Management Science

Publication details, including instructions for authors and subscription information:
http://pubsonline.informs.org

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MANAGEMENT SCIENCE
Articles in Advance, pp. 1–22
ISSN 0025-1909 (print), ISSN 1526-5501 (online)
https://pubsonline.informs.org/journal/mnsc

The Value of Competitor Information: Evidence from a Field Experiment

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Received: December 18, 2022
Revised: June 29, 2023
Accepted: July 14, 2023
Published Online in Articles in Advance: August 14, 2024

https://doi.org/10.1287/mnsc.2022.04062

Abstract. To what extent do firms know available information on key competitor decisions, and how does competitor information change their own strategic choices? These questions are fundamental to understanding how firms compete and make strategic decisions, yet systematic evidence on them remains limited. I designed a field experiment across 3,218 differentiated firms in the personal care industry, where firms randomly assigned to treatment received easily accessible information on competitor prices. At baseline, nearly half of treatment firms appeared to lack knowledge of competitor prices. Once treatment firms received competitor information, they were more likely to change their own decisions, aligning them with competitors rather than differentiating. These changes were driven by firms that were more misaligned in their price and quality decisions, and appear to have been performance enhancing. If competitor information was both easily accessible and decision relevant, why did firms not use this information on their own? Results from a follow-up experiment suggest that this lack of knowledge may have been driven by managerial inattention. These findings highlight that limited information processing is a key problem for firms and a central issue in strategy, and raise the possibility that growing availability of competitor data may lead firms to align their decisions more with their competitors.

History: Accepted by Alfonso Gambardella, business strategy.

Funding: H. Kim is grateful for the support of Harvard Business School, the Foundations of Human Behavior, the Ewing Marion Kauffman Foundation, and Yelp.
Supplemental Material: The online appendix and data files are available at https://doi.org/10.1287/mnsc.2022.04062.

Keywords: competitive strategy • decision making • data • analytics • randomized controlled trial

1. Introduction

Understanding the competitive environment is central to firms' strategic decisions, especially for key choices such as price, quality, and location. Firms' knowledge of key competitor decisions is thus often implicitly assumed in theoretical models and interpretations of empirical evidence in strategy research.

However, systematic evidence on how knowledgeable firms are in practice of their competitive environment and how this information leads them to change their own decisions remains limited. Well-known examples suggest that firms may lack awareness of some competitors (Cyert and March 1963, Porac et al. 1989, Baum and Lant 2003, Thatchenkery and Katila 2021),[1] but they often explore contexts with high barriers to information acquisition or low competition—raising the possibility that any lack of competitor knowledge may be limited to these contexts. Furthermore, although case studies and business teaching curriculum propose that analyzing competitor decisions and the market more broadly will lead firms to discover more differentiated strategies, there has been no large-scale causal evidence to support this view. A major challenge has been measurement and selection; firm knowledge and decisions must be evaluated across a large sample of firms across markets with accessible information and varying degrees of competition, and the treatment effect of competitor information must be isolated from the nonrandom selection of firms that choose to invest in it.

This raises the question, to what extent do firms use information they have access to about the competitive environment, and how does this information change their own decisions? This question is fundamental to understanding how firms compete and make strategic decisions, especially as market data become increasingly accessible for firms.

This paper explores this question using a randomized controlled trial across 3,218 businesses in the personal care industry. I provide large-scale evidence that firms across hundreds of local markets lacked knowledge of key competitor decisions even when this information was readily accessible and led to performance-enhancing changes, and provide suggestive evidence that this was driven by managerial inattention. Furthermore, I show that providing this information led firms to align their decisions more with those of their competitors. These findings highlight that limited information processing is a key problem of firms and a central issue in strategy, and raise the possibility that growing availability of data may lead firms to make more similar decisions to their competitors.

The experiment ran across personal care firms offering nail care services, a differentiated $9.8 billion market in the United States that enables precise identification of competitor knowledge and its impact across thousands of firms in hundreds of local markets. Collaborating with Yelp, an online reviews platform, I physically sent canvassers to all firms for a standard marketing visit. Firms randomly assigned to treatment received additional information during this visit on their price positioning relative to their nearest competitors, a key strategic lever that drives customer decisions in this market.

To evaluate the impact of this information, I measured firms' baseline knowledge of competitors prior to treatment and constructed a dataset of positioning and performance measures over 12 months. Approximately 50 data collectors at any given time made phone calls each month to all 3,218 firms to obtain data on pricing. They also physically visited firms at baseline and endline to observe measures of their quality positioning.

At baseline, nearly half of treatment firms appeared to lack competitor knowledge, including those facing higher levels of competition. Many managers stated that they could easily acquire competitor pricing information, suggesting that competitor prices may not be decision relevant—either because of other information like residual market demand that offers sufficient statistics for this information, or because of a large base of regular customers that shields firms from competition.

However, once treatment firms received information on competitor prices, they changed their pricing accordingly, suggesting that this information was valuable. Treatment firms were three percentage points more likely to change their prices relative to control firms in the months following the canvasser visit, a 17% increase. Rather than differentiating by shifting their decisions to distance themselves from their competitors, treatment firms changed their pricing to align with their geographically nearest competitor's decisions; those charging more than their nearest competitors reduced their prices, whereas those charging less increased their prices. Although I primarily examine pricing decisions as they can be adjusted faster and are easier to measure precisely, I additionally find that treatment firms were 9% more likely to change their quality, consistent with the interpretation that treatment firms changed their positioning. Firms that were over- or underpricing relative to their quality were more likely to change prices, suggesting that these changes were improvements.

I find evidence consistent with the interpretation that competitor information improved firm performance. Treatment firms observed 8% more employees and customers at the business at endline; had 3% lower availability for an appointment during peak hours the next day; and received 15% more calls, page views, and map direction views on Yelp. Treatment firms also obtained more customer reviews and photos on Yelp, and back-of-the-envelope calculations provide consistent results for revenues. These performance effects were mainly driven by firms that were overpricing relative to their nearest competitor. I observe little supportive evidence that these effects were driven by firms' increased usage of the Yelp platform as measured by their log-ins, account claims, advertising, and comments on reviews. I also do not find evidence of significant spillover effects, although performance effects are likely to stem at least in part from business stealing from control firms unless the market for these services expanded over the period of the experiment.

Why did easily accessible competitor information lead firms to change their decisions? I explore potential mechanisms underlying these results and provide some suggestive evidence. First, I assess why competitor information might lead firms to change their decisions. In principle, competitor information could help firms learn about competitor decisions, or help them learn about market demand from observing their competitors' decisions. Although these two channels are conceptually and empirically difficult to cleanly disentangle, the evidence points largely to the competitor rather than the demand effect.

Second, given the positive impact of the competitor information treatment, the natural question is why firms did not previously invest in this information on their own. Collecting competitor information shown as treatment took managers a maximum of one minute per competitor, with back-of-the envelope calculations implying that the profit margin on additional customers would have to be smaller than 1.8% for the marginal cost of collecting this information to be lower than the marginal benefit for the average firm. Although not conclusive, I find suggestive evidence that this behavior may have been driven by a form of managerial inattention; they believed that they already knew it and underestimated the value of paying attention, having looked at it at an earlier point in time. To explore this further, I run a follow-up experiment across control firms, randomly assigning managers to reassess their competitor knowledge *before* being asked whether they were interested in receiving competitor information for free compared with *after*. Those who were asked to reassess their knowledge first were more likely to sign up to receive competitor information, providing suggestive evidence consistent with this interpretation.

In addition to research in strategy on competitive interactions, this paper contributes to a few strands of literature. First, a variation of the concern about whether firms lack awareness of competitors is how firms apply available data to frame and improve decisions. Research on data-driven decision making has provided evidence that using more information in decisions is associated with higher firm performance (Bajari et al. 2019, Brynjolfsson and McElheran 2019, Camuffo et al. 2020, Koning et al. 2022). This paper provides causal evidence on how competitor information impacts firm decisions, and suggests that despite its value and accessibility, firms may fail to attend to and use data about the competitive environment.

Second, research on management practices has documented how firms' lack of knowledge and adoption of best practices drives dispersion in firm performance (Bloom and Van Reenen 2007, Syverson 2011, Bloom et al. 2013, Bruhn et al. 2018). This paper provides evidence on how widespread this phenomenon may be, even for first-order strategic decisions in settings with relatively low barriers to information and high competition. The findings also provide suggestive evidence that behavioral factors, like managerial inattention, may drive this lack of knowledge, consistent with growing work on behavioral firms and pricing frictions (Cho and Rust 2010, Goldfarb and Xiao 2011, DellaVigna and Gentzkow 2019).[2]

Relatedly, research on the cognitive underpinnings of strategy has proposed the importance of managerial capabilities for attention and information processing (Ocasio 1997, Eggers and Kaplan 2009, Helfat and Peteraf 2015). However, problems in measurement and identification have made it hard to evaluate the extent to which they might impact firm strategies. This paper provides empirical evidence on how inattention might lead firms to overlook competitor decisions, and proposes that firms may become inattentive because of prior outdated knowledge that leads them to be complacent to new information. Building on ideas raised by Gavetti (2012), these findings suggest that even in competitive markets, managers may need to worry about inattention to the immediate competitive environment and that attention may increasingly create opportunities for competitive advantage.

2. Conceptual Motivation

Despite the centrality of competitor knowledge and its frequent assumption in theory and interpretations of empirical analyses, there has been limited systematic evidence on the extent to which firms hold knowledge of their competitors and how it affects their decisions on strategic positioning. In this section, I discuss this work and consider three ways in which competitor information may impact firm decisions.

2.1. Firms' Knowledge of Competitors

Strategy is centrally concerned with how firms respond to their internal and external environments. Although the idea of blind spots or awareness of peripheral competitors has received much attention in strategy frameworks (Porac et al. 1989, Baum and Lant 2003, Chen et al. 2007, Thatchenkery and Katila 2021), knowledge of key decisions taken by direct competitors has often been implicitly assumed. For example, research on competitive interactions and strategic positioning often analyzes firm decisions relative to their competitors' decisions to conclude when and why firms differentiate (e.g., Haveman 1993, Baum and Haveman 1997, Deephouse 1999, Semadeni 2006, Wang and Shaver 2014). By interpreting firms' positions as reflecting intentional choices based on their competitors' choices, the implicit assumption in these studies is that firms are knowledgeable of their competitors' decisions and are responding to them, although their knowledge of them is not observed. This assumption of competitor knowledge is so deeply held that some studies have even argued that any advantage from doing competitor analysis has dissipated because all firms already know this information (Argote and Ingram 2000).

However, systematic evidence on how knowledgeable firms are of their competitors' key decisions in practice and how this information impacts their own decisions remains limited.[3] Although a rich literature of case studies and business teaching curriculum suggests that analyzing competitor decisions will lead firms to allocate resources into superior positions or influence industry structure in favorable directions (e.g., Porter 1980), there has been little supportive large-scale causal evidence. Understanding how firms use available information on competitor decisions across varying competitive contexts is critical to better understand how firms make strategic decisions and respond to competition, especially as competitor data become increasingly available.

This paper seeks to provide empirical insight on this question through a large-scale study of firms' knowledge of competitors in an industry where competitor information is easily attainable. Across thousands of firms competing in hundreds of local markets, I examine measures of competitor knowledge and analyze whether firms that are randomly assigned to receive competitor information change their decisions.

2.2. How Competitor Information May Impact Firm Decisions

Although a large literature suggests that firms can learn from other firms (Baum and Ingram 1998, Conley and Udry 2010) and that more information should at least weakly improve firm decisions (Galbraith 1974, Brynjolfsson and McElheran 2019), there is less insight on *how* information on competitor decisions might affect firm decisions. There are three possible alternatives.

First, it is possible that competitor information has little impact. Firms may already know competitor decisions or not need to know them if other informative sources, such as observing customers and residual market demand, offer sufficient statistics for competitor information, especially in more competitive markets where strategic interaction may be limited. Consistent with this view, some popular management articles even advise managers to ignore competitors, with well-known executives like Jeff Bezos of Amazon and Larry Page of Google echoing this advice.[4] Although this advice may be driven by potential concerns of distraction or hindrance to originality, underlying it is the suggestion that firms may be able to obtain functionally equivalent insights without paying close attention to competitor decisions.

Second, the positioning view suggests that competitor information may result in more differentiated positioning, as industry analysis leads firms to arrive at more distinctive positions compared with their competitors (Porter 1980, Greenstein and Mazzeo 2006). Competitor data may thus lead firms to move to a better position, resulting in firms shifting their pricing and quality decisions to be farther away from competitors' decisions such that they end up being more spread out in their positioning. This would suggest that when firms receive competitor information, they decrease their prices further if they charged lower prices compared with their competitors and increase their prices further if they charged higher prices in order to differentiate themselves more.

However, another strand of research suggests that firms may imitate the strategies of their competitors to economize on their search costs in the face of uncertainty, follow others who may have superior information, or maintain competitive parity from the view of consumers (DiMaggio and Powell 1983, Haveman 1993, Greve 1996, Lieberman and Asaba 2006). This may result in firms seeking to adjust their pricing to match price-quality combinations offered by competitors to make consumers more comfortable with their offering, which could also be thought of as a class of managerial best practices—as firms that are initially mispricing or mispositioned move to the productivity frontier (Bloom and Van Reenen 2007, McKenzie and Woodruff 2017). This would suggest that when firms learn competitor information, they align their pricing relative to competitor offerings, increasing their prices if they charge lower prices compared with their competitors and decreasing prices if they charge higher prices compared with their competitors.

The treatment in this experiment is designed to tease apart how competitor information impacts firm decisions by randomly assigning firms to physically receive competitor information, which helps alleviate concerns of endogeneity and ensures that firms pay attention to this information. I also explore whether this information ultimately results in improvements in measures of performance.

3. Competitor Knowledge and Positioning in the Personal Care Industry

3.1. Why Study Firms in the Personal Care Industry?

Any empirical study must choose a setting, and in-depth industry studies have long uncovered valuable empirical facts. Studies of hotels provided insights on firm positioning and competitor perception (Baum and Haveman 1997, Baum and Lant 2003, Li et al. 2017). Pizza stores offered evidence on how organizations acquire and transfer knowledge (Darr et al. 1995). Studies of fishing boats, pineapple farms, and ready-mix concrete provided detailed insights on firm productivity and learning (Jensen 2007, Conley and Udry 2010, Syverson 2011).

Finding a market to study whether and why firms lack competitor knowledge and how this impacts their strategic choices like positioning imposes many requirements. First, it requires a large number of firms across varying market conditions to evaluate the impact of competition and firm-specific attributes. Second, price positioning must be clear, measurable, and comparable across firms, which is challenging to find. Even in a relatively simple market like cafés, a cup of coffee can vary in size and perceived quality across firms. Finally, information on competitors must be easily accessible to rule out the possibility that the cost of acquiring information is too high.

Assessing many possible industries on these criteria led me to choose personal care businesses that offer nail care services,[5] as this context enables precise identification of firms' knowledge of competitor decisions and its impact across thousands of firms that compete in hundreds of local markets with varying degrees of competition. It is a $9.8 billion market in the United States (IBISWorld 2019)—which is slightly larger than the market for men's clothing stores (approximately $8.5 billion) and slightly smaller than egg production (approximately $10.5 billion) (IBISWorld 2019). The market is competitive and fragmented, but there are also large chains with hundreds of salons across countries and over a million in annual revenues. Many businesses represent entrepreneurial endeavors, often founded by immigrants and women who pursue entrepreneurship as a career alternative (Nails Magazine 2015). Although some consumers are loyal to one business, the market is generally characterized by substantial consumer search compared with similar local business verticals.

This is a compelling setting to study the impact of competitor information for several reasons. First, nail salons represent one of the largest industries in sheer number among local businesses, and they compete locally. Ninety-four percent of consumer search occurs within a radius of five miles, even in a geographically dispersed city like Los Angeles (Figure E.1 in the Online Appendix). This provides a large sample of thousands of firms across hundreds of local markets to evaluate the impact of competitor knowledge and how it varies with firm attributes and market competition.

Second, nail salons are differentiated, but they have simple strategy spaces that are comparable and observable, where pricing is a key competitive driver. Every salon has a price for a regular manicure that approximates to its price positioning (as other services are priced proportionally to the regular manicure price), which generally varies from \$5 to \$60. Quality can be observed from the luxuriousness of the decor, the cleanliness of the interior, and the brands of nail polish used—which can vary from \$9 to \$70 per bottle at retail cost. These decisions and how they are made are typical of other retail firms and of small and medium enterprises (SMEs) more generally, which make up 99.7% of U.S. establishments and represent 47% of employment and 46% of gross domestic product (GDP).[6]

Finally, information on competitor pricing is easily accessible, enabling a study of why firms might lack competitor knowledge even when this information is available. Consumer reviews on Yelp indicate that price comparisons are a key consideration (Figure E.4 in the Online Appendix); hence, a slight drop in price can have a large effect on demand. Many managers commented that they could easily obtain competitor pricing online or in person, suggesting that the cost of information is fairly low. Nearly all firms were aware of Google and Yelp, and most had a competitor within 0.5 miles that they passed by on their way to work. Obtaining information on competitor prices directly via phone calls also took less than one minute per competitor.

3.2. The Selection of Firms for the Field Experiment

To run the experiment, I partnered with Yelp, an online platform that crowdsourced listings and reviews of local businesses, to deliver the information treatment in a more natural manner. As of June 2018, Yelp listed over 4.6 million verified local businesses with 163 million reviews, 74 million desktop monthly visitors, and 72 million mobile monthly visitors (Yelp 2018).[7] The platform had a free dashboard for businesses to observe information about their reviews where it could, in theory, provide information about the market. At the time of the experiment, Yelp was sending canvassers to physically visit a handful of businesses each year as a marketing initiative to inform them about their free business page. I expanded these efforts and added an information intervention on top of their standard marketing visit for businesses assigned to treatment.

The greater metropolitan areas of San Francisco Bay, New York City, Los Angeles, and Chicago were chosen as markets for the field experiment based on (i) the presence of Yelp offices to facilitate the canvassing effort, (ii) the number of nail salons in the area to allow for a sufficiently large sample, and (iii) coverage of Yelp to obtain robust data on businesses (Glaeser et al. 2022). I identified zip codes within these areas and extracted all 9,889 nail salon listings on Yelp in these zip codes.

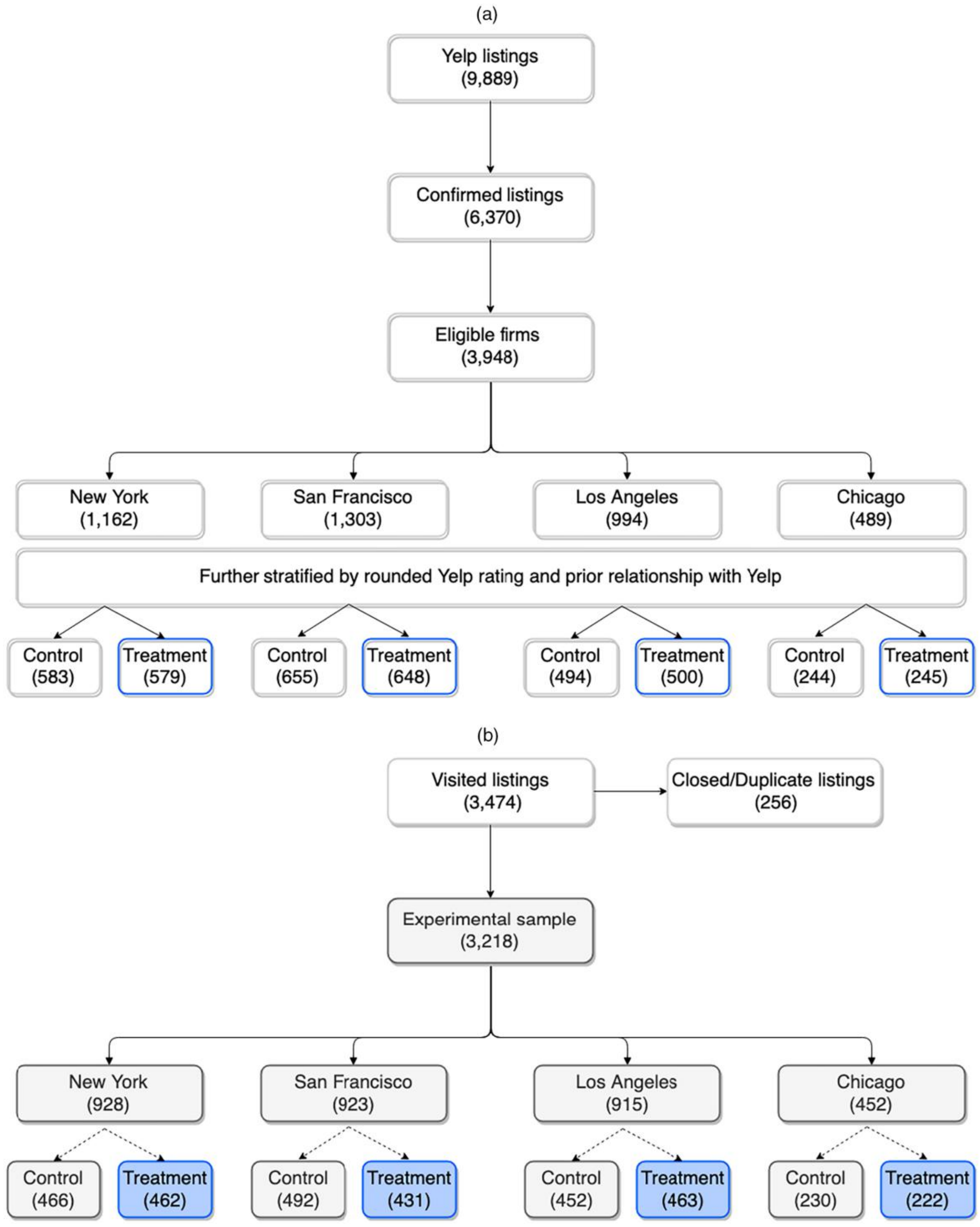
I applied the following criteria to determine the eligible set of businesses for the experiment (Figure 1(a)). I called every listing and used Google Maps Streetview to confirm that they were open, offering nail services, correctly located,[8] and not a duplicate listing. Any salons with Yelp ratings of 1–2.5 stars (of 5 stars) were excluded to maximize the likelihood of compliance—as businesses with 1 or 2 stars were more likely to have antagonistic stances against Yelp and less likely to speak to canvassers. This restriction was applied only for defining the experimental sample, with the full set of firms used to determine treatment information on competitor pricing as well as any measures of competition. Although this might introduce some selection in the sample, to the extent that lower-rated firms were lower performing and less likely to know competitor information, the experimental sample provided a stronger test for the impact of competitor information. The resulting eligible set of 3,948 businesses (62% of the confirmed set of salons) was the goal that Yelp canvassers strived toward reaching in the summer of 2018, subject to a fixed canvassing budget and timeline.

Canvassers reached 3,474 businesses,[9] 256 of which were identified as duplicates or closed by the time that they visited. This resulted in an experimental sample of 3,218 firms (see Figure 1(b) for a diagram and Figures A.4 and A.5 in the Online Appendix for maps of the sample).

3.3. Measuring Competitor Knowledge, Firm Positioning, and Performance

Across this sample, I constructed a data set of competitor knowledge, positioning, and performance over a 12-month period between May 15, 2018 and September

Figure 1. (Color online) Randomization and Experimental Sample



Notes. This figure shows the sample definition and randomization map. (a) Randomization. All nail salon listings on Yelp across the greater San Francisco Bay Area, New York City, Los Angeles, and Chicago were verified via phone calls and Google Streetview, resulting in 6,370 confirmed firms. This set was further restricted by excluding any salons with Yelp ratings of 1–2.5 stars (of 5 stars) to maximize the likelihood of compliance to treatment, which resulted in an eligible set of 3,948 businesses (62% of confirmed firms) that canvassers strived toward reaching, subject to the budget and timeline. (b) Experimental sample. Between June 18, 2018 and November 18, 2018, canvassers reached 3,474 firms. Two hundred and fifty-six were duplicates or closed by the time that they visited, resulting in an experimental sample of 3,218 firms. All firms in Los Angeles and Chicago and most firms in New York and the Bay Area were reached (excluding the Bronx and North Bay as shown in Figures A.4 and A.5 in the Online Appendix).

15, 2019 (see Figure A.6 in the Online Appendix for a full timeline).

To measure competitor knowledge and perceived positioning, Yelp canvassers asked the following questions to owners or key managers at treatment firms prior to delivering competitor information. (1) "What do you think sets your salon apart from your competitors?" (2) "Who do you consider as your primary competitors?" (3) "What do you think they charge for a regular manicure?" (See Figure A.3 in the Online Appendix for a full script.) Canvassers recorded answers as close to verbatim as possible, which was monitored daily and coded by two independent coders. To ensure accuracy, canvassers, managers, and coders were all blind to the experiment and its outcome variables.

Data on positioning were collected by a team of approximately 50 data collectors who called and visited businesses.[10] Price positioning was measured by the price of a regular manicure without taxes or cash discounts, which was collected via monthly calls made to all businesses between May 2018 and May 2019. Quality was measured as a sum of the level of nail polish brands used, the cleanliness of the interior, and the luxuriousness of the decor as observed via physical visits to each business at baseline (May to August 2018) and endline (May to September 2019).[11] To ensure data validity and accuracy, data collectors were given detailed scripts and evaluation rubrics, required to document photo evidence, and had 5% of their assignments validated by another independent data collector (see Online Appendix C for further details). All data collectors were blind to the experiment, and were assigned to collect data on both control and treatment firms, with their location and performance tracked on a weekly basis.

Measures of firm performance were proxied using several indicators. I collected a binary indicator of whether there was availability for an appointment during a peak time (4–5 p.m.) the next day via monthly calls[12] and counted the number of employees and customers observed at the time of data collector visits. From the Yelp platform, I constructed monthly measures of performance based on the number of unique consumer views of the business page, the number of calls made to the business, and the number of views of map directions to the business—measures that prior studies have found to be positively correlated with firm revenues (e.g., Dai et al. 2023).

3.4. Baseline Competitor Knowledge and Positioning

At baseline, firms displayed substantial dispersion in both their perceived and observed positioning. Figure 2 shows firms' descriptions of their perceived positioning, which was mostly differentiated along quality; higher-quality firms (in brown in Figure 2) offered better service, cleanliness, and luxuriousness, and lower-quality firms (in red in Figure 2) offered lower prices.

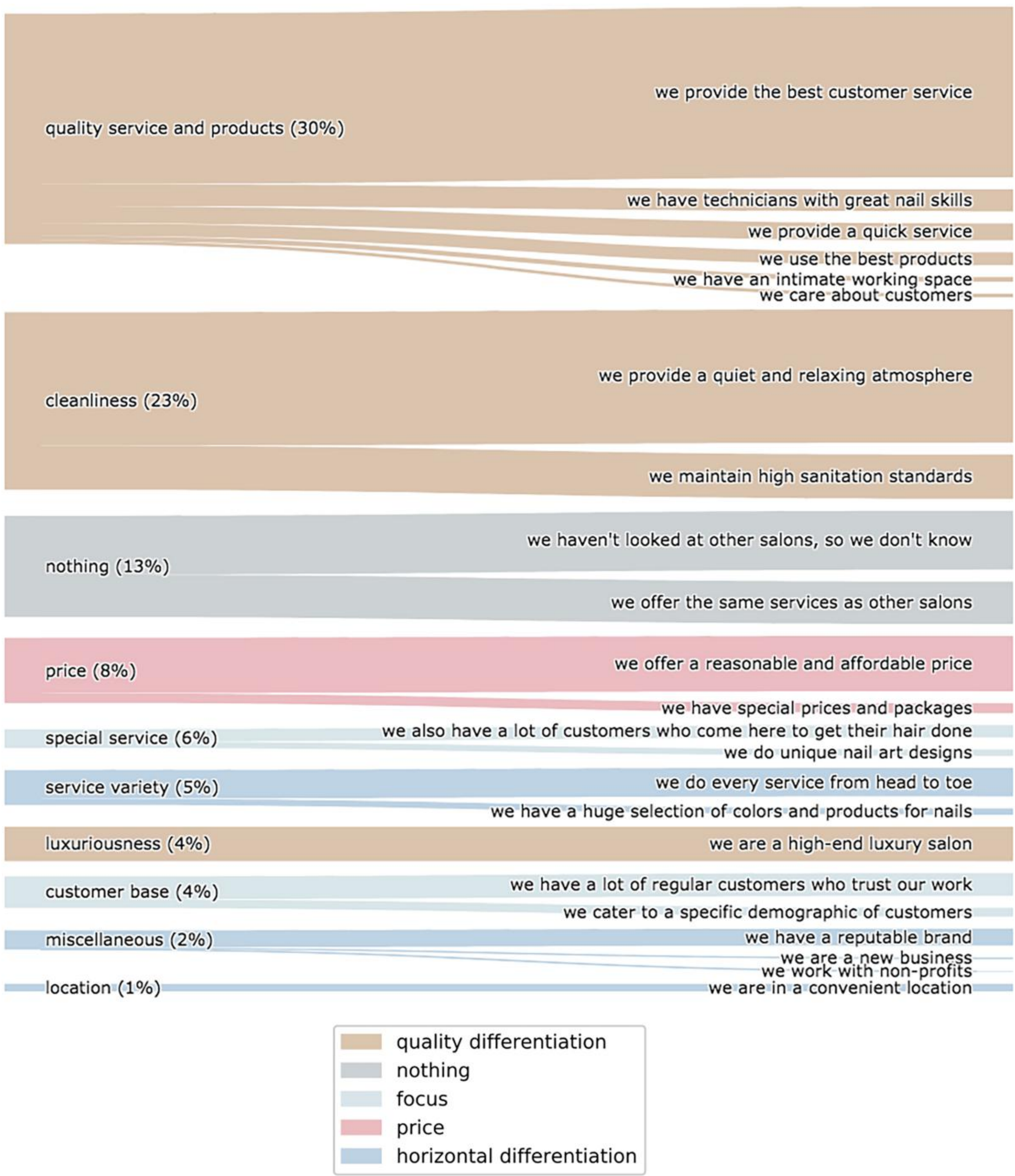
In line with this, firms that offered higher quality charged higher prices on average (Figure 3(a)). However, firms displayed a large dispersion in pricing, even among those located in the same zip code offering similar quality. Figure 3(b) plots the same figure as Figure 3(a) but shows every firm observation within each quality level sorted by price, along with the interquartile range. The coefficient of variation in price is 38% and ranges from 22% to 47% within each quality level, which persists when controlling for zip code fixed effects and across firms that faced higher competition (Figures D.3 and D.4 in the Online Appendix). Some of this price dispersion may in part be explained by noise in the quality measures or firm attributes not captured (e.g., customer service).

Baseline measures also suggested that many firms may have lacked competitor knowledge. Forty-six percent of managers at treatment firms were unable to state their primary competitors prior to treatment—responding that they did not know or that it had been a while since they looked at other businesses (Figure B.1(a) in the Online Appendix).[13] Similarly, 58% of managers at treatment firms were unable to state the prices that their primary competitors charged (Figure B.1(b) in the Online Appendix). Consistent with this, I find that firms' observed pricing and quality decisions did not match their descriptions of perceived positioning relative to competitors (Figures D.5 and D.6 in the Online Appendix).

Informal interviews with managers conducted separately at 25 businesses provided additional suggestive evidence that managers may not know competitor information, even though it was easily accessible. When asked to specify who their primary competitors were and what they were charging, most managers were unable to answer precisely. They explained that they were not sure exactly what the prices may be because it had been a while since they had last checked, suggesting that the competitive information that firms use may lag the actual situation. For example, one salon owner responded, "I thought I knew, but I guess it's now been a few years since I've checked who our competitors are." Another manager corroborated, "now that I'm trying to answer these questions, it must have been about 10 years ago that I last looked at competitors' prices … in detail."

Higher levels of competition only marginally reduced the number of firms whose managers stated that they lacked competitor knowledge. Proxying the level of competition using two measures—(1) the firm's distance from its geographically nearest competitor and (2) baseline price dispersion across its nine geographically nearest competitors[14]—I find that a substantial percentage of firms that faced higher competition were also not able to

Figure 2. (Color online) Descriptions of Firm Positioning

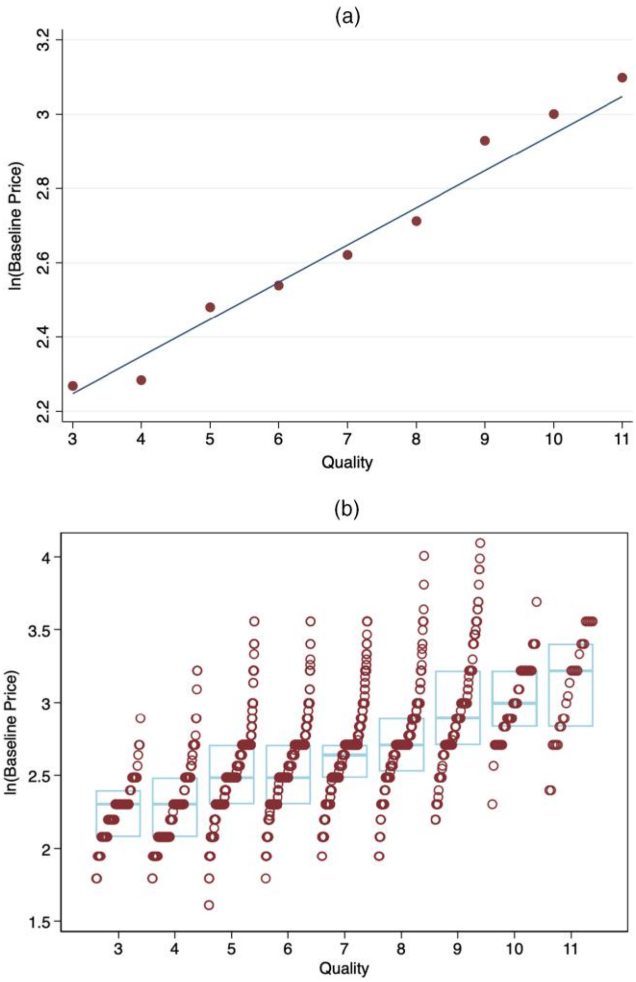


Notes. This figure shows a diagram of the self-descriptions that managers at treatment firms provided on their positioning prior to treatment prompted by the following question: "What sets you apart from your competitors?" Each response was coded into categories by two independent research assistants, with any discrepancies sent to a third research assistant. The largest category of responses is quality differentiation (59%) followed by nothing (14%), focus (10%), price (9%), and horizontal differentiation (8%).

state competitors and their prices (39% and 44%, respectively) (Figures B.3 and B.4 in the Online Appendix).[15] This lack of knowledge also persisted across firms above median size (number of employees), age, and price points (Figures B.5–B.7 in the Online Appendix).

These baseline observations suggested that firms may not know competitor information, possibly because they did not need to know it. Other informative sources, such as observing customers and residual market demand, could have offered sufficient statistics for key competitor decisions, especially in more competitive markets. This provided a clear null hypothesis that if this was the case, providing competitor information should have little effect on firms' decisions.

Figure 3. (Color online) Mapping Pricing and Quality Decisions



Notes. (a) Average price by quality. This panel plots a binscatter of logged baseline price on baseline quality. Quality represents a sum of the firm's polish brand level, cleanliness, and luxuriousness, and ranges from 3 (lowest) to 11 (highest). This is robust to using a standardized sum of polish brands, cleanliness, and luxuriousness as well as each individual measure alone (reported in Figures D.2 and D.3 in the Online Appendix). (b) Dispersion in firm pricing by quality. This panel plots every firm observation (represented by red circles) within each quality level sorted by price along with the interquartile range (in blue). The coefficient of variation in price across all observations is 37.8%. Within each quality level, the coefficient of variation in price ranges from 22.2% to 47%.

4. The Competitor Information Experiment

4.1. The Competitor Information Intervention

All firms in the experimental sample received a physical visit from a Yelp canvasser between June and November 2018. Across all visits, canvassers provided a marketing brochure with information on how to edit business details, add photos, and respond to reviews (Figure A.1 in the Online Appendix), and helped with logging in or claiming the account. This marketing brochure was accompanied by a standard marketing postcard with Yelp advertising credits (Figure A.1 in the Online Appendix).

For firms assigned to control, the back of the postcard showed a blank canvas. For firms assigned to treatment, the back of the postcard showed a personalized competitor pricing report (Figure 4), which displayed the firm's regular manicure price compared with its nine geographically closest competitors[16] along with their names and exact prices. The report showed the name of the business with a summary description, algorithmically generated to take one of three versions. (1) "You charge the lowest/highest price in the area." (If applicable, the following statement was added: "*n* businesses charge the same price.") (2) "Most businesses nearby charge (the same or) higher/lower prices than you. *n* businesses charge less/more." (3) "Most/all businesses nearby charge the same price as you." (See Figure A.2 in the Online Appendix for the distribution of descriptions).[17] Before providing this information, canvassers also asked treatment firms who their primary competitors were, what prices they charged, and how they compared with their competitors as detailed in Section 3.3.

Of course, pricing is simply one piece of information about competitors that firms may be interested in. This intervention focused on pricing because it appeared to be a key driver of customer decisions in this market. Analyzing the text of Yelp reviews for all nail salons using word2vec, a natural language processing technique to learn word embeddings from large data sets,[18] I

Figure 4. (Color online) Sample Treatment Information

Notes. The back of the marketing postcard for treatment businesses included a personalized competitor pricing report, a sample of which is shown. The image shows the firm's regular manicure price compared with its nine geographically closest competitors. The right side of the postcard lists the names of each competitor along with the exact prices they charge. The postcard displays the name of the business at the top with a line summarizing the firm's relative price positioning, which was algorithmically generated to take one of three versions. (1) "You charge the lowest/highest price in the area." (If applicable, the following statement was added: "*n* businesses charge the same price.") (2) "Most businesses nearby charge (the same or) higher/lower prices than you. *n* businesses charge less/more." (3) "Most/all businesses nearby charge the same price as you."

found that pricing and comparison with competitors accounted for the most frequent phrases in reviews, with 46% of reviews discussing topics related to price and 35% mentioning competitors (Figure E.4 in the Online Appendix).

Every canvasser worked independently and was individually trained by me and Yelp's team managers with a standardized script and practice visits (Figure A.3 in the Online Appendix).[19] Canvassers were blind to the experiment, and were assigned to one type of canvassing before being transitioned to the other, with Yelp informing them that they were trying different ways of canvassing.[20] A phone application recorded the canvasser's location and date stamp for each visit, and canvassers were instructed to follow up with a business up to three times if they were not able to speak with a manager or owner.[21]

4.2. The Experimental Design

All eligible businesses described in Section 3.2 were assigned to experimental groups through a stratified randomization process based on their metropolitan area, prior relationship with Yelp, and Yelp rating (Figure 1(a)).[22] Within each stratum, firms were randomly assigned to one of two experimental groups: control or treatment. This resulted in 1,640 control firms and 1,578 treatment firms across an experimental sample of 3,218 firms.

Table 1 shows that control and treatment firms were generally well balanced, consistent with randomization. For two variables, control and treatment firms appear to be statistically different. The difference in luxuriousness is small (0.10) and may be explained by missing observations because of businesses being closed at the time of data collector visits,[23] but the timing of canvassing visits appears to be delayed among treatment firms by 1.4 weeks, even though canvassers were assigned to finish all visits across firms within a neighborhood before moving on to the next neighborhood in order to balance the timing of visits.[24] Given the importance of this variable, I add fixed effects for the week of the canvassing visit to all preregistered specifications.[25]

Noncompliance and attrition were low at 2% and 1%, respectively, and did not vary significantly across groups (Table A.1 in the Online Appendix).[26]

5. The Impact of Competitor Information on Firm Pricing

5.1. Do Treated Firms Change Their Pricing?

5.1.1. Graphical Evidence. Figure 5(a) plots the raw share of control firms versus treatment firms that

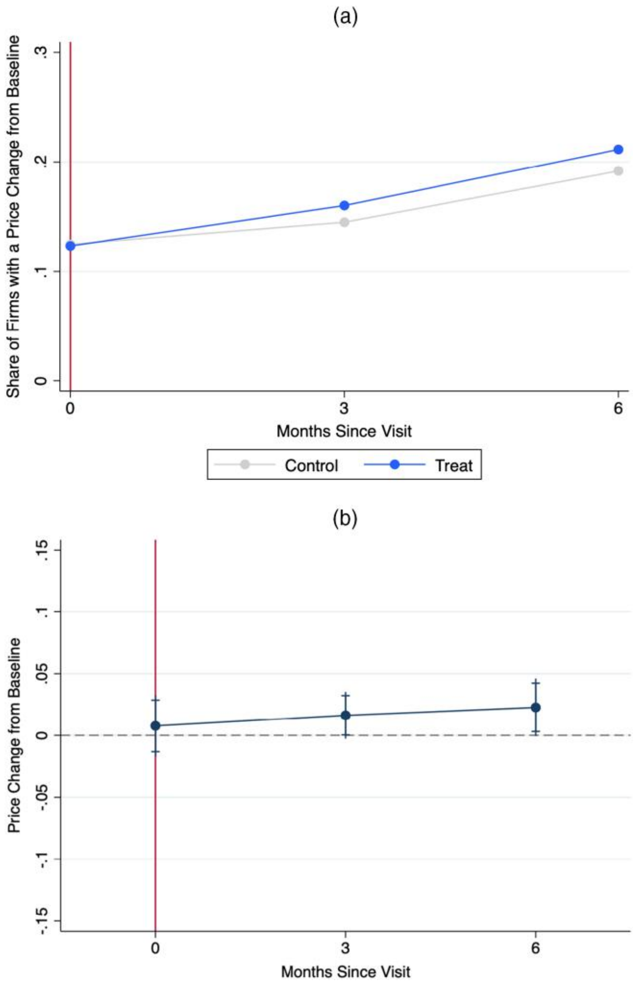
Table 1. Summary Statistics and Balance of Variables

	Control mean	Treatment mean	SD	Min	Max	Count	Difference	p-value
Baseline Price	13.79	13.98	5.24	5.00	60.00	3,218	−0.19	0.30
Latitude	38.13	38.09	2.95	33.72	42.05	3,218	0.04	0.71
Longitude	−102.58	−102.08	21.17	−122.56	−73.68	3,218	−0.49	0.51
Baseline Number of Employees	4.22	4.31	2.53	1.00	25.00	2,923	−0.09	0.31
Baseline Number of Customers	3.68	3.82	3.23	0.00	30.00	2,926	−0.13	0.26
Baseline Total Hours Open Weekly	61.89	62.23	10.25	8.00	115.50	3,073	−0.33	0.37
Baseline Cleanliness1to4	2.63	2.67	0.70	1.00	4.00	2,964	−0.04	0.13
Baseline Luxuriousness1to4	2.37	2.46	0.73	1.00	4.00	2,969	−0.10***	<0.01
Baseline Polish Brand Level	1.12	1.12	0.37	1.00	3.00	3,018	−0.00	0.74
Baseline Number of Services (Scope)	2.08	2.11	1.24	0.00	7.00	3,092	−0.02	0.59
Baseline Availability Next Day 4–5 p.m.	0.75	0.75	0.27	0.00	1.00	3,209	−0.00	0.95
Baseline Average Daily Opening Hour	09:44	09:43	00:31	06:00	14:00	3,075	00:01	0.40
Baseline Average Daily Closing Hour	19:14	19:15	00:50	13:00	23:25	3,074	−00:01	0.42
Baseline Price of Gel Manicure	29.29	29.35	8.06	10.00	105.00	2,806	−0.05	0.86
Baseline Price (Dollar Signs) on Yelp	1.77	1.79	0.52	1.00	4.00	3,008	−0.02	0.29
Baseline Yelp Rating	3.89	3.88	0.61	3.00	5.00	3,142	0.01	0.49
Baseline Number of Yelp Reviews	68.41	69.62	84.68	0.00	1,075.00	3,218	−1.21	0.69
Baseline Chain Status	0.03	0.04	0.18	0.00	1.00	3,218	−0.01	0.28
Yelp Canvass Week	32.95	34.39	5.33	24.00	44.00	3,218	−1.44***	<0.01

Notes. This table shows summary statistics and the balance of baseline variables collected by data collectors via phone calls or physical visits to the business. Variables from the Yelp platform on business engagement and performance that were used to perform a randomization check are excluded from this table because of the data-sharing agreement. Variables collected by physical visits (e.g., cleanliness and luxuriousness) are not available across the full sample as data collectors were sometimes unable to collect these measures (e.g., if the business was closed). *Baseline Price* refers to the regular manicure price. *Baseline Number of Employees* and *Baseline Number of Customers* count the total number of employees and the total number of customers who were observed at the time of visit, respectively. Cleanliness and luxuriousness are coded on a scale from one to four as detailed in Table C.1 in the Online Appendix. Polish brand level is coded on a scale from one to three based on the retail price of the most expensive nail polish brand observed. The number of services counts the total number of service types offered by the firm (e.g., spa services, haircuts, hair removal, makeup, tanning, and tattoos and piercings). *Baseline Availability Next Day 4–5 p.m.* is a binary variable collected by data collectors when inquiring for an appointment between 4 and 5 p.m., a peak hour for salon services. *Yelp Canvass Week* measures the week that canvassers visited each firm. SD, standard deviation.

***$p < 0.01$.

Figure 5. (Color online) Share of Firms That Changed Prices Across Months



Notes. (a) Raw share of firms that changed prices. Panel (a) plots the raw share of control and treatment firms that changed their price from their baseline price by the number of months since the canvassing visit, pooling across months for which data are available for the full sample. Each month begins on the 15th of each calendar month in order to count months following the canvasser visit, which began on June 18, 2018. The figure displays outcomes across the six months for which data are available for the full sample. Because of the staggered timeline of visits across the 12 months of data collection, firms visited between June 15 and July 14 only had one month of previsit data (the baseline price), whereas firms visited between October 15 and November 14 had only five months of postvisit data. (b) Estimated treatment effects for price change. Panel (b) plots the estimated treatment effects with 90% and 95% confidence intervals.

charged a different price from their baseline price across months following the canvassing visit.[27] At the time of the canvassing visit, about 12% of firms had changed their prices relative to baseline, which may reflect promotions captured at the time of the phone call as well as changes in prices between the baseline and the first month of data collection. There was little difference in this dimension between the control and treatment groups as expected from randomization and the balance of baseline variables.

In the months following the canvasser visit, both control firms and treatment firms showed an increasing likelihood of price change relative to baseline, as a larger percentage of postvisit months coincided with seasons when firms traditionally change their prices because of variation in demand. They were more likely to use promotions in slower months (fall and winter),[28] and generally changed menu prices at the end of the year between December and January. These patterns, shown in Figure F.1 in the Online Appendix, were confirmed by managers and documented in industry magazines and the broader retail economy (Nails Magazine 2008, 2018; Nakamura and Steinsson 2008).

Figure 5(b) shows that treatment firms were more likely to change prices compared with control firms following the canvasser visit. Because firms generally changed menu prices at the end of the year, treatment effects appear to increase over time as treatment firms show a visible jump in December (Figure F.2 in the Online Appendix) and these post-December months comprise a larger share of observations as the number of months since treatment increases.

To quantify the difference more precisely, I turn to regressions.

5.1.2. Empirical Specification. My main empirical specification leverages a difference-in-differences model as preregistered:

$$y_{iswt} = \beta_0 + \beta_1 Post_{iswt} * Treat_{isw} + \beta_2 Post_{iswt} + \beta_3 Treat_{isw} + \gamma_w + \varepsilon_{iswt}, \tag{1}$$

where y_{iswt} is the outcome of interest for firm i in randomization strata s visited in week w, measured at month t. The primary outcome of interest is whether firms adjust their pricing, which was measured by a binary variable indicating whether a firm's manicure price each month differs from the baseline price (May 2018). I decompose this price change into a price increase or decrease.

$Post_{iswt}$ is an indicator that takes the value of one for firms in either control or treatment starting from the month that they are visited by a Yelp canvasser until the end of the study and zero otherwise. $Treat_{isw}$ is an indicator that takes the value of one for firms assigned to treatment and zero otherwise. γ_w controls for canvasser visit week fixed effects, and ε_{iswt} is an idiosyncratic error term.[29] Because the unit of randomization is the firm, standard errors are clustered at the firm level (Abadie et al. 2023).

β_1 identifies the differential change in outcome variables for treatment firms relative to control firms after

Table 2. Price Changes Across Control and Treatment Firms

Panel A: Price changes

	(1) *Price Change*	(2) *Price Change*	(3) *Price Change*	(4) *Price Change*
Post × Treat	0.029**	0.028**	0.030**	0.030**
	(0.013)	(0.013)	(0.013)	(0.013)
Controls	Yes	Yes	Yes	Yes
Visit week FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Strata FE	No	No	Yes	Yes
Observations	30,142	30,142	29,552	29,552
Mean (control in months after visit)	0.173			
SD (control in months after visit)	0.378			

Panel B: Direction of price change

	(1) *Price Decrease*	(2) *Price Increase*	(3) ln(*Price*)
Post × Treat	0.005	0.023**	0.023***
	(0.006)	(0.011)	(0.009)
Controls	Yes	Yes	Yes
Visit week FE	Yes	Yes	Yes
Observations	30,142	30,142	30,142
Mean (control in months after visit)	0.036	0.137	2.580
SD (control in months after visit)	0.185	0.344	0.304

Notes. This table shows ITT estimates of the competitor information treatment on firms' likelihood of changing prices. In panel A, the dependent variable is a binary indicator of whether the firm's regular manicure price in a given month is different from its baseline price. In panel B, the dependent variables are a binary indicator of whether the firm's regular manicure price is lower (column (1)) or higher (column (2)) than its baseline price and the logged price (column (3)). Observations are at the firm-month level. All regressions control for any baseline differences between control and treatment groups, an indicator for months post-canvasser visits, and fixed effects (FEs) for the week of the canvasser visit. Columns (2)–(4) in panel A additionally control for randomization strata FEs and/or month FEs. Standard errors are clustered at the firm level. SD, standard deviation.
$^{**}p < 0.05$; $^{***}p < 0.01$.

the canvasser visit and is the main coefficient of interest. β_2 captures the passing of time and any effect of a canvasser visit across all firms, and β_3 identifies any pretreatment differences between treatment and control firms. The key identifying assumption is that firms assigned to treatment did not have systematically different trajectories from those in the control group for reasons other than the competitor information treatment, which was randomized.

5.1.3. Estimated Effects on Pricing. Panel A of Table 2 shows the intention-to-treat (ITT) estimates of the competitor information on firms' likelihood of changing their price. Treatment firms were significantly more likely to change prices by three percentage points (p = 0.023). This point estimate represents a 17% increase relative to 17.3% of control firms that change prices after the canvassing visit. Estimates of the treatment effect are stable across all specifications, which control for any previsit differences between control and treatment firms, the passing of time, and the week of the canvasser visit, with columns (2)–(4) in panel A of Table 2 additionally controlling for month and/or randomization strata fixed effects. These results are also robust to adding canvasser fixed effects, which are reported in Online Appendix L.[30]

In comparison with the benchmark assumption that firms' decisions are conditioned on the observable decisions of their competitors, this suggests that some firms may not have been knowledgeable of competitor prices, and yet, that this information is decision relevant.

Consistent with this, 19% of treatment firms showed surprise and direct interest in the competitor information received during the canvassing visit, and indicated that they intended to change their prices (Figure A.7 in the Online Appendix). For example, one canvasser note indicated: "Manager was surprised that her salon charges the lowest price in the area. She is thinking of raising her prices." Another noted that the owner expressed surprise that a competitor charged $45 for a manicure and that she planned to research what this firm offered to see how she might be able to raise her prices. Moreover, 4% of treatment firms directly requested more information on competitors' other decisions, and 65% of all treatment businesses signed up to continue receiving this information. Canvassers rated firms' interest in the pricing information with mean and median ratings of four on a scale from one (uninterested) to five (highly interested).

Treatment firms on average increased prices (panel B of Table 2). Column (1) in panel B of Table 2 shows that 3.6% of observations among control firms showed a price decrease relative to the baseline in the months following the canvasser visit, and treatment firms were 0.5 percentage points (p = 0.388) more likely to decrease their prices in the postvisit period, which is imprecisely estimated. Column (2) in panel B of Table 2 shows that treatment firms were 2.3 percentage points (a 17% change; p = 0.036) more likely to increase their prices in the postperiod compared with 13.7% of observations among control firms. These changes resulted in a price increase of approximately \$0.30 (a 2% change; p = 0.009) on the average price of \$13.20 among control firms (column (3) in panel B of Table 2). These results are robust to adding randomization strata and canvasser fixed effects (Tables L.2 and L.4 in the Online Appendix).

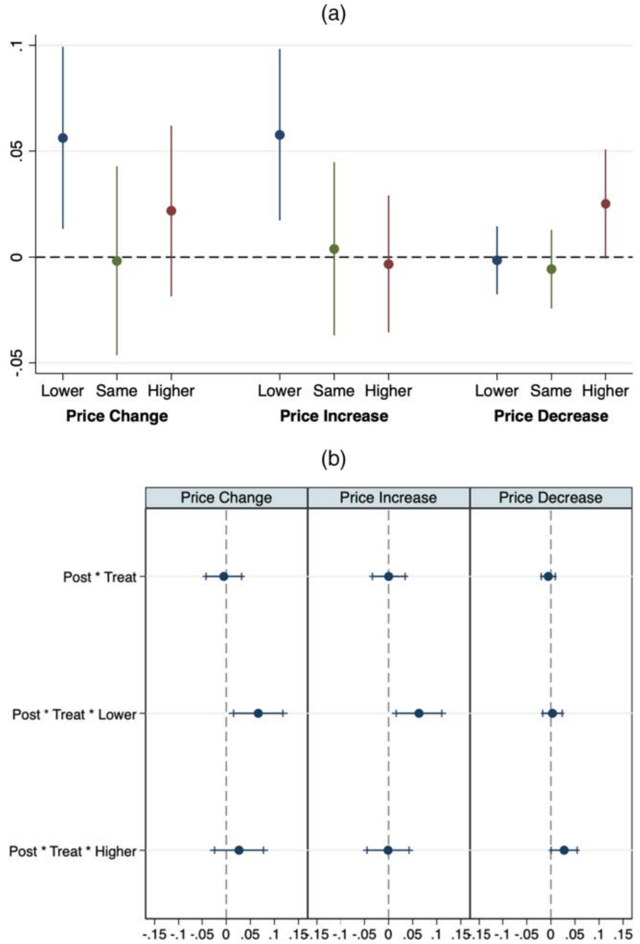
The magnitude of the effect is relatively modest, which may be reasonable given the light-touch nature of the treatment intervention—additional information on the back of a postcard along with a few additional minutes of conversation on a single day of the year. It is also worth noting that any spillover effects, which would violate the stable unit treatment value assumption, would bias any treatment effect estimate downward because control firms should be more likely to change prices as they become aware of competitor information. When surveyed after endline to explore potential spillover effects, 28 control salons (less than 1.5%) stated that they heard about postcards from another salon (Table J.1 in the Online Appendix). I exploit variation in the share of treated firms across zip codes to explore if control firms in markets with a higher share of treated firms were more likely to change prices, but find limited supportive evidence (Figure J.1 and Table J.2 in the Online Appendix).

5.2. How Do Firms Change Their Pricing?

Analyzing prespecified dimensions of heterogeneity, I find that treatment firms align their decisions with those of their nearest competitors rather than differentiating from them. As discussed in Section 2.2, differentiating would mean that firms shift their pricing and quality decisions to be farther away from their competitors rather than aligning and moving closer to their competitors. This would imply that firms that charged the same price as their nearest competitors should be more likely to change their pricing. Furthermore, they should decrease their prices further if they charged lower prices compared with their competitors, and increase their prices further if they charged higher prices compared with their competitors. In contrast, aligning with competitors would mean that firms that charged the same price as their nearest competitors should be less likely to change their pricing. They should increase their prices if they charged lower prices compared with their competitors, and decrease their prices if they charged higher prices compared with their competitors.

Figure 6 shows treatment effects on price change decomposed into price increases and decreases (see

Figure 6. (Color online) How Firms Change Prices Relative to Their Nearest Competitor



Notes. (a) Estimated treatment effects by subgroup. Panel (a) plots estimates of treatment effects on price change, increase, and decrease, respectively (with 95% confidence intervals) by subsamples based on firms' baseline price positioning relative to their nearest competitor (i.e., whether the firm charged lower prices, the same prices, or higher prices compared with its nearest competitor). (b) Differences in estimated treatment effects across subgroups. Panel (b) shows estimates of treatment effects on price change, increase, and decrease by interacting a binary indicator of whether the firm charged lower or higher prices compared with its nearest competitor (i.e., the estimate for *Post* × *Treat* indicates the treatment effect for firms that charged the same price relative to the nearest competitor; the estimate for *Post* × *Treat* × *Lower* indicates whether the treatment effect for firms that charged less than the nearest competitor is statistically different). Observations are at the firm-month level, and all regressions control for any baseline differences between control and treatment groups, an indicator for months post-canvasser visits, and fixed effects for the week of the canvasser visit. Standard errors are clustered at the firm level.

Table G.1 in the Online Appendix for regression results). Firms that charged baseline prices that were lower or higher than their nearest competitor were more likely to change prices than firms that charged similar baseline prices. Moreover, Panel (b) of Figure 6 shows that those with lower baseline prices were six percentage points ($p = 0.028$) more likely to increase prices; those with higher baseline prices were three percentage points ($p = 0.094$) more likely to reduce them. This evidence of firms matching competitors is consistent with qualitative evidence from other industries, such as news (Boczkowski 2010).

Although I primarily examine pricing decisions because they can be adjusted faster and are easier to measure precisely, I also observe evidence that firms changed their quality decisions. Table H.1 in the Online Appendix shows that treatment firms were also 9% more likely to change their quality between baseline and endline compared with control firms and that they both increased and decreased quality. However, heterogenous treatment effects are mostly noisy, making it difficult to draw any clear conclusions.

Figures G.1 and G.2 in the Online Appendix report additional heterogeneous treatment effects in other dimensions, including firm size, age, baseline price, scope, and chain status.

6. The Impact of Competitor Information on Performance

The findings so far indicate that some firms were unaware of key competitor decisions on pricing despite its accessibility and that this information was decision relevant. They also show that when provided with this information, firms were more likely to adjust their prices to align with their nearest competitors. In this section, I explore the effects of this information on firm performance.

Columns (1)–(3) in panel A of Table 3 show that following canvasser visits, treatment firms received 15% more calls, page views, and map directions views on Yelp compared with control firms (all $p < 0.001$)—measures that have been shown to be positively correlated with sales (Dai et al. 2023).[31] I additionally find that treatment firms received 7% more customer reviews and 6%

Table 3. Performance Across Control and Treatment Firms

Panel A: Proxies of performance						
	(1) ln(*Calls*)	(2) ln(*Pageviews*)	(3) ln(*MapViews*)	(4) *Availability*	(5) *#Customers*	(6) *#Employees*
Post × Treat	0.148***	0.146***	0.145***	−0.027		
	(0.042)	(0.039)	(0.040)	(0.018)		
Treatment					0.248*	0.313***
					(0.127)	(0.110)
Controls	Yes	Yes	Yes	Yes	No	No
Visit week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,398	35,398	35,398	25,755	2,491	2,494
Mean (control)				0.772	3.148	3.960
SD (control)				0.420	2.751	2.409

Panel B: Platform engagement					
	(1) ln(*Login Days*)	(2) *Account Claimed*	(3) *Advertising*	(4) *Responses*	(5) ln(*Comments*)
Post × Treat	0.026	−0.002	0.006	0.013**	0.009
	(0.027)	(0.014)	(0.005)	(0.005)	(0.007)
Controls	Yes	Yes	Yes	Yes	Yes
Visit week FE	Yes	Yes	Yes	Yes	Yes
Observations	35,398	35,398	35,398	35,398	35,398

Notes. Panel A shows ITT estimates of competitor information on proxies of firm performance. Columns (1)–(3) show treatment effects on measures from Yelp: the number of calls to the business, page views, and map directions views, respectively. Column (4) shows treatment effects on a binary indicator of availability for an appointment the next day during a peak hour (4–5 p.m.) when asked via phone calls. Columns (5) and (6) show treatment effects on the number of customers and employees observed at endline visits. Panel B shows ITT estimates of competitor information on firms' engagement with the Yelp platform: (1) the number of days of log-ins, (2) claiming of a business page, (3) advertising purchasing, (4) the number of responses to inbound consumer questions, and (5) the number of comments on reviews. For all regressions except for columns (5) and (6) in panel A, observations are at the firm-month level, and regressions control for any baseline differences between control and treatment groups, an indicator for months post-canvasser visits, and fixed effects (FEs) for the week of the canvasser visit. For columns (5) and (6) in panel A, observations are at the firm level, and regressions control for the week of the canvasser visit. Standard errors are at the firm level. SD, standard deviation.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

more photos on Yelp compared with control firms (Figure I.2 in the Online Appendix), consistent with the interpretation that the number of customers increased. This also suggests that any changes made by the business were communicated to consumers on the platform as reviews and photos are highlighted on search results and the business page (Figures E.2 and E.3 in the Online Appendix).

Treatment firms also had 0.31 more employees (p = 0.004) and 0.25 more customers (p = 0.051) when visited at endline. This represented an 8% increase in both measures relative to control firms, suggesting that these treatment firms grew in size as has been interpreted in prior work (Chatterji et al. 2019). These firms also observed lower availability for a peak-hour appointment the next day (a three-percentage-point decrease; $p = 0.138$), suggesting that they were busier with less slack.

I use these measures to conduct back-of-the-envelope calculations to proxy revenues and put bounds on implications for profit. First, I multiply purchase intentions with the price charged each month to proxy monthly revenues. Although this analysis provides suggestive evidence that treatment firms observed higher revenues (Table L.1 in the Online Appendix), interpreting this measure as revenues requires the assumption that (1) each purchase intention is independent and leads to a sale, which likely overestimates the effect, and that (2) every customer purchases a regular manicure and not any other services, which likely underestimates the effect. Therefore, these estimates are useful as a directional result rather than to evaluate the magnitude of effects. Another back-of-the-envelope calculation relying on prior studies' estimates of correlations between Yelp page views and revenues suggests that treatment firms observed 4.8% higher revenues compared with control firms.[32]

I also conduct a back-of-the-envelope calculation to calculate bounds on profit margins. I sat with pilot salons to collect competitor information shown as treatment, which took a maximum of one minute per competitor to collect.[33] Assuming the highest minimum hourly wage ($15) across these cities, collecting this information costs $0.25 per competitor. This implies that the profit margin on additional customers would have to be smaller than 1.8% for the marginal cost of collecting pricing information on the nearest competitor to be lower than the marginal benefit for the average salon.[34]

Why might treatment firms have improved their performance? First, I find that the gains in purchase intentions were driven more by firms that were overpricing at baseline relative to their nearest competitor, which on average, decreased their prices (Figure I.1 in the Online Appendix). Furthermore, I find that firms that were over- or underpricing relative to their quality responded most to treatment, consistent with the interpretation that treatment led the firms that were mispriced or mispositioned to improve their decisions. Measuring the degree of misalignment in pricing and quality by taking the absolute error from the best-fit line regressing baseline price on quality and zip code fixed effects, I find that at baseline, firms that priced most consistently with the market exhibited higher proxies of performance, consistent with better management in general (Figure D.7 and Table D.1 in the Online Appendix). Treatment firms that were less aligned in their pricing relative to quality were more likely to change prices following treatment (Figure G.3 and Table G.2 in the Online Appendix), suggesting that they may have improved their decisions to price more consistently with the market.[35]

I find little supportive evidence that this increase in performance measures was driven by treatment firms increasing their engagement with the Yelp platform. Panel B of Table 3 shows that treatment firms were not significantly more likely than control firms to log in (2.6%; p = 0.348),[36] claim their page (−0.2%; p = 0.865), purchase advertising (0.6%; $p = 0.222$), or comment on reviews (0.9%; $p = 0.193$). Treatment firms were 1% more likely to respond directly ($p = 0.022$), but this measure reflects an increase in customer interest more than business engagement as firms must first receive a request for a quote or appointment to respond.

Although these results suggest that treatment may have resulted in improved firm performance, there are at least two reasons to be cautious about their interpretation. First, although quantity-based measures can provide insights into firm productivity and survival, they do not necessarily move together with profitability (Foster et al. 2008, Syverson 2011). Second, these performance effects are likely to stem, at least in part, from spillover effects encompassing some business stealing from control firms unless the market for nail services expanded over the period of the experiment. I explore this by leveraging the differential proportion of treated firms across local markets to analyze the extent to which control firms in markets with a higher share of treated firms were more likely to observe lower measures of performance (Table J.3 in the Online Appendix). Although I do not find supportive evidence, confidence intervals are large, and I cannot rule out large effects.

7. Mechanisms Underlying Treatment Effects and the Lack of Competitor Knowledge

7.1. What Drives Treatment Effects: Competitor Response or Learning About Demand?

These results show that information on competitor pricing increased firms' likelihood of changing their pricing decisions. There are two mechanisms through which competitor information could drive this outcome. First, it may be that competitor information allowed firms to learn about competitor decisions and led them to change their prices in response. Second, it may be that information on competitor pricing allowed firms to learn about demand, and this learning about demand—rather than response to competitors—drove their price change. In fact, canvassers' notes on how managers reacted to receiving the competitor information treatment show that some referred to demand in the area, whereas others referenced comparisons with competitors and how they planned to respond (Table M.1 in the Online Appendix).

Both effects could be present anytime that information on competitor pricing is provided, and these channels are conceptually and empirically difficult to cleanly separate; directly responding to competitors may involve or result in learning more about demand, and learning about demand may lead firms to be more likely to consider competitor decisions and respond to them. Nevertheless, I explore these channels to determine whether treatment effects are driven mainly or solely by learning about demand. If so, this would suggest that simply providing noncompetitor-related demand information would lead to the same treatment effects.

Although these mechanisms are difficult to fully disentangle, the evidence points largely to the competitor response effect, and I find limited evidence that treatment effects are likely to be driven by learning about demand alone. I leverage variation in how firms responded to competitors in the microarea around the firm. Firms in this industry are extremely closely located, with 75% of businesses having their ninth nearest competitor within 1 mile (1.6 kilometers, a 15-minute walking radius) and 50% of businesses having their nearest competitor within 0.08 miles (0.1 kilometers, a 2-minute walking radius) (see Figures M.1 and M.2 in the Online Appendix). Given that competitors located within a 15-minute walking radius are likely to be in the same demand market, I examine whether firms responded more to the average or median competitor in that radius (from which they can learn about demand) versus their nearest competitor (whose decisions may be more competitively relevant and salient). Treatment effects are larger and more precisely estimated when comparing how treatment firms change their decisions relative to their nearest competitor compared with their average or median competitor (Figure M.3 in the Online Appendix)—suggesting that firms were more responsive to their nearest competitor whose decisions are more competitively relevant rather than their average or median competitor in the same demand market from which they can learn more about demand.

7.2. Why Did Firms Lack Knowledge of Key Competitor Decisions?

Given that information on competitor pricing was accessible and decision relevant, the natural question is why firms did not previously use this information. Although conclusively answering this question is beyond the scope of this paper, I consider three possible explanations.

The first is what I refer to as *managerial inattention.* Recent research suggests that managers may be inattentive to important features, which may lead to biased underestimates of the value of information $\hat{v} < v$ such that the costs c of obtaining it outweigh $\hat{v}$ (i.e., $\hat{v} - c < 0$) (Bloom et al. 2013, Hanna et al. 2014). A parallel literature in cognition and strategy has investigated how managers rely on cognitive frames and mental models, which may be incomplete or inaccurate (Porac et al. 1989, Ocasio 1997, Tripsas and Gavetti 2000, Baum and Lant 2003, Kaplan et al. 2003, Helfat and Peteraf 2015, Menon and Yao 2017). This research suggests that even when competitor information is valuable and firms know how to use it to improve their decisions, they may not pay attention to it sufficiently because of cognitive factors.

Several other explanations could also contribute to why firms lacked competitor knowledge, although they seem less supported by the evidence. One explanation is that the value of competitor information may be lower relative to costs for some firms such that $\underline{v} - c < 0$, and the competitor information treatment marginally lowers the cost for these firms with $\underline{v} < v$. For example, this is likely to be the case for businesses in less competitive markets as well as those run by "lifestyle entrepreneur[s]" with little desire to grow (Hurst and Pugsley 2011).

Another explanation is that v varies across firms depending on their complementary capabilities (Milgrom and Roberts 1990, Bloom et al. 2012).[37] This would imply that firms that did not know competitor prices were those that lacked relevant pricing capabilities to process the information to improve their decisions and that treatment led firms with these capabilities to adjust their prices marginally earlier than they otherwise would have on their own.

Although these possible explanations cannot be fully empirically distinguished, a follow-up experiment and analyses of heterogeneous treatment effects offer some speculative evidence. At endline (between May and August 2019), managers across all firms were asked a series of questions on their competitors, incentivized with a $10 gift card if they answered all questions correctly.[38] I randomly assigned control firms from the main experiment to one of two experimental conditions, which varied the sequence of when these questions were asked to infer whether managers underestimated the value of attending to competitor information when not prompted to first evaluate how outdated their knowledge might be. Half of the control firms were assigned to be "asked first" whether they would like to sign up to receive competitor information for free (showing a sample treatment postcard for a salon in a different city) before being asked questions to reassess their knowledge. The rest were assigned to be "asked last" to sign up for this information after first answering questions about their competitor knowledge.

Across 1,405 control firms,[39] I find evidence consistent with the interpretation that managers were inattentive to competitor information until prompted to reassess their knowledge. Firms assigned to reassess their knowledge before being asked to sign up for competitor information ("asked last") were four percentage points (p = 0.089) more likely to sign up over a base of 22% of firms assigned to "asked first," representing an 18% increase (Table 4). Furthermore, 45% of these "asked last" firms that signed up for competitor information stated that this information was helpful because they had not looked at their competitors in a while, with half (22%) additionally stating that they planned to change prices.

I also explore the other two explanations by analyzing heterogeneous treatment effects. I find limited evidence that treatment effects were driven by firms with a lower value for competitor information (e.g., because of lower competition or lower sophistication as "lifestyle entrepreneurs") as treatment firms that faced higher levels of competition—firms that are likely to value this information more highly—were more likely to change prices and observe performance improvements following treatment (Tables G.3 and I.4 in the Online Appendix).[40] I also find limited supportive evidence that firms lacked knowledge because they did not have complementary capabilities to leverage competitor information. Firms that did not use demand-based promotions at baseline, which may reflect a capability to understand customer demand fluctuations, appear to be more likely to respond to treatment and observe improvements in performance, suggesting that treatment effects were not driven by firms with these capabilities adjusting

Table 4. The Effect of Re-Evaluating Competitor Knowledge on Demand for Information

	(1) *Information Signup*
Signup Asked Last	0.039* (0.023)
Constant	0.218*** (0.016)
Observations	1,405

Notes. This table shows ITT estimates from the follow-up experiment on control firms, showing the effect of asking firms to first reassess their competitor knowledge ("*Signup Asked Last*") rather than after on whether the firm signed up to receive free competitor information ("*Information Signup*"). Observations are at the firm level, and they include all control firms that were available for a conversation. Standard errors are robust at the firm level.

*$p < 0.10$; ***$p < 0.01$.

their pricing earlier (Tables G.4 and I.5 in the Online Appendix).[41]

Although speculative, these results raise the possibility that firms may be inattentive to competitor information because of cognitive factors that lead them to underestimate its value. This is consistent with evidence found in other contexts, such as manufacturing firms in India (Bloom et al. 2013), farmers in Indonesia (Hanna et al. 2014), large grocery chains in the United States (DellaVigna and Gentzkow 2019), and SMEs in China (Cai and Szeidl 2018), where managers appear to have underestimated the gains of a practice that could improve firm performance.

8. Discussion and Conclusion

I find that in the personal care industry, a large percentage of firms appear to be unaware of competitor prices, a key strategic lever in this market, even when this information is easy to obtain and leads to higher proxies of firm performance. I find suggestive evidence consistent with the interpretation that this lack of knowledge may be driven by managerial inattention, and firms that receive information on competitor pricing change their decisions by increasing alignment with competitors. These findings highlight that limited information processing may be a key problem for firms and a central issue in strategy.

This study focuses on the personal care industry, where strategic simplicity enables precise empirical measurement and a high degree of internal validity, which is especially important for early tests of theory (List 2020). This, however, raises a question about generalizability. This study examines thousands of local retail firms across multiple geographic markets, making the findings more likely to be representative of other retail SMEs that are similarly differentiated with pricing as one of the key drivers of competition. SMEs like these represent a major segment of the economy, as firms with fewer than 20 workers represent 89% of all U.S. establishments—thus accounting for much of the sample in studies that use census data to understand firm behavior, as well as studies of small entrepreneurial businesses that are of increasing interest to many scholars (Camuffo et al. 2020, Zolas et al. 2021, Azoulay et al. 2022).

How far they apply to larger firms in other industries is an open question for future study. Larger firms have more resources to track competitor prices, and they often have more dimensions on which to differentiate. Therefore, the specific finding that firms are not informed of their competitors' pricing may be less likely to be applicable to larger firms.

However, the broader mechanism of inattention to key competitor decisions may not be limited to small firms, given that larger firms also have more complex strategy spaces and many more competitive dimensions beyond pricing that they could be uninformed of. Examples of managerial frictions and limited information processing have been found to exist in large firms across contexts as varied as manufacturing (Bloom et al. 2013), pharmaceuticals (Kaplan et al. 2003), airlines (Hortaçsu et al. 2024), technology (Eggers and Kaplan 2009), and grocery stores (DellaVigna and Gentzkow 2019)—suggesting that even large firms may have similar problems with competitor decisions. In fact, Leisten (2021) finds that hotels affiliated with large chains do not have better information about market demand when setting prices compared with hotels affiliated with smaller chains, which raises the possibility that large firms may also be uninformed about some key competitor decisions. Moreover, *how* firms change in response to competitor information—by aligning with their competitors—may be a result that is more generalizable across different contexts. Whether this is the case is an empirical question for future study.

More broadly, data on the competitive environment are becoming increasingly available across many markets. The findings in this paper suggest that although many firms—even in competitive markets—may be farther away from the productivity frontier in their positioning than we may expect, simply making information accessible may not be sufficient to change firm decisions. Firms may have different cognitive frames that drive their attention, and understanding what drives these differences and designing mechanisms to overcome inattention may be a fruitful direction for future work, especially in contexts like online platforms that introduce information into their marketplaces in the hope of optimizing their supply side (Huang 2022).

Finally, studies across various industries have documented increasing similarity across competing firms over the past few decades (e.g., Boczkowski 2010). These findings raise the possibility that data about the broader market may be a potential driver of this similarity by leading firms to make decisions that align more with their competitors. However, this study focuses on a single context and a particular type of competitor information on pricing. Exploring the extent to which this pattern generalizes across other settings and other types of information is an important question for future work, to provide a better understanding of how the availability and use of data may change the competitive landscape.

Acknowledgments
The author is grateful to Rebecca Henderson, Michael Luca, Amanda Pallais, and Dennis Yao for their support and encouragement throughout this project. This project would not have been possible without collaboration from Yelp (Darnell Holloway, Genna Gold, Ali Schwartz, Emily Washcovick, Matt Donzella, Sebastien Couvidat, and Wolfe Styke); administrative support from Jennifer Mucciarone and Angela Valvis; excellent research assistance from Alice Dai, Sarah Xu, Byron Perpetua, Dilhan Perera, Robert Courtney, Jiahao Huang, Fabian Konig, Claire Liu, Yao Xen Tan, Nicholas Young, and Ruijing Chen; and the tireless work of all of the data collectors. The author also thanks Juan Alcacer, Susan Athey, Nicholas Bloom, Yo-Jud Cheng, Ashley Craig, Christine Exley, Shane Greenstein, Rem Koning, Josh Krieger, Elizabeth Linos, Hong Luo, David Martin, Frank Nagle, and many seminar participants for valuable comments. The ability to publish is not tied to the results of this paper. This experiment was approved by the Harvard University and INSEAD Institutional Review Boards, and was preregistered on the AEA RCT Registry under trial number AEARCTR-0003013.

Endnotes
[1] Cyert and March (1963) documented in their book how a department store priced its products by rounding up the cost and multiplying it by a constant—with no consideration of competitor prices. A number of studies across other industries, such as hotels and manufacturing, have suggested that firms may lack knowledge of key competitors because of the costs of monitoring rivals' decisions (Li et al 2017), barriers to acquiring competitor information (Bloom et al 2013), or cognitive filters and categorization that lead them to overlook some competitors altogether (Porac et al 1989, Baum and Lant 2003, Thatchenkery and Katila 2021).

[2] A related body of research examines how access to information affects firm prices and price dispersion (e.g., Jensen 2007, Grennan and Swanson 2020) as well as the consumer implications of information disclosure more broadly in homogeneous consumer good markets (e.g., Stigler 1961, Varian 1980, Sorensen 2000, Baye et al. 2006, Brynjolfsson and Smith 2000, Ellison and Ellison 2009, Pennerstorfer et al. 2020). This literature has generally focused on the role of consumer search frictions. Smith et al. (1999), Sorensen (2000), and the literature that followed (see Baye et al. 2006 for a review) document persistent and pervasive price dispersion across various markets, and they show how prices and price dispersion are lowered by the frequency of purchases, exposure to the internet, and policies on price transparency, which are likely to increase consumer search. Related work has also documented the effect of information disclosure on market-level prices and margins in homogeneous product markets given that this may also make it easier for firms to monitor their rivals' actions, potentially facilitating coordination (Byrne and de Roos 2017, Luco 2019). This paper is distinct in two ways to this literature. First, it isolates the effect of competitor information on *firms* rather than *consumers*, unbundling these two channels. Much of this literature has focused on the latter, assuming costly price search on the consumer side à la Varian (1980) and Stahl (1989) but not frictions on the firm side (Ellison et al 2018). In highlighting this channel of *firm* responses to competitor information, this paper proposes a supply-side explanation rather than a demand-side explanation for price dispersion. Second, this paper focuses on how individual firms respond rather than market-wide effects on price dispersion, price levels, or estimated margins. Although other papers have used firm pricing to estimate market-level price dispersion or price levels, the focus has not been to distinguish *how* firms change their prices relative to their competitors, *which* firms change, and *what* the resulting performance implications are for individual firms—which are critical to understand for competitive strategy.

[3] Related research on management practices has documented a large variation in the knowledge and adoption of basic management practices across firms (Bloom and Van Reenen 2007, Bruhn et al. 2018), but it does not focus on competitor information and strategic behavior.

[4] In his 2019 letter to shareholders, Jeff Bezos stated that he believed that it was important to focus on customers, not competitors. Larry Page has been widely cited as stating, "You don't want to be looking at your competitors."

[5] I analyzed all local business verticals on Yelp, including dry cleaners, restaurants, and florists, and assessed them based on market and sample size, comparability and observability of price positioning, and competitor information accessibility.

[6] Small Business and Entrepreneurship Council (2019) are defined by the U.S. Small Business Administration as firms with fewer than 500 workers. Firms with fewer than 100 workers account for 98% of employer firms, and firms with fewer than 20 workers make up 89%. SMEs represent 47% of employment and 46% of GDP.

[7] Verification means that the business claimed their free page on Yelp and verified that the listing was a true business.

[8] Correctly located meant checking that the actual location matched the listed location and that the business was not located inside an airport.

[9] All firms in Los Angeles and Chicago and most firms in New York and San Francisco were reached, excluding areas farther out (the Bronx and the outer areas of Queens for New York and North Bay for San Francisco).

[10] Data collectors were undergraduates and master's degree students recruited using job postings across every university in the four cities posted every three to six months. They were selected after an interview asking questions about data validity and collection methods. Over the course of the project, 83 data collectors were hired. Data collectors also noted whether the phone number was no longer in service, no one answered, nail services were no longer offered, the business was permanently closed, or the business refused to provide prices over the phone. Because of these reasons, data collectors were not able to obtain a price every month for each salon, resulting in an unbalanced panel.

[11] Polish brands ranged from one to three based on the retail price per bottle, and cleanliness and luxuriousness were rated on a scale from one to four. Results are robust to using a standardized sum of polish brands, cleanliness, and luxuriousness or each individual measure alone (see Figures D.1 and D.2 and Table C.1 in the Online Appendix). Although reviews and photos on Yelp may potentially provide a subset of these data points for some businesses, they are collected at different points in time and missing for a large percentage of businesses in the sample. Thus, collecting these data by physically visiting businesses improved measurement and ensured more comprehensive coverage across the sample.

[12] In order to prevent any suspicion from salons, the specific time within this hour was changed on a monthly basis.

[13] Canvassers classified any answers that appeared to be brush-offs as "did not answer" based on any disinterest in answering follow-up questions or continuing the conversation, which accounted for 6% of responses. This low brush-off rate may possibly be driven by the fact that Yelp was providing free assistance and information on these visits as well as the general perception by many retail businesses that Yelp is important for their sales.

[14] Both the treatment information and measures of competition were determined using the full sample of verified businesses in the area to identify the geographically closest competitors based on longitude and latitude coordinates.

[15] From this point onward, I only report results for distance from the nearest competitor when referring to competition levels, but results are robust to using the baseline price dispersion measure.

[16] The nine geographically closest competitors were determined using the full sample of verified businesses in the area based on longitude and latitude coordinates. This meant that information on businesses not in the experimental sample was included in these postcards. I chose to show nine geographically closest competitors because this number generally appeared to encompass most competitors that a given nail salon may consider, which varied substantially across markets. There were no cases in which equal numbers of competitors charged higher versus lower prices as nine competitors were shown on the postcard.

[17] This image was extensively piloted prior to the experiment on nail salons in Boston (outside of the experimental sample) to ensure that business owners and managers could easily understand the information.

[18] word2vec identifies words that share common contexts by computing cosine similarity between a mean of the projection weight vectors of the words and for each word in the model. This model is further described in detail in Gentzkow et al. (2019).

[19] Training spanned a full day, guiding canvassers through at least three hours of practice with the script and detailed data-recording steps followed by a few hours of canvassing visits together to confirm correct execution.

[20] Canvassers were part-time contractors hired for the duration of this project. They worked independently and were in constant communication with me and the Yelp managers throughout each daily shift.

[21] If they were still unable to do so by the third visit, canvassers left the brochure and postcard.

[22] Stratified randomization ensures that treatment and control groups are similar not just in expectation but also in practice in the sample along important observable dimensions. The variables for stratification were chosen mainly for logistical reasons and to limit potential issues from noncompliance. Yelp's team manager and canvassers were assigned based on metropolitan areas. Prior relationship with Yelp, which defines whether a business has claimed its free business page on Yelp and/or previously advertised with Yelp, and prior Yelp rating are likely to be correlated with the business's receptiveness to Yelp canvassers and any information that they might provide. Stratification can also improve precision to the extent that these variables explain the variation in the treatment of interest (Cox and Reid 2000). The randomization process was implemented using Stata.

[23] Data collectors were sometimes not able to visit the salon because of closure upon multiple tries or because of security at reception, leading to varying numbers of observations across variables.

[24] One possible reason for this lag is that there were times when a canvasser had to take a break because of personal reasons or it took longer to fill a canvasser role, leading to odd numbers of canvassers, which may have driven idiosyncratic differences. Another reason is that anecdotally, treatment canvassers sometimes had a harder time speaking with the owner or manager as they had to ask questions before providing information and were asked to come back at a different time. Because of the importance of this variable, I control for the week that each firm was visited in all specifications.

[25] I describe differences between the paper and preregistration in detail in Online Appendix K.

[26] Fewer than 2% of firms (58) were marked as noncompliant, which manifested in the form of firms rejecting any interaction with and information from Yelp canvassers when they arrived at the business. Attrition stemmed from permanent firm closures in the 12-month period (5% of firms), which were unlikely to be influenced by treatment, and from firms that could not be reached after canvassing visits (1% or 36 firms). Neither noncompliance nor attrition varied significantly across groups.

[27] Each month begins on the 15th of each month in order to count months following canvasser visits, which began on June 18th. The number of observations collected in each month varied because of

some firms not answering their phones or being closed. Because of the staggered timeline of visits, only firms that were visited in the first set of canvassing visits between June 15 and July 15 had observations 10 months after the canvassing visit. Similarly, only firms that were visited in the last set of canvassing visits between October 15 and November 15 had observations four months prior to the canvassing visit.

[28] In total, 24.7% of firms used promotions.

[29] Although all results are robust to adding randomization strata fixed effects, they are not included in the base specificationas they are not necessary for identification and were determined by logistical reasons to limit noncompliance rather than their likelihood of predicting the outcome (Imbens 2011, Lin et al. 2016).

[30] Given that canvassers were not aware of the experiment or price change as an outcome, were balanced in their assignments, and were strictly trained to use scripts, canvasser effects are less likely to drive outcomes. Adding canvasser fixed effects also drops all observations for 139 firms as a few canvassers left within a few weeks of being hired (balanced on assignment to control or treatment canvassing). Online Appendix L reports all results adding canvasser fixed effects, which show that the results are consistent.

[31] Because of restrictions in the data-sharing agreement, I am not able to publicly share the base level of the number of calls, page views, or map directions views for control firms.

[32] Using historical tax revenue data from the Washington State Department of Revenue, Dai et al (2023) regress logged revenue change on logged change in page views, restaurant fixed effects, and quarterly dummies for a matched set of 835 restaurants. They find that a 10% increase in quarterly page views is correlated with a 3.3% increase in quarterly revenue.

[33] They took on average 30 seconds per competitor to call and ask about their regular manicure price, and no one took more than 45 seconds.

[34] The mean baseline price is \$13.88, and 0.25/13.88 = 1.8%.

[35] These results are robust to different specifications (e.g., continuous, tertile, or quartile measures of misalignment).

[36] The upper end of the confidence interval on log-in days is high, but for any increase in log-in days to drive the change in customer calls, map views, or page views, firms would have to engage in activities such as purchasing advertising (column (3) in panel B of Table 3) or commenting on reviews (column (5) in panel B of Table 3), for which I find little evidence. Rather, it appears more likely that businesses may have logged in to respond to inbound customer messages (column (4) in panel B of Table 3) or to update their page to reflect changes in their prices or services.

[37] For example, firms may need a prior understanding of customer preferences across the market or analytic skills to process competitor data in order to use the information to their benefit (e.g., Dutta et al. 2003).

[38] The questions were as follows. (1) "What salon is located closest to you?" (2) "What do you think they are charging for a regular manicure?" (3) "How do you think your price compares with your two nearest nail salons?" Using these questions, I also find speculative evidence that treatment firms may have learned to pay attention as they were more likely to correctly guess their nearest competitors and their prices approximately 12 months after treatment (5–8 months after most price changes occurred) (Table N.4 in the Online Appendix).

[39] Firms were well balanced across experimental conditions, and attrition did not vary significantly across conditions (Tables N.1 and N.2 in the Online Appendix).

[40] This result that treatment firms that face higher levels of competition are more likely to respond is robust to using other cutoffs than the median, such as quartiles. This evidence raises the following question: Why do these firms survive? One explanation may be that we are observing short-run dynamics. Another explanation is that there is some friction that limits competition (e.g., that quality firms are capacity constrained), which reduces the strength of the selection mechanism in the market.

[41] In total, 10.1% of firms used demand-based promotions based on hours of the day or days of the week. Conversations with managers and owners supported the interpretation that the use of these promotions was linked to sophistication in pricing; those managers and owners who used it explained that they based these promotions on when they expected customer demand would slow as well as observed data on customer throughput. Cash or credit card discounts were not included in this coding as most firms used these discounts. I also excluded promotions for new customers, promotions for repeat visits, and group- and birthday-based discounts as these were also common and did not appear to indicate sophistication with pricing based on knowledge of fluctuating customer demand. However, the results are robust to using this broader definition of promotions.

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