hypothesized in Experiments 2 and 3 given the lack of significance in Experiment 1, I looked at the moderating effect in these experiments. Full regression results from each experiment (2, 3A, and 3B) are presented below as well as tables with construction of the indices of moderated mediation. Like in Experiment 1, no significant indices were found in Experiments 2, 3A, or 3B. This indicates that the association between stockout cause and RPI via trust is not significantly different for females as compared to males.

Table B.1 Experiment 2 Regression Results Including Gender

	PROCESS Model 8			
	Model 1		Model 2	
	Trust	**p-value**	**RPI**	**p-value**
Intercept	–0.138 (0.199)	0.489	–0.114 (0.144)	0.429
Earthquake	0.608 (0.255)	0.018	0.128 (0.187)	0.495
COVID-19 cases at supplier	0.335 (0.265)	0.208	0.230 (0.192)	0.232
Labor shortage	–0.640 (0.255)	0.013	–0.106 (0.188)	0.573
COVID-19 cases at warehouse	0.228 (0.275)	0.408	0.123 (0.199)	0.537
Gender	0.092 (0.278)	0.742	0.333 (0.201)	0.099
Earthquake × Gender	–0.252 (0.427)	0.555	–0.328 (0.309)	0.289
COVID-19 cases at supplier × Gender	0.182 (0.411)	0.658	–0.299 (0.297)	0.315
Labor shortage × Gender	0.360 (0.411)	0.382	–0.167 (0.298)	0.575
COVID-19 cases at warehouse × Gender	–0.053 (0.400)	0.894	–0.204 (0.289)	0.481
Trust			0.720 (0.052)	0.000
F-value(df)	4.576(9)	0.000	26.789(10)	0.000
R^2	0.176		0.583	

Note: standard errors are reported in parentheses.

Table B.2 Experiment 2 Indices of Moderated Mediation

X (Stockout condition)	$\theta_{(X_i * Gender \to Trust)} = a_1$	$\theta_{(Trust \to RPI)} = b_1$	Index of moderated mediation ($a_1 * b_1$)	Bootstrapped confidence interval	Significant
Earthquake	–0.252	0.720	–0.182	(–0.752, 0.355)	No
COVID-19 cases at supplier	0.182	0.720	0.131	(–0.331, 0.613)	No
Labor shortage	0.360	0.720	0.259	(–0.357, 0.904)	No
COVID-19 cases at retailer	–0.053	0.720	–0.038	(–0.574, 0.479)	No

Table B.3 Experiment 3A Regression Results Including Gender

	PROCESS Model 8			
	Model 1		Model 2	
	Trust	p-value	RPI	p-value
Intercept	–0.798 (0.236)	0.001	0.156 (0.197)	0.431
Earthquake	0.625 (0.322)	0.053	0.539 (0.264)	0.043
COVID-19 cases at supplier	0.009 (0.318)	0.978	0.062 (0.258)	0.810
Labor shortage	0.035 (0.329)	0.915	–0.304 (0.267)	0.257
COVID-19 cases at warehouse	0.478 (0.315)	0.131	0.057 (0.257)	0.824
Gender	0.182 (0.307)	0.555	–0.073 (0.250)	0.771
Earthquake × Gender	–0.002 (0.436)	0.996	–0.116 (0.354)	0.743
COVID-19 cases at supplier × Gender	0.311 (0.455)	0.494	0.013 (0.370)	0.973
Labor shortage × Gender	0.070 (0.447)	0.876	0.398 (0.363)	0.274
COVID-19 cases at warehouse × Gender	0.008 (0.444)	0.985	0.213 (0.360)	0.555
ΔTrust			0.426 (0.059)	0.000
F-value(df)	1.756(9)	0.079	8.374(10)	0.000
R^2	0.076		0.306	

Note: standard errors are reported in parentheses.

Table B.4 Experiment 3A Indices of Moderated Mediation

$\boldsymbol{X}$ (Stockout condition)	$\boldsymbol{\theta}_{(X_i*Gender\to\Delta Trust)} = \boldsymbol{a_1}$	$\boldsymbol{\theta}_{(\Delta Trust\to RPI)} = \boldsymbol{b_1}$	Index of moderated mediation ($\boldsymbol{a_1 * b_1}$)	Bootstrapped confidence interval	Significant
Earthquake	–0.002	0.426	–0.001	(–0.371, 0.347)	No
COVID-19 cases at supplier	0.311	0.426	0.133	(–0.280, 0.474)	No
Labor shortage	0.070	0.426	0.030	(–0.424, 0.431)	No
COVID-19 cases at warehouse	0.008	0.426	0.004	(–0.373, 0.336)	No

Table B.5 Experiment 3B Regression Results Including Gender

	Panel A: Retailer B PROCESS Model 8				Panel B: Retailer C PROCESS Model 8			
	Model 1		Model 2		Model 1		Model 2	
	Trust	p-value	RPI	p-value	Trust	p-value	RPI	p-value
Intercept	–0.533 (0.161)	0.001	–0.284 (0.163)	0.083	–0.469 (0.160)	0.004	0.088 (0.178)	0.621
Earthquake	0.177 (0.221)	0.425	0.210 (0.218)	0.337	0.196 (0.229)	0.393	0.266 (0.250)	0.288
COVID-19 cases at supplier	0.259 (0.207)	0.212	0.643 (0.204)	0.002	0.049 (0.225)	0.826	0.315 (0.244)	0.199
Labor shortage	–0.287 (0.225)	0.204	0.544 (0.221)	0.015	–0.491 (0.225)	0.031	–0.212 (0.248)	0.395
COVID-19 cases at warehouse	0.309 (0.215)	0.153	0.445 (0.213)	0.038	–0.116 (0.254)	0.648	–0.314 (0.277)	0.258
Gender	–0.196 (0.207)	0.346	0.251 (0.204)	0.219	–0.111 (0.213)	0.605	0.268 (0.232)	0.250
Earthquake × Gender	0.302 (0.291)	0.301	0.203 (0.287)	0.480	0.336 (0.303)	0.268	–0.519 (0.330)	0.118
COVID-19 cases at supplier × Gender	–0.326 (0.296)	0.272	–0.462 (0.292)	0.115	0.242 (0.304)	0.428	–0.849 (0.331)	0.011
Labor shortage × Gender	0.356 (0.291)	0.222	–0.412 (0.287)	0.153	0.235 (0.297)	0.430	0.135 (0.324)	0.677
COVID-19 cases at warehouse × Gender	–0.050 (0.302)	0.868	–0.361 (0.296)	0.226	0.047 (0.323)	0.886	0.062 (0.351)	0.861
ΔTrust			0.214 (0.073)	0.004			0.295 (0.082)	0.000
Quality	0.072 (0.067)	0.284	0.113 (0.066)	0.089	–0.170 (0.074)	0.023	0.424 (0.082)	0.000
Loyalty	0.039 (0.073)	0.591	0.591 (0.072)	0.000	0.106 (0.090)	0.240	0.360 (0.099)	0.000
F-value(df)	2.145(11)	0.019	15.064(12)	0.000	3.190(11)	0.001	13.544(12)	0.000
R^2	0.116		0.502		0.167		0.483	

Note: standard errors are reported in parentheses.

Table B.6 Experiment 3B Indices of Moderated Mediation

$\boldsymbol{X}$ (Stockout condition)	$\boldsymbol{\theta}_{(X_i * Gender \rightarrow \Delta Trust)} = \boldsymbol{a_1}$	$\boldsymbol{\theta}_{(\Delta Trust \rightarrow RPI)} = \boldsymbol{b_1}$	Index of moderated mediation ($\boldsymbol{a_1} * \boldsymbol{b_1}$)	Bootstrapped confidence interval	Significant
Earthquake					
Retailer B	0.302	0.214	0.065	(–0.054, 0.208)	No
Retailer C	0.336	0.295	0.099	(–0.054, 0.270)	No
COVID-19 cases at supplier					
Retailer B	–0.326	0.214	–0.070	(–0.269, 0.068)	No
Retailer C	0.242	0.295	0.071	(–0.101, 0.277)	No
Labor shortage					
Retailer B	0.356	0.214	0.076	(–0.067, 0.250)	No
Retailer C	0.235	0.295	0.070	(–0.084, 0.256)	No
COVID-19 cases at warehouse					
Retailer B	–0.050	0.214	–0.011	(–0.160, 0.113)	No
Retailer C	0.047	0.295	0.014	(–0.190, 0.217)	No

APPENDIX C: EXPERIMENTS 2, 3A, AND 3B DESIGN, CONDITIONS, AND SCENARIOS

Design:

- Experiment 2: 5-level (stockout cause: earthquake, COVID-19 cases at the supplier, labor shortage at the retailer's warehouse, COVID-19 cases at the retailer's warehouse, no cause disclosed) between-subjects.
- Experiment 3A: Mixed factorial design with a 5-level (stockout cause: earthquake, COVID-19 cases at the supplier, labor shortage at the retailer's warehouse, COVID-19 cases at the retailer's warehouse, no cause disclosed) between-subjects design and a within-subjects measure of change in trust (ΔTrust).
- Experiment 3B: Mixed factorial design with a 5-level (stockout cause: earthquake, COVID-19 cases at the supplier, labor shortage at the retailer's warehouse, COVID-19 cases at the retailer's warehouse, no cause disclosed) x 2-level (retailer: Retailer B, Retailer C) between-subjects design and a within-subjects measure of change in trust (ΔTrust).

Stockout Conditions:

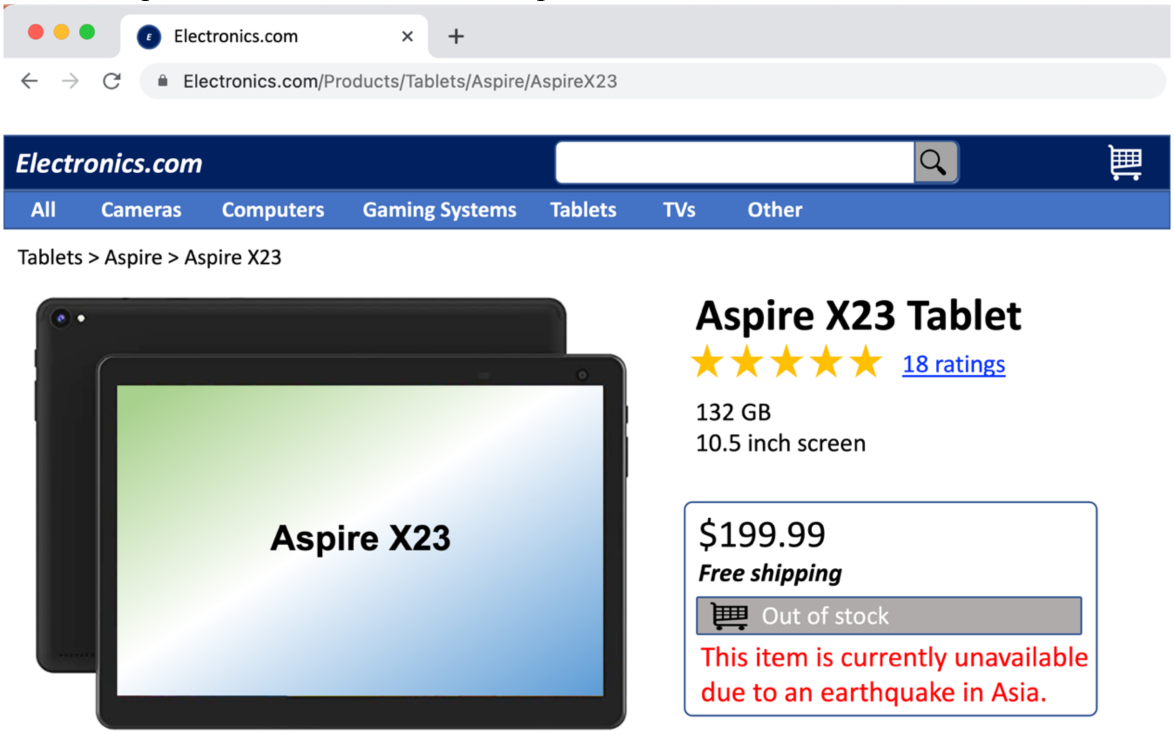
- Earthquake: "This item is currently unavailable ***due to an earthquake in Asia***."
- COVID-19 cases at the supplier: "This item is currently unavailable ***due to COVID-19 cases at our supplier***."
- Labor shortage at the retailer's warehouse: "This item is currently unavailable ***due to worker shortages at our warehouse***."
- COVID-19 cases at the retailer's warehouse: "This item is currently unavailable ***due to COVID-19 cases at our warehouse***."
- Control: "This item is currently unavailable."

Example Scenario:

You have been planning to buy the new Aspire X23 tablet for yourself. Aspire is a leading brand in the electronics market and sells innovative, high-quality tablets. The Aspire X23 is sold exclusively through online retailer Electronics.com (Retailer B, Retailer C). You visit the website of Electronics.com (Retailer B, Retailer C). When you navigate to the Aspire X23, you find it is currently unavailable.

You see the below message on the product's webpage: "This item is currently unavailable ***due to an earthquake in Asia***."

Figure C.1 Experiments 2, 3A, and 3B Example Scenario



APPENDIX D: EXPERIMENT 3B PRETEST

In accordance with Peinkofer and Jin (2023), I conducted a pretest for Experiment 3B via MTurk with 38 participants (average age = 42 years, 58% female, median income = \$40,000 – \$49,999, 89.5% indicated having received at least some college education) to identify differences in perceptions of the two real retailers (anonymized to be Retailer B and Retailer C) used in the study. The pretest was a within-subjects design, so each participant was asked to rate both of the retailers. The retailers were presented in a counterbalanced order to control for ordering effects. Based on paired-sample t-tests, participants indicated significantly higher loyalty to Retailer B than Retailer C ($t = 5.314$, $p = 0.000$, Retailer B average loyalty = 5.128, Retailer C average loyalty = 3.241). Additionally, participants perceived the products sold via Retailer B to be of higher quality than the products sold via Retailer C ($t = 2.449$, $p = 0.019$, Retailer B average quality = 4.809, Retailer C average quality = 4.316).