The Value of Competitor Information: Evidence from a Field Experiment

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Abstract

To what extent are firms knowledgeable of 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 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. 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 make more similar decisions.

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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 has therefore often been implicitly assumed in theoretical models and interpretations of empirical analyses in strategy research. But to what extent do firms use the competitor information they have access to, and how do they change their own decisions when they learn more? These are questions fundamental to understanding how firms compete and make strategic decisions, especially as such data become increasingly accessible for firms (Brynjolfsson and McElheran 2016, Camuffo et al 2020, Koning et al 2022).

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 competitors (Cyert and March 1963, Porac et al 1989, Baum and Lant 2003, Thatchenkery and Katila 2021),[1] but these 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, while a rich literature of case studies and business teaching curriculum proposes that analyzing competitor decisions will lead firms to discover better strategies, there has been no large-scale causal evidence to support this view. A major challenge has been measurement: firm knowledge and decisions must be evaluated across a sufficiently large sample of firms across many markets with accessible information and varying degrees of competition, and the treatment effect of competitor information must be isolated from the non-random selection of firms that choose to invest in it.

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 varying 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 more with competitor decisions. 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 increasingly similar decisions to their competitors.

The experiment ran across personal care firms offering nailcare services, a mostly vertically differentiated $9.8 billion market in the U.S. that enables precise identification of competitor knowledge and its impact across thousands of firms in hundreds of local markets.[2] Collaborating

[1] Cyert and March demonstrated in their classic book (1963) documenting how a department store priced their 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 like hotels and manufacturing have suggested that firms may lack knowledge of key competitors, either due to 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] Firms compete locally, and can easily observe competitor prices online, via telephone calls, or even physically passing by, which enables me to study firms' knowledge of competitor decisions. Moreover, firms have simple strategy spaces along pricing and quality, and both decisions are easy to observe and compare. Every salon has a price for a regular manicure that varies from $5 to $60 and serves as the base price for other services. Quality positioning can be observed from the polish

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.[3]

To measure the impact of competitor information, I measured firms' baseline knowledge of competitors prior to treatment and constructed a data set of monthly prices and proxies of performance 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 regular manicure prices. They also physically visited firms (at baseline and endline) to observe their polish brands, cleanliness, and luxuriousness as measures of quality. To measure performance, I collected the number of customers and employees observed at the time of endline visits, an indicator of availability for a next-day appointment at peak hours, and the number of calls, page views, and map direction views of the firm on the Yelp platform.

At baseline, a large percentage of firms appeared to lack competitor knowledge, including those facing higher levels of competition. When asked who their primary competitors were and what prices they were charging, nearly half of managers at treatment firms were unable to state specific names or their prices, and their descriptions of their own price-quality positioning did not match their observed decisions relative to competitors.[4] Consistent with these patterns, firms offering similar levels of quality in the same ZIP code showed a large dispersion of prices, and firms with higher misalignment in pricing and quality observed lower proxies of performance.

Since many managers stated that they could easily acquire competitor pricing information, this dispersion may arise from unaccounted key attributes. Alternatively, it could be that competitor prices are not decision-relevant—either due to other information like residual market demand that offers sufficient statistics for competitor information, or 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 the information was valuable. Treatment firms were 3 percentage points more likely to change their prices relative to control firms in the months following the canvasser visit, a 17 percent increase. In principle, these effects could be driven by firms responding to competitors based on the information, or by learning about demand via observing their competitors' pricing decisions. While these two channels are conceptually and empirically difficult to cleanly disentangle,[5] the evidence points largely to the competitor effect. Moreover, 65% of treatment firms signed up to continue receiving this information, with 4% requesting more competitor information on other decisions, 18% actively asking follow-up questions, and 19%

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brands used, the cleanliness of the interior, and the luxuriousness of the decor. How these price and quality decisions are made are similar to other retail businesses—and of SMEs more generally, which make up a large percent of the economy.

[3] Among consumer reviews on Yelp prior to the experiment, 46% included words related to pricing, 35% of reviews referenced comparisons to competitors, 24% commented on cleanliness, and 17% commented on luxuriousness. The most frequent phrases also mentioned price and comparison to other salons, suggesting that customers search along these dimensions.

[4] This percentage excludes firms where managers were not willing to answer questions in general, appeared to brush off canvassers, or did not fully engage with questions that followed.

[5] Responding to competitors may involve or result in learning from them.

expressing intentions to change their prices based on the information—suggesting that increased price salience alone is unlikely to drive these effects.

Rather than differentiating by shifting their decisions to be farther away 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, while those charging less increased their prices. While I primarily examine pricing decisions as they can be adjusted faster and are easier to measure precisely, I also find that treatment firms were 9% more likely to change their quality between baseline and endline, both increasing and decreasing quality—consistent with the interpretation that treatment firms changed their positioning. Firms that were over- or under-pricing 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 performance. Treatment firms observed 8% more employees and customers at the business at endline, 3% lower availability for an appointment the next day, and 15% more calls, page views, and map directions views on Yelp. Treatment firms also received more customer reviews and photos on Yelp, indicating that any changes made by the firm were disseminated to other consumers via search results and business pages that prominently displayed prices and reviews. Back-of-the-envelope calculations on revenues also suggest consistent results. These performance effects were mainly driven by firms that were over-pricing relative to their nearest competitor. I observe little supportive evidence that firms increased their usage of the Yelp platform, as measured by their logins, 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 nail services expanded over the period of the experiment.

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. 75% of businesses had next-day availability during peak hours, and the market was characterized by relatively thin margins, high competition, and closure rates (J. Kim 2020). Collecting competitor information shown as treatment took managers a maximum of 1 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 average firm for the marginal cost of collecting this information to be lower than the marginal benefit.

While not conclusive, I consider a few possible explanations and find the most supportive evidence for managerial inattention. Interviews with 25 managers raised the possibility that this behavior may have been driven by a form of overconfidence that led managers to not update sufficiently and underestimate the value of paying attention – they believed they already knew it, 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 to *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 decision-making. Research on data-driven decision-making and information technology adoption has provided evidence that using more information in decisions is associated with higher firm performance (Camuffo et al 2020; Bloom et al 2012; Brynjolfsson and McElheran 2016; Bajari et al 2019, 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.

Second, research on management practices has documented how firms' lack of knowledge and adoption of best practices drives dispersion in firm productivity (Syverson 2011; Bloom et al. 2013; Bloom and Van Reenen 2007; Bruhn, Karlan, and Schoar 2018). One puzzle raised by this literature is why firms seem unaware of even commonly used management practices. This paper provides evidence on how widespread this phenomenon may be, even for first-order decisions like pricing in settings like the personal care industry with relatively low barriers to information and strong 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).

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). But, problems in measurement and identification have made it hard to evaluate how 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 due to prior (outdated) knowledge that leads them to be complacent to new information. Building on ideas proposed 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 create opportunities for competitive advantage.

Lastly, these findings complement research on broader economic phenomena. Price dispersion in contexts as diverse as prescription drugs (Sorensen 2000), gasoline (Lewis 2008), and online consumer goods (Brynjolfsson and Smith 2000) have provided demand-side explanations for observed price dispersion, such as the presence of consumer search frictions. This paper proposes a supply-side explanation for price dispersion, which stems from a lack of knowledge of competitors. More broadly, this paper isolates *firm* responses to competitor information rather than *consumer* search, unbundling these two effects in a differentiated market.[6]

[6] A related body of research examines how access to information affects firm prices and price dispersion (e.g. Jensen 2007, Grennan and Swanson 2020), and the consumer implications of information disclosure more broadly in homogeneous consumer good markets (e.g., Stigler 1961, Varian 1980, Sorensen 2000, Ellison and Ellison 2009, Baye et al 2006), Pennerstorfer et al 2020). This literature has generally focused on how access to information affects price dispersion, especially focusing on the role of consumer search frictions. Much of the empirical work advances and tests theories from seminal papers by Stigler (1961) and Varian (1980) on a search-theoretic explanation for price dispersion, driven by consumers who are uninformed about prices. Sorensen (2000), Smith, Bailey, and Brynjolfsson (1999), and the literature that followed (see Baye, Morgan, and Scholten (2006) for a review) document persistent and pervasive price dispersion across various markets, and 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 drive consumer search. Byrne and de Roos (2017) use

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. 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 environment. While the idea of blind spots or awareness of peripheral competitors has received much attention in strategy frameworks (Chen et al 2007, Porac et al 1989, Baum and Lant 2003, Thatchenkery and Katila 2021), knowledge of key decisions taken by competitors—once identified—has often been implicitly assumed. For example, research on competitive interactions and strategic positioning often analyzes firms' decisions relative to their competitors' 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', the implicit assumption in these studies is that firms are aware of their competitors' decisions and are responding to them, although their knowledge or motivation behind decisions are often 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).

gasoline station-level pricing data and an indicator of consumer web search to show that consumers engage in both cross-sectional and inter-temporal search. Pennerstorfer et al (2020) study how price dispersion in the market is related to the share of informed consumers, theorizing a U-shaped relationship. Notably, Ellison and Ellison (2009) also consider firm responses, showing that easy price search makes consumers more price-sensitive among internet retailers, but firms engage in obfuscation to frustrate 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 (2019) document cyclical pricing jumps across gasoline stations in Perth, Australia over 15 years, and provide evidence consistent with firms learning to coordinate with the market leader and resulting in higher estimated overall margins in the market. Luco (2019) examines the impact of a regulation in Chile requiring gas stations to post and update their prices on a government website, finding that this policy increased market-level margins on average, but varied depending on the level of consumer search in the area.

This paper is distinct in two ways to this literature. First, it isolates the effect of competitor information directly for *firms* rather than *consumer* or overall market-wide effects, unbundling these two channels. Much of this literature has focused on the latter, assuming costly price search on the consumer side a la Varian (1980) and Stahl (1989), but not frictions on the firm side. If firms also experience frictions in obtaining pricing information – as much of the management and strategy literature suggests (e.g., Simon 1947, Cyert and March, Li et al 2017) and increasingly in economics as well (e.g., Cho and Rust 2010, DellaVigna and Gentzkow 2019, Hortacsu et al 2021), it is important to understand how firms change their decisions to strategically respond to competitor information as digitization makes this information easier for firms to acquire. Second, this paper focuses on how individual firms respond, rather than market-wide effects on price dispersion, price levels, or estimated margins. While 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. Moreover, much of this literature has examined homogeneous product markets like gasoline that do not vary in product attributes across firms and where pricing may be less of a strategic variable, rather than differentiated product markets where firms are strategically positioning relative to their competitors on various attributes. This paper examines a differentiated product market where firms differentiate on quality as well as some horizontal attributes to examine how firms change their decisions.

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.[7] While 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 both stated and revealed measures of competitor knowledge using a field experiment, by analyzing whether firms that are randomly assigned to receive competitor information change their decisions.

2.2 How competitor information may impact firm decisions

While 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 2016), 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 not need to know competitor decisions, 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.[8] While 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 to their competitors (Porter 1980, Greenstein and Mazzeo 2006). Using competitor data may similarly lead firms to move to a better position, resulting in firms shifting their pricing and quality decisions to be farther away from competitors' such that they end up being more spread out in their positioning. This would suggest that when firms receive competitor information, they

[7] A related body of research examines how access to information affects firm prices and price dispersion (e.g. Jensen 2007, Grennan and Swanson 2020), and the consumer implications of information disclosure more broadly (discussed in detail in footnote 8). This work has generally focused on market-wide price transparency in homogeneous consumer good markets where price may be less of a strategic variable, analyzing observational data. Research on management practices has also documented large variation in the knowledge and adoption of basic management practices across firms (Bloom and Van Reenen 2007, Bruhn, Karlan, and Schoar 2018), but does not focus on competitor information and strategic behavior.

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

decrease their prices further if they charged lower prices compared to 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 to their competitors and decreasing prices if they charge higher prices compared to 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 Setting and Context

Any empirical study must choose a setting, and in-depth industry studies have long uncovered valuable empirical facts. Studies of hotels have provided insights on firm positioning, learning, and competitor perception (Baum and Haveman 1997, Baum and Ingram 1998, Baum and Lant 2003, Li et al 2017). Pizza stores offered evidence on how organizations acquire and transfer knowledge (Darr, Argote, and Epple 1995). Taxicab companies enabled a study of coordination costs and organizational rigidity (Rawley 2010). Studies of fishing boats in India, pineapple farms in Kenya, local gas stations, and ready-mix concrete have provided detailed insights on issues as diverse as firm productivity, learning, and pricing (Conley and Udry 2010, Jensen 2006, Lewis 2008, Syverson 2004). In each of these papers, grounding the question in a case study of a particular industry helps identify precise measures of concepts and uncover new hypotheses or puzzles from the richness of the context.

Finding a market to study whether and why firms lack competitor knowledge and how this impacts their strategic choices like price positioning imposes many requirements. First, it requires a large sample 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 can be challenging. Even in a relatively simple market like cafés, 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.

After assessing many possible industries on these criteria,[9] I chose to study personal care businesses that offer nail services, which enables precise identification of firms' knowledge of competitor decisions and its impact across thousands of firms in hundreds of local markets with varying degrees of competition. It is a $9.8 billion market in the U.S. (IBISWorld 2019)—which is slightly larger than the market for men's clothing stores (~$8.5 billion) and slightly smaller than egg production (~$10.5 billion) (IBISWorld 2019). The market is competitive and fragmented, but there are also large chains such as Regal Nails, with more than 800 salons across multiple countries and over $1.15 million in annual revenues. Many salons represent entrepreneurial endeavors, often founded by immigrants and women who pursue entrepreneurship as a career alternative (Nails Magazine 2015). While some consumers are loyal to one business, the market is generally characterized by more consumer search than 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 markets among local businesses and compete locally. Appendix Figure F.1 shows that 94% of consumer search occurs within a radius of 5 miles even in a geographically dispersed city like Los Angeles, suggesting that competition is fairly local. This provides a large sample of thousands of firms across hundreds of local markets to evaluate the impact of competitor knowledge depending on firm attributes and degree of market competition.

Second, nail salons are differentiated (mostly vertically), but have simple strategy spaces where pricing is a key competitive driver.[10] Pricing and quality decisions are comparable and observable. 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), and generally vary from $5 to $60 depending on quality. 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 SMEs more generally, which make up 99.7% of U.S. establishments and represent 46% of GDP.[11]

Finally, information on competitor pricing is easily accessible, enabling me to study why firms might lack competitor knowledge even when this information is easy to obtain. Consumer reviews on Yelp indicate that price comparisons are a key consideration (Appendix Figure F.4), 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 in the sample were aware of Yelp, and most had a competitor within 0.5 miles that they passed by on their way to work. Obtaining the information on competitors' prices (provided as treatment) took less than a one-minute phone call per competitor.

[9] I analyzed local business verticals including drycleaners, florists, and restaurants based on market (and sample) size, comparability and observability of price positioning, and competitor information accessibility.

[10] Competition can thus be represented using a differentiated Bertrand model. While canonical models of vertical and horizontal differentiation assume perfect information on competitor decisions, information on competitor prices can be seen as a price change, as firms discover the up-to-date price which may be different from the price they had in mind. Both models predict that best replies in pricing are increasing with the competitor's price and that profits should increase, which are consistent with the results I observe.

[11] SMEs 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. (https://sbecouncil.org/about-us/facts-and-data/).

I partnered with Yelp, an online platform that crowdsourced listings and reviews of local businesses, to deliver the treatment information in a more natural manner. As of June 2018, Yelp listed over 4.6 million verified[12] businesses including restaurants, home services, beauty salons, and fitness centers, accumulating 163 million reviews and 74 million unique desktop and 72 million mobile visitors on a monthly average basis (Yelp 2018). Yelp displayed business listings with location information sourced by an internal team, user reports, and partner acquisitions, and checked by an internal data quality team. Yelp also provided reviews and photos that reflected business decisions, as well as proxies of business performance such as the number of calls to the business, views of map directions to the business, and business pageviews. It had a free business dashboard for businesses to observe information about their reviews, where Yelp could, in theory, provide information about their competitive context.

I collaborated with Yelp by scaling up a marketing initiative that sent canvassers to physically visit local businesses. At the time of the experiment, Yelp was making marketing visits to a handful of businesses each year to inform them about Yelp's free business page. I expanded these efforts and added an information intervention on top of their standard marketing visit for businesses assigned to treatment, which enabled me to verify that businesses saw this information.

4 Experimental Design

4.1 The competitor information intervention

All firms in the experimental sample received a physical visit from a Yelp canvasser. The canvasser provided a marketing brochure with information on how to edit business details, add photos, and respond to reviews (Appendix Figure A.1), and helped with logging in or claiming their their account. The marketing brochure was accompanied by a standard marketing postcard with Yelp advertising credits on the front and a blank canvas on the back (Appendix Figure A.1).

Instead of the blank canvas, firms assigned to treatment were shown a personalized competitor pricing report on the back of the postcard (Figure 1), which showed the firm's regular manicure price compared to its nine geographically closest competitors,[13] along with their names and exact prices. The report displayed the name of the business at the top with a summary description, algorithmically generated to take one of three versions: (1) "You charge the lowest/highest price in the area." [If applicable: "*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 Appendix Figure A.2 for the distribution of descriptions across businesses).[14] Before providing this information, canvassers first asked treatment firms who their

[12] Verification means that the business claimed their free page on Yelp, verifying that the listing was a true business.
[13] 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 were 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 vs. lower prices, as nine competitors were shown on the postcard.
[14] 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.

primary competitors were, what prices they charged, and how they compared (described in detail in Section 5.1, see Appendix Figure A.3 for scripts).[15]

Of course, pricing is simply one piece of information about competitors that firms may be interested in. I focused on pricing because it appeared to be a major driver of customer decisions, as consumer reviews often refer to prices and comparisons to competitors. Analyzing the text of reviews for all nail salons on Yelp at baseline using a neural network called word2vec,[16] I found that 46% of customer reviews discussed topics related to price and 35% mentioned words related to competitors, indicating that these were important for customers (Appendix Figure F.4). The most frequent phrases found in reviews also mentioned price and comparison to other salons.

Every canvasser was individually trained by me and Yelp's team managers with a standardized script and practice visits (Appendix Figure A.3).[17] A phone application recorded the canvasser's location and date stamp for the business 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. If they were still unable to do so by the third visit, canvassers left the brochure and postcard. They recorded descriptions of each interaction they had with businesses, such as whether they were able to speak with someone or asked to come back at another time. Canvassers were blind to the experiment and outcomes, and were assigned to one type of canvassing to begin before being transitioned to the other, with Yelp informing them that they were trying different ways of canvassing.[18] Canvassers worked independently in the area of the city they were assigned to.

4.2 Sample definition, randomization, and timing

The greater metropolitan areas of San Francisco Bay, New York City, Los Angeles, and Chicago were chosen as the markets for intervention, based on (i) the presence of Yelp offices to leverage 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. I identified ZIP codes within these areas[19] and extracted all 9,889 nail salon listings on Yelp in these ZIP codes.

I applied the following criteria to this set of 9,889 listings to determine the eligible set of businesses for the experiment (Figure 2(a)). I called every listing and used Google Maps Streetview to confirm they were open, offering nail services, correctly located,[20] and not a duplicate Yelp listing. Any salons with Yelp ratings of 1 to 2.5 stars (out of 5) were excluded to maximize the likelihood of compliance to treatments—as businesses with one or two stars were more likely to have antagonistic

[15] Canvassers asked three questions: (1) "What do you think sets your salon apart from your competitors?" (2) "Who do you consider as your primary competitors?" and (3) "What do you think they charge for a regular manicure?".
[16] 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 described in Gentzkow, Kelly, and Taddy (2019).
[17] 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.
[18] Canvassers were part-time contractors hired for the duration of this project. They worked independently and were assigned to one form of canvassing (either control or treatment) to begin. Canvassers were in constant communication with me and the Yelp managers, and checked in at the beginning and end of each daily shift.
[19] For the San Francisco Bay area, I identified ZIP codes in cities with more than 50,000 people across the greater Bay area.
[20] Correctly located meant checking that the actual location matched the listed location, and that the business was not located inside an airport.

stances against Yelp and less likely to speak to canvassers.[21] To the extent that lower-rated firms were lower-performing and less likely to know competitor information, the experimental sample should provide a stronger test for the impact of competitor information. The resulting eligible set of 3,948 businesses (62% of the full set of salons) was the goal that Yelp canvassers strived toward reaching, subject to a fixed canvassing budget and timeline and whether the business was still open at the time of visit.

Businesses in this eligible set were assigned to experimental groups through a stratified randomization process using the metropolitan area, prior relationship with Yelp, and Yelp rating rounded to the nearest multiple of 0.5 (Figure 2(a)).[22] Within each stratum, firms were randomly assigned to one of two experimental groups, control or treatment. 1,972 firms were assigned to treatment, and 1,976 firms were assigned to the control group.[23] To ensure that the resulting sample was balanced in the timing of visits across experimental groups, canvassers were assigned to finish all visits across firms within a neighborhood before moving on to their next neighborhood.

Between June 18 and November 18 of 2018, canvassers reached 3,474 businesses. 256 were identified as duplicates or closed by the time that they visited, resulting in an experimental sample of 3,218 firms (Figure 2(b)). All firms in Los Angeles and Chicago and most firms in New York and San Francisco were reached, excluding areas further out (the Bronx and outer areas of Queens for New York and North Bay for San Francisco; see Appendix Figures A.4 and A.5 for a map).

4.3 Balance, attrition, and non-compliance

Table 1 displays summary statistics for the baseline characteristics of firms in the experimental sample, showing that control and treatment firms were generally well-balanced, consistent with randomization.[24] In two of sixteen 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 due to businesses being closed at the time of data collector visits, but the timing of canvassing visits appears to be delayed among treatment firms by 1.4 weeks.[25] Given the importance

[21] Treatment information on competitor pricing, as well as measures of competition, were not subject to this restriction. I take the full set of verified firms for both.

[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. It can also improve precision to the extent that these variables explain the variation in the treatment of interest (Cox and Reid 2000). These variables were chosen based on a number of reasons. Metropolitan areas may have different dynamics of competition due to variation in business density as well as customer tastes, which could determine how the business responds to information on competitors. 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 key firm attributes and determine the business's receptiveness to Yelp canvassers and any information that they might provide.

[23] Stratified randomization was done using Stata.

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

[25] There was no one clear reason for this lag. One possible reason is that there were times where a canvasser had to take a break due to 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. Due to the importance of this variable, I control for the week that each firm was visited in all specifications.

of this variable, I add fixed effects for the week of the canvassing visit to all specifications that were pre-registered in my preanalysis plan.[26]

Non-compliance rates were low. Fewer than 2% of firms (58) were marked as non-compliant, which manifested in the form of firms rejecting any conversations with Yelp canvassers when they arrived at the business (Appendix Table A.1). In these cases, neither control nor treatment firms received any information from the canvasser.

I observed similarly low levels of attrition. Attrition stemmed from both firm closures, which were unlikely to be influenced by treatment, and from firms that could not be reached after canvassing visits—which I made a considerable effort to keep low through multiple calls and visits. Approximately 5% of firms in the sample permanently closed during the 12-month period. 1% of firms (36) in the sample were unreachable for any data after canvassing visits. Neither type of attrition varied significantly across experimental group, indicating that selective attrition is unlikely to bias the results.

5 Measuring competitor knowledge, firm positioning, and performance

I constructed a data set of competitor knowledge, price and quality positioning, and firm performance over a 12-month period between May 15, 2018, to September 15, 2019 (timeline in Appendix Figure A.6).

5.1 Measuring stated positions and knowledge of competitors

To collect measures of competitor knowledge and stated positions, Yelp canvassers asked a set of questions to treatment firms during their visits (script in Appendix Figure A.3). Prior to the delivery of competitor information, canvassers asked the key decision-maker (i.e. owner or key manager) at the business, (1) "What do you think sets your salon apart from your competitors?" (2) "Who do you consider as your primary competitors?" and (3) "What do you think they charge for a regular manicure?". Canvassers then delivered the information treatment and asked if they were interested in signing up to continue receiving the information. Canvassers recorded managers' answers as close to verbatim as possible. To ensure accuracy, both canvassers and managers were unaware of the experiment and its outcome variables, and canvassers' data entry and performance were monitored on a daily basis. All answers were read and coded by two independent research assistants, who first read a few hundred responses to understand potential categories of answers, and compared notes to arrive at a list of categories. They then individually assigned each answer to one of the categories, with any conflicts resolved by a third research assistant.

Building on research using managerial interviews to infer firm-level attributes (e.g., Bloom and Van Reenen 2007, Bloom et al 2015, and Yang et al 2020), these manager responses were interpreted as reflecting firm knowledge. Moreover, most businesses were sole proprietorships where the owner or key manager generally made important firm decisions unilaterally, and what they knew

[26] I describe differences between the paper and pre-registration in detail in Appendix K.

represented the competitor knowledge that drove those decisions. This interpretation of firm knowledge is consistent with the knowledge-based view that characterizes this as the knowledge that drives the key decisions of the business (Kogut and Zander 1992).

5.2 Measuring price positioning relative to quality

Data on price positioning were collected by a team of ~50 data collectors who made calls and visits to businesses.[27] All data collectors were blind to the experiment and experimental conditions, and assigned to collect data on both control and treatment businesses by neighborhood. To ensure data validity and accuracy, data collectors were given detailed scripts and evaluation rubrics, and had a subset of their data validated by another independent data collector. Their location and performance (in terms of accuracy and productivity) were tracked on a weekly basis.

Price positioning was measured by the price of a regular manicure, collected via monthly calls made to all businesses between May 2018 and May 2019. Data collectors asked for the price of a regular manicure without taxes or cash discounts. In a subset of the months, prices of other services (pedicure, manicure and pedicure combination) were also collected.[28]

These pricing data were validated in two steps. The full list of salons was divided among data collectors, with a random subset (5%) additionally allocated to another data collector as a quality check. Once all data collectors submitted their data, any observations with a business closure, unreachable flag, conflict in prices across two data collectors, or a mismatch between the name and identifier were reassigned to data collectors. This step was repeated up to three times each month.

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 – August 2018) and endline (May – September 2019).[29] It ranged from 3 (lowest quality) to 11 (highest quality), and results are robust to using a standardized sum of polish brands, cleanliness, and luxuriousness, or each individual measure alone (Appendix Figures D.2-3).[30]

To ensure standardization and accuracy, data collectors used an evaluation rubric to code quality metrics, and their coding went through several validation checks. For polish brands, data collectors were given a list of brands classified as low, medium, and high according to their retail price per bottle (below \$10; between \$10-\$20; more than \$20 respectively). They were instructed to select the highest level of polish brand they observed, as most firms used at least some low-cost brands. They recorded any brands that were not present on this list, which were then coded ex-post using their retail prices. For cleanliness and luxuriousness, data collectors were given a rubric of metrics

[27] Data collectors were undergraduates and Masters students recruited using job postings across every university in the four cities, posted every 3-6 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.
[28] Data collectors also noted whether the phone number was no longer in service, no one answered, nail services were no longer offered, business was permanently closed, or business refused to provide prices over the phone. Due to these reasons, data collectors were not able to obtain a price every month for each salon, resulting in an unbalanced panel.
[29] While 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. Collecting this data by physically visiting businesses improved measurement and ensured more thorough coverage across the sample.
[30] Polish brands ranged from 1 to 3 based on retail price per bottle, and cleanliness and luxuriousness were rated on a scale of 1 to 4.

to guide their coding, detailed in Appendix Table C.1. They were required to take photos of the interior, polish brands, menu, and exterior to ensure accuracy, and 5% of photos were checked every week. Approximately 5% of firms were assigned to an additional independent data collector to validate quality measures.[31] Data collectors also collected data on businesses' opening hours, promotions, and the number of employees and customers at the time of the visit.

5.3 Measuring performance

Firm performance was measured using indicators from the Yelp platform, next-day peak availability via phone calls, and the number of employees and customers physically observed at the business at the time of endline visits.

From the Yelp platform, I constructed monthly measures of business 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, along with the number of consumer reviews and photos—measures which prior studies have found to be positively correlated with firm revenues (e.g. Luca 2016, Dai, Kim, and Luca 2021).[32]

I also collected measures outside the Yelp platform. I collected a binary indicator of whether there was availability for an appointment during a peak time (4-5pm[33]) the next day via monthly calls, as well as the number of employees and customers observed at the time of visits at endline.

6 The landscape of competitor knowledge and firm positioning

6.1 Baseline competitor knowledge

Baseline measures suggest that many firms may have lacked competitor knowledge, including those that faced higher levels of competition. 46% 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 to be able to state specific competitors (Appendix Figure B.1(a)). 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.[34] Similarly, 58% of managers at treatment firms were unable to state the prices that their primary competitors charged (Appendix Figure B.1(b)). Consistent with this, I find that firms' observed pricing and quality decisions did not match the descriptions of their positioning relative to their competitors (Appendix Figures D.6-7).

[31] Any data collectors below a threshold accuracy level were replaced immediately, but this was rare: only two data collectors were dismissed over the full project period.
[32] Using historical tax revenue data from the Washington State Department of Revenue, Dai et al (2021) regress logged revenue change on logged change in page views, restaurant fixed effects, and quarterly dummies for a matched set of 835 restaurants, as a back-of-the-envelope calculation. Their estimate of the coefficient on change in pageviews is 32.54%, which was precisely estimated (1% level) with standard errors clustered at the business level.
[33] In order to prevent any suspicion across salons, the specific time within this hour was changed on a monthly basis (e.g. 4pm in January, 4:45pm in February).
[34] 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.

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) the baseline price dispersion across its geographically nearest 9 competitors[35] -- I find that a substantial percentage of managers at firms facing higher levels of competition were also not able to state competitor names and their prices (39% and 44% of firms, respectively) (Appendix Figures B.3 and B.4).[36] This lack of knowledge also persisted across managers at firms with below and above median size (number of employees), age, and price points (Appendix Figures B.5-7).

While these responses raise the possibility that many firms may not have been aware of their competitors, they are based on stated responses and may overstate the percentage of firms. I explore additional evidence of baseline price positions for a more complete picture of the baseline landscape before analyzing results on the impact of competitor information.

6.2 Dispersion in baseline price positioning

Consistent with the interpretation that firms lacked competitor knowledge, firms with similar quality located in the same ZIP code displayed substantial dispersion in their price positioning. While on average, firms that offered higher quality also charged higher prices (Figure 3(a)), firms displayed a large dispersion in their pricing. 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.[37] This price dispersion persists when controlling for ZIP code fixed effects and remains across firms that faced higher levels of competition (Appendix Figures D.4-5).

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). Figure 4 shows how these businesses perceive their positioning, which is mostly differentiated along quality (in brown): higher quality firms (in brown) offer better service, cleanliness, and luxuriousness, and lower quality firms (in red) offer lower prices. There is also some horizontal differentiation (in shades of blue), where the optimal choice at equal prices depends on the consumer (e.g., depending on location). This pattern of differentiation means that consumers do not view firms as perfect substitutes, as with other retail businesses like drycleaners, florists, and repair services.

However, this dispersion is also consistent with evidence of inefficient management documented across various industries (Bloom et al. 2013), as well as price dispersion in other contexts such as general retail (Lach 2002), prescription drugs (Sorensen 2000), gasoline (Lewis 2008), and online consumer goods (Brynjolfsson and Smith 2000; Ellison, et al 2018).

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[35] 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.

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

[37] The same pattern can be observed when plotting by a standardized sum of each quality measure, or each individual measure of quality alone (see Appendix Figures D.2-3).

Furthermore, firms that priced most consistently with the market exhibited higher proxies of performance, consistent with better management in general (reported in Appendix Figure E.1 and Table E.1). I proxy 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, which defines firms farther away from the best-fit line as having greater misalignment. I find that on average, firms with greater misalignment in their pricing and quality also observed fewer calls, map direction views, and page views on Yelp.

7 The impact of competitor information on firm pricing

These descriptive statistics suggest that firms may lack knowledge of their competitors' decisions, which highlights the possibility that firms may not need to know competitor decisions. Other informative sources such as observing customers and residual market demand may offer sufficient statistics for key competitor decisions, especially in more competitive markets where strategic interaction may be limited. If this were the case, the competitor information treatment should have little effect on the likelihood of treatment firms changing their prices after a canvasser visit. This section explores this question by discussing empirical results from the experiment.

7.1 Do treated firms change their pricing?

A. Graphical Evidence

Figure 5(a) plots the raw share of control versus treatment firms that charged a different price from their baseline price across months following the canvassing visit.[38] 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 and treatment firms showed an increasing likelihood of price change relative to baseline, as a larger percentage of post-visit months coincided with seasons when firms traditionally change their prices due to variation in demand. They were more likely to use promotions in slower months (fall and winter)[39], and generally changed menu prices at the end of the year between December and January. These patterns, shown in Appendix Figure G.1, were confirmed by managers and documented in industry magazines as well as the broader retail economy (Nakamura and Steinsson 2008, Nails Magazine 2008; 2018).

Figure 5(b) shows that treatment firms were more likely to change prices compared to control firms following the canvasser visit. To quantify the difference more precisely, I turn to regressions.

[38] Each month begins on the 15th of each month, in order to count months following canvasser visits, which began in June 18th. The number of observations collected in each month varied, due to some firms not answering their phones or being closed. Due to 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 4 months prior to the canvassing visit.
[39] 24.7% of firms used promotions of any kind.

B. Empirical Strategy

My main empirical specification leverages a difference-in-differences model as pre-registered:

$$y_{iswt} = \beta_0 + \beta_1 Post_{iswt} * Treat_{isw} + \beta_2 Post_{iswt} + \beta_3 Treat_{isw} + \gamma_w + \delta_s + \eta_t + \varepsilon_{iswt} \qquad (1)$$

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, measured by a binary variable indicating whether a firm's regular manicure price each month is different from the price observed at baseline (May 2018). I decompose this price change into a price increase or decrease, and examine percentage changes in price levels.

$Post_{iswt}$ is an indicator that takes the value 1 for firms in either control or treatment, starting from the month they are visited by a Yelp canvasser until the end of the study and 0 otherwise. $Treat_{isw}$ is an indicator that takes value 1 for firms assigned to treatment and 0 otherwise. γ_w controls for canvasser visit week fixed effects,[40] δ_s controls for randomization strata fixed effects, and η_t controls for data collection survey month fixed effects. ε_{iswt} is an idiosyncratic error term. The base model includes canvasser visit week fixed effects, and I additionally estimate equation (1) with randomization strata and survey month fixed effects to absorb noise. Since the unit of randomization is the firm, standard errors are clustered at the firm level.

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

C. The Impact of Competitor Information on Firm Pricing

Table 2 Panel A 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 3 percentage points (p=0.023). This point estimate represents a 17 percent increase compared to control firms after the canvassing visit. Estimates of the treatment effect are stable across all specifications, which control for any pre-visit differences between control and treatment firms, the passing of time, and the week of the canvasser visit, with columns (2)-(4) additionally

[40] This variable was not pre-specified but included given the observed lag among treatment firm visits compared to control firms.

controlling for month and/or strata fixed effects to absorb noise from seasonality and location.[41] These results are also robust to adding canvasser fixed effects, which are reported in Appendix O.[42]

In comparison to 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 this information is decision-relevant. As discussed in 7.1.A, firms generally changed menu prices at the end of the year. This pattern makes treatment effects appear to increase over time, as treatment firms show a visible jump in December (Appendix Figure G.2) and these post-December months comprise a larger share of observations as the number of months since treatment increases. Consistent with this, many treatment firms noted at the time of treatment that they would save the information for year-end when they changed their prices.

The magnitude of the effect is relatively modest, which seems 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 (SUTVA), would bias any treatment effect estimate downward, since control firms should be more likely to change prices as they become aware of competitor information. When surveyed after endline to explore the extent of any spillover effects, 28 control salons (less than 1.5%) stated that they heard about postcards from another salon, even if they had not seen the information (Appendix Table J.1). I exploit variation in the share of treated firms across ZIP codes (Appendix Figure J.1) to explore if control firms in markets with a higher share of treated firms were more likely to change prices, but find little supportive evidence (Appendix Table J.2).

Treatment firms on average increased prices (Table 2 Panel B). Column (1) shows that 4% 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 post-visit period, which is imprecisely estimated. A larger percentage of firms increased their prices in the months following the canvasser visit, as shown in Column (2). Treatment firms were 2.3 percentage points (a 17 percent change; p=0.036) more likely to increase their prices in the post-period, relative to 13.7% of observations among control firms. These changes resulted in a price increase of approximately $0.30 (a 2 percent change; p=0.009) on the average price of $13.20 among control firms (Column 3).

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[41] Strata and month fixed effects are not necessary for identification given that treatment is randomly assigned, but help absorb noise. Given potential non-spurious imbalance between control and treatment groups in canvasser visit timing, I control for the week of the canvasser visit across all specifications. The estimate for "Treat" captures any pre-visit differences between control and treatment firms, which are small and statistically insignificant. The estimate for "Post" reflects control firms' likelihood of changing prices after the canvassing visit, but also captures a mechanical increase from the passing of time. For all results going forward, I report results from my base specification with canvasser visit week. Results are robust to adding strata and/or month fixed effects.

[42] 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. Rather, canvassers were trained to not nudge businesses to change prices or provide their opinions even if asked, due to potential risks this could pose for Yelp. However, adding these 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). Appendix O reports all results adding canvasser fixed effects, which show that the results are consistent.

D. What drives treatment effects?

These results show that information on competitor pricing increased firms' likelihood of changing their own pricing decisions. There are three channels through which a treatment providing competitor information on pricing could drive this outcome. First, it may indeed be *competitor* information: firms change their prices in direct response to learning about what competitors charge. A second channel is that any treatment that provides information on competitor pricing also allows firms to learn about demand by observing competitors' pricing decisions, and this learning about demand—rather than any response to competitors—is what drives their price change. Lastly, information on competitor pricing makes pricing salient simply by providing information. All three effects could be present anytime 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, learning about demand may lead firms to be more likely to consider competitor decisions and respond to them, and price salience may trigger firms to learn more about both their competitor decisions and demand conditions.

In this section, I explore these channels to determine whether treatment effects may potentially be driven solely by learning about demand or price salience. If so, this would suggest that simply mentioning pricing to increase its salience or providing non-competitor-related pricing information would lead to the same treatment effects. While these mechanisms are difficult to fully disentangle, the evidence points largely to the competition effect. I find limited evidence that treatment effects are likely to be driven by learning about demand or salience alone, and find some support consistent with the interpretation that effects may be driven by response to competitors.

Learning about demand To analyze whether treatment effects are driven solely or mainly by firms learning about demand, I leverage four sources of variation. First, I leverage variation across markets in the degree of competition and changes in market demand. Using cartographic boundary files from the U.S. Census Bureau, I map perimeters of the ZIP codes included in the experiment. I then manually identify natural geographic barriers (e.g., bodies of water, mountains, airports, large parks, and highways) that may keep individual or groups of ZIP codes plausibly isolated by making it difficult for consumers to easily travel for services, to determine plausibly isolated markets (Appendix Figure M.1).[43] Following Bresnahan and Reiss (1991), I proxy demand in these isolated markets by the population and competition by the number of firms prior to the experiment, sourcing data from the 2017 U.S. Census. I then compare treatment effects for firms in isolated markets with higher levels of competition holding fixed demand, and treatment effects for firms in isolated markets with higher changes in demand, holding fixed the level of competition. If learning about demand is solely responsible for treatment effects, we should expect treated firms to be more likely to change their pricing in markets with large changes in demand, and not more likely to change prices in markets with more competition.[44] While estimates are noisy, I

[43] This results in 31 isolated markets across 118 ZIP codes that include 745 firms in the experimental sample (23%).

[44] This analysis makes two key assumptions: (1) Natural geographic barriers keep markets plausibly isolated, by making it difficult for consumers to easily travel for services; (2) In isolated markets, market demand can be proxied by the population in the area, and competition can be proxied by the number of firms in the area (following Bresnahan and Reiss 1991).

find that the magnitude of treatment effects are larger and positive for firms in isolated markets with more competition (Appendix Table M.1),[45] while they are smaller and negative for firms in markets with larger changes in demand (Appendix Table M.2). This suggests that treatment effects may be unlikely to be driven solely or mainly by learning about demand.

Second, I leverage the fact that the value of information for learning about demand for next year's prices is lower when it comes at the wrong time (e.g., when there is high tourist demand, providing less informative estimates for normal menu prices). I leverage exogenous variation in the month of treatment relative to tourist peaks to infer whether treatment effects vary depending on the timing, by comparing treatment effects for firms randomly assigned to receive treatment during peak tourist summer months compared to the fall. If firms that were treated during peak summer tourist months (e.g. from June 15 – September 15) are less likely to respond to treatment than those treated in the fall (e.g. September 15 – November 15), this would be consistent with learning about demand, given the complexity of inferring normal demand from tourist demand. Appendix Table M.3 shows that treatment effects are not lower during summer months and rather highest between June 15 – August 14 and October 15 – November 14, suggesting that the effects may not be driven solely by learning about demand.

Third, I leverage variation in how firms respond to competitors in the micro-area around the firm. Firms in this industry are extremely closely located, with 75% of businesses having their 9th nearest competitor within 1 mile (1.6km, a 15-minute walking radius), and 50% of businesses having their nearest competitor within 0.08 miles (0.1km, a 2-minute walking radius; see Appendix Figures M. 2 and M.3). Given that competitors located within a 15 minute walking radius are likely to be in the same demand market, I examine whether firms respond more to the average/median competitor in that radius (from whom they can learn about demand) versus their nearest competitor (whose decisions may be more salient). Treatment effects are larger and more precisely estimated when comparing whether and how treatment firms change their decisions relative to their nearest competitor, compared to their average or median competitor (Appendix Figure M.4) – suggesting that firms are more responsive to their nearest competitor whose decisions are most salient, rather than their average or median competitor in the same demand market from whom they can learn more about demand.

Lastly, I leverage variation in manager responses prior to treatment on who their competitors are. Appendix Table M.4 shows that the more specific the manager at the firm was in describing their competitors, the more likely they were to change prices after treatment. While this must be interpreted with caution given bias and lack of precision, this is consistent with the interpretation that firms were at least in part driven by *competitor* information, as those who were more aware of competitors were more likely to respond to treatment. I also tabulate notes from Yelp canvassers on how managers reacted to receiving the competitor information treatment (Appendix Table M.5).

[45] Effects appear to be driven by firms in isolated markets with the second top quartile of competition, rather than the most competitive that may potentially be closer to perfect competition.

While some refer to learning about demand in the area, more reference comparisons to competitors and how they plan to respond.

These analyses suggest that treatment effects are unlikely to be driven mainly or solely by learning about demand, and provides supportive evidence consistent with the interpretation that treatment firms may have at least in part been responding to competitor decisions.

Salience Any information provision treatment also inherently makes the information more salient, bundling in salience effects as part of the information effect. A key question is whether treatment effects may be solely or mainly driven by salience rather than the competitor information itself. While this is possible, the context and evidence observed suggest that it may be less likely.

First, 19% of treatment firms showed surprise and direct interest in the competitor information received during the canvassing visit, while indicating that they intended to change their prices (Appendix Figure A.7). For example, one 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 (both on other decisions and pricing for other services), and 65% of all treatment businesses signed up to continue receiving this information, with canvassers rating firms' interest in the pricing information with a mean and median rating of 4 on a scale of 1 (uninterested) to 5 (highly interested). Together, this provides evidence consistent with the interpretation that the increased likelihood of treated firms to change prices was driven by the competitor information.

Second, pricing is generally highly salient to these firms. Customers regularly call and walk in to ask about prices, and many businesses display their prices on windows and doors. Furthermore, the data collection for the experiment is more likely to have had an effect on increasing price salience across both control and treatment firms, as data collectors called multiple times a month to ask about pricing, whereas Yelp canvasser visits only happened once during this 12-month period.

Third, the timing of price changes is less consistent with salience alone. Salience effects have been shown to be short-lived (Donk and Zoest 2008, Donk and Soesman 2010), leading to effects during the short time interval immediately following the event. However, much of the price changes occurred months after firms received the competitor information treatment between December and January, as shown in Appendix Figure G.2. This delayed response suggests that salience is unlikely to fully drive the treatment effects observed.

Finally, for salience to result in price change, it must work through one of two channels. One channel is that making price top of mind nudges firms to experiment with changing their price, independently of competitors' pricing information, in which case the pricing patterns would look more idiosyncratic relative to competitor prices. Alternatively, salience may work through the same competitor information channel as the treatment, triggering a search for additional information about competitor pricing and other decisions. To unpack this further, I explore next how firms changed their pricing in response to treatment, which also provides evidence more consistent with firms responding to competitor information rather than salience alone.

7.2 How do firms change their pricing?

Analyzing pre-specified dimensions of heterogeneity, I find that treatment firms align their pricing 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 who 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 to their competitors, and increase their prices further if they charged higher prices compared to their competitors. In contrast, aligning with competitors would mean that firms who 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 to their competitors, and decrease their prices if they charged higher prices compared to their competitors.

Figure 6 shows treatment effects on price change, decomposed into price increases and decreases (regression results reported in Appendix Table H.1). Firms who charged baseline prices that were lower or higher than their nearest competitor were more likely to change prices than firms who charged similar baseline prices. Moreover, Panel B shows that those with lower baseline prices were 6 percentage points (p=0.028) more likely to increase prices; those with higher baseline prices were 3 percentage points (p=0.094) more likely to reduce them. This evidence of firms matching competitors is consistent with qualitative studies of industries such as news (Boczkowski 2010).

While I primarily examine pricing decisions because they can be adjusted faster and are easier to measure precisely, I also observe evidence consistent with firms changing both pricing and quality decisions. Appendix Table N.1 shows that treatment firms are 9% more likely to change their quality between baseline and endline compared to control firms, and both increase and decrease quality. However, the heterogeneity analysis is mostly noisy, making it difficult to draw any clear conclusions.

Consistent with the interpretation that this matching behavior was driven by firms that were mispriced or mispositioned, firms that were over- or under-pricing relative to their quality responded most to treatment. Appendix Figure H.1 shows how treatment effects varied depending on firms' baseline alignment between pricing and quality (regression results reported in Appendix Table H.2).[46] The degree of misalignment in baseline decisions is measured by the absolute error from the best-fit line regressing baseline price on quality and ZIP code fixed effects, defining firms further from the best-fit line as having greater misalignment. Treatment firms with higher baseline misalignment in pricing relative to quality were more likely to change prices.

Appendix Figure H.2 and H.3 report additional heterogeneous treatment effects in other dimensions, including firm size, age, baseline price, scope, and chain status.

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

8 The impact of competitor information on performance

The findings so far indicate that some personal care 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 how this affected firm performance. While the ideal way to understand performance effects would be to examine profits across all firms, unfortunately there are few industries where it is possible to get data on profits on individual firms (Foster, Haltiwenger, and Syverson 2007, Byrne and de Roos 2019). I examine the number of customers and employees observed, the number of customer reviews and photos posted, and the number of purchase intentions (calls, map direction views, and pageviews) on Yelp, which allow me to build on related literature in competitive strategy by providing richer data.

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 from Yelp compared to control firms (all p<0.001).[47] These gains were driven more by firms that were over-pricing at baseline relative to their nearest competitor, who were more likely to respond to treatment by decreasing their prices (Appendix Figure I.1). I also find that treatment firms received 7% more customer reviews and 6% more photos on Yelp compared to control firms (Appendix Figure I.2), suggesting that any changes made by the business were communicated to other consumers—as reviews and photos are saliently reflected on the search page and well as the business page (Appendix Figures F.2-3).

Treatment firms also had 0.3 more employees (p=0.004) and 0.25 more customers (p=0.051) when visited at endline—an 8% increase in both measures relative to control firms, and lower availability for a peak-hour appointment the next day (a 3 percentage point decrease; p=0.138). In addition to measures from Yelp mentioned above, which have been shown to be positively correlated with sales (Luca 2016, Dai, Kim and Luca 2021), these measures from outside the platform provide additional evidence consistent with improved firm performance. In particular, the number of employees provides a measure of performance that has been interpreted in prior work as an indicator of firm growth for small businesses (e.g., Chatterji et al 2019).

I also 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 for both control and treatment firms. While this analysis provides suggestive evidence that treatment firms observed higher revenues (Appendix Table I.1), interpreting these measures as revenues requires the assumption that (1) each purchase intention is independent and leads to a sale--which likely greatly overestimates the effect, and (2) that 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

[47] Due to 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.

of correlations between Yelp page views and revenues[48] suggests that treatment firms observe 4.8% higher revenues compared to control firms.

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 1 minute per competitor to collect.[49] 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 average salon for the marginal cost of collecting pricing information on the nearest competitor to be lower than the marginal benefit.[50]

I find little supportive evidence that this performance increase was driven by treatment firms increasing their engagement with the Yelp platform. Table 3 Panel B shows that following the canvasser visit, treatment firms were not significantly more likely than control firms to log in (2.6 percent, p=0.348),[51] claim their page (-0.2 percent, p=0.865), purchase advertising (0.6 percent, p=0.222), or comment on reviews (0.9 percent; 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 an appointment to respond.

While these results suggest that treatment may have resulted in improved performance, there are at least two reasons to be cautious about their interpretation. First, none of these measures capture profits, only proxies of them. One of the challenges of quantity-based measures in this case is that they may exhibit a downward bias (e.g., when increasing price, quantity may decrease, even if profitability rises). In this case, this appears less likely to be a concern, as treatment firms were on average more likely to increase prices, and also observe increases in performance proxies. Nevertheless, it is possible that while these indicators increased, profitability did not increase. Although quantity-based measures can provide insights into firm productivity, which strongly correlates with survival, they do not necessarily move together with profitability (Foster, Haltiwanger, and Syverson 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 (Appendix Table J.3). While I do not find supportive evidence, confidence intervals are large, and cannot rule out large effects.

[48] Dai et al (2021) who use revenue data from the Washington State Department of Revenue find that a 10% increase in quarterly page views is correlated with a 3.3% increase in quarterly revenue

[49] I sat with employees across pilot salons to do this calculation and measured the time taken for them to look up and call their nearest competitor to ask about their regular manicure price. They took on average 30 seconds per competitor, and no one took more than 45 seconds.

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

[51] The upper end of the confidence interval on login days is high, but for any increase in login days to drive the change in customer calls, map views, or pageviews, firms would have to engage in activities such as purchasing advertising (column 3) or commenting on reviews (column 5), 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), or to update their page to reflect changes in their prices or services.

9 Why do firms lack knowledge of key competitor decisions?

The positive effects of the competitor information treatment, combined with the fact that 75% of businesses had capacity for a next-day peak-time appointment at baseline, raises a puzzle. Given that information on competitor prices was not only accessible but decision-relevant, the natural question is why firms did not previously use this information. I consider several possible explanations and find the most supportive evidence for managerial inattention.

9.1 Possible explanations

A. Low competition

It may be that while the value v of paying attention to competitor information is on average positive, it varies depending on the level of market competition. When competition is lower, the value of competitor information may be lower than the expected costs c of processing the information, such that $\underline{v} - c < 0$. This may also be the case when a business is run by a "lifestyle entrepreneur" with little desire to grow—who hence places less value on competitor information (Hurst and Pugsley 2011). This would imply that the competitor information treatment, by marginally lowering c, led those with $\underline{v} < v$ (i.e., firms who derive insufficient value from competitor information to incur the cost of it themselves) to change their prices. A broader version of this explanation is that competitor information is simply not useful for some firms relative to the costs, and these firms are those that do not know competitor information and respond to treatment.

I find limited support for this explanation, as treatment firms with above-median levels of competition were more likely to change prices (Appendix Table H.3).[52]

B. Lack of ability to use competitor information

A second explanation may be that v varies across firms depending on their ability to take advantage of new information (Cohen and Levinthal 1990; Henderson and Cockburn 1994) or their complementary capabilities (Milgrom and Roberts 1990; Bloom, Sadun, and Van Reenen 2012).[53] 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 with a sufficiently high v. This explanation is thus distinct from the first in that it proposes that competitor information is valuable relative to the cost for firms, but some do not know it because they do not have the capability to use the information effectively to change their decisions. If this were the case, then the treatment should have had no effect on such firms since it did not change their v, while making firms with such capabilities adjust their prices marginally earlier than they otherwise would have on their own.

[52] This result that treatment firms that face higher levels of competition are more likely to respond is robust to using other cutoffs such as quartiles. This evidence raises the question: why do these firms survive? One explanation may be that I am observing short-run dynamics. Another explanation may be that there is some friction that limits competition. For example, quality firms may be capacity constrained, which reduces the strength of the selection mechanism in the market. My results likely do not generalize to perfectly competitive markets in the long run.

[53] For example, firms may need a prior understanding of customer preferences across the market, or analytic skills to process optimal responses to many competitors, in order to use the information to their benefit (e,g, Dutta, Zbaracki, and Bergen 2003).

I code whether firms used demand-based promotions at baseline as a proxy for sophistication in pricing, as it indicates an understanding of customer demand fluctuations.[54] I find limited support for this explanation: estimates are noisy, and suggest that treatment firms that did not use demand-based promotions at baseline did in fact respond to competitor information (Appendix Table H.4).

C. Managerial inattention

Lastly, I explore whether managers underestimated v. Recent research suggests that managers may be inattentive to important features, which may lead to biased estimates of $\hat{v} < v$ that underestimate the value of information – in this case, on competitors (Hanna, Mullainathan, and Schwartzstein 2014, DellaVigna and Gentzkow 2017). A parallel literature in cognition and strategy has investigated how managers rely on cognitive filters and mental models, which may be incomplete or inaccurate (Simon 1947, Cyert and March 1963, Menon and Yao 2017). This research suggests that these cognitive biases could lead managers to overlook some competitors, or underestimate the value of paying attention to any competitors altogether (Porac et al 1989, Baum and Lant 2003, Tripsas and Gavetti 2000, Kaplan, Murray, and Henderson 2003, Helfat and Peteraf 2015, Ocasio 1997). This last category of explanations suggests that this competitor information may be valuable, firms know how to use it to improve their decisions, but they do not pay attention to it sufficiently because of cognitive biases that lead them to underestimate its value.

Informal interviews with managers at 25 businesses outside the experimental sample raised the possibility that managers may have failed to attend to competitor information because they held outdated knowledge that led them to underestimate the value of acquiring information. These interviews lasted approximately 30 minutes to 2 hours. When asked whether they would find information on competitor prices valuable, 14 out of 25 managers answered that this information would not be useful, explaining that they were already aware of what competitors were doing.[55] However, when asked to specify who their primary competitors were and what they were charging, many managers could not answer precisely, consistent with patterns described in Section 6.1. They explained that they were not sure exactly what the price points may be, because it had been a while since they had last checked, some spanning years. 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 ten years ago that I last looked at competitors' prices [...] in detail." Once given treatment postcards with competitor prices, 6 managers expressed surprise and stated that they would change

[54] 10.1% of firms used demand-based promotions (based on hours of week, days of week). Conversations with managers and owners supported the interpretation that the use of these promotions were linked to sophistication in pricing: those who use 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 are not included in this coding, as almost every firm uses these discounts. I also exclude promotions for new customers, repeat visits, and group- and birthday-based discounts, as these are also common and do not indicate sophistication with pricing based on knowledge of fluctuating customer demand. However, the results are robust to using this broader definition of promotions.

[55] Managers at seven businesses did not want to speak further, and the remaining four said that they would find the competitor information useful and wanted to see it. Those who stated that they were already aware of what competitors were doing explained that they could easily observe this information themselves on Yelp, or emphasized that they had "competitive prices."

their prices, consistent with the responses of treatment businesses that canvassers reported in the experiment. For example, one salon manager commented, "Wow, a lot has changed. I should think about how to change my prices...I'll keep it in mind at end of the year".

These comments raise the possibility that managers may underestimate the value of competitor information due to a form of overconfidence about previously gathered knowledge that lead them to not update sufficiently, consistent with a large body of research that individuals, especially managers (Malmendier and Tate 2005, Chatterjee and Hambrick 2008), overestimate their own performance and have excessive precision and confidence in their own answers or beliefs (Moore and Healy 2008, Johnson and Fowler 2011). If true, this would suggest that information may be sticky even when it is not tacit, and that the competitive information firms use may lag the actual situation. I evaluate this notion using a follow-up experiment across control firms.

9.2 A follow-up experiment among control firms on managerial inattention

At endline (between May and August 2019), key decision-makers at all firms were asked a series of questions by data collectors to assess their knowledge of competitors.[56] Once they responded, they were given correct answers based on data collected the same week (often the same day). They were incentivized with a $10 Amazon gift card that was provided if they answered all questions correctly.

I randomly assigned control firms to one of two experimental conditions, which varied the sequence of when these questions were asked. 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" about signup, after answering questions about their nearest competitors. Randomizing the sequence enabled me to explore whether these managers underestimated the value of attending to competitor information (thus showing lower demand for it) when not prompted to evaluate their knowledge first.

The experiment included 1,405 control firms. Firms were well-balanced across experimental conditions, and attrition did not vary significantly across conditions (Appendix Tables L.1-2).[57]

Consistent with the interpretation that managers were inattentive to competitor information until prompted to reassess their knowledge, firms assigned to be "Asked Last" whether they were interested in signing up for competitor information were 4 percentage points (p=0.089) more likely to sign up over a base of 22% of firms assigned to "Asked First", representing an 18 percent increase (Table 4). Furthermore, 45% of the "Asked Last" firms who signed up for competitor information

[56] 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 to your two nearest nail salons?". Using these questions, I also find suggestive 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) (Appendix Table L.4). This provides suggestive evidence consistent with the interpretation that firms in the main experiment that signed up to receive competitor information may have found the information valuable. However, it is worth noting that treatment firms appear to be less likely to correctly answer their relative price to the nearest 2 competitors, which is imprecisely estimated.

[57] Attrition stemmed from firms that were not available or not willing to have a conversation, as well as firm closures, both permanent and temporary.

stated that this information was helpful because they had not looked at their competitors in a while, with half of these responses (22%) additionally stating that they planned to change prices.

While these results provide little conclusive evidence, it raises the possibility that firms may be inattentive to competitor information because they 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), and SMEs in China (Cai and Szeidl 2018) where managers appear to have underestimated the gains of a practice. While this broader behavior may be in principle correlated with managerial inexperience or incompetence, in this context I do not find significant evidence of these as key drivers of the treatment effects observed (Appendix Figures H.3(a)-(d)).[58]

10 External validity

The key motivation behind this experiment was to establish a high degree of internal validity to provide a test of the causal effects of competitor information, which is especially important in early causal tests of theory (List 2020). To do so, this study focuses on the personal care industry, where strategic simplicity enables precise empirical measurement. These findings are thus likely to be most applicable to other small and medium enterprises with similar characteristics, a major segment of the economy. How far they apply to larger firms is an open question. While generalizability is a question of speculation for any empirical study, List (2020) provides three key criteria to consider for field experiments: (1) the representativeness of the sample that may affect the response to treatment; (2) the degree of attrition and non-compliance suggesting issues that could limit compliance; (3) naturalness of the choice task, setting, and time frame that influences how interchangeable the nature of choice is between the research setting and target settings.[59]

On the latter two criteria, this study suggests a high level of external validity. There is low attrition and non-compliance, and the nature of choice is essentially interchangeable between the research setting and target settings. Firms in their natural environment are provided with

[58] While I do not have data on managerial experience or competence, I explore these potential mechanisms by leveraging firm-level attributes, imperfect proxies that leave much to be desired but are likely to correlate with these managerial attributes. To explore managerial incompetence, I analyze heterogeneous treatment effects by baseline firm performance measures (number of customers, number of employees, and purchase intentions). This analysis assumes that more competent managers are more likely to better manage the business and achieve higher performance. However, across all three firm performance proxies, I do not find supportive evidence that firms with below-median baseline performance were significantly more likely to respond to treatment. To explore the role of managerial experience, I analyze heterogeneous treatment effects by firm age (Appendix Figure H.3(d)). While managers may turn over during the course of a firm's lifetime, given that many of the firms in the experimental sample are sole proprietorships, I assume that firms that have been operating for a longer time may be more likely to have owners or managers with longer experience. While the coefficient estimates for Post * Treat * Above Median Age are slightly negative for older firms and smaller in subgroup analyses, these estimates are noisy and indistinguishable from zero. This raises the possibility that less experienced managers may be less attentive to competitor information, but does not provide strongly supportive evidence, especially given other potential explanations independent of managerial experience (e.g., that older businesses have a larger base of regular customers and face less competition, reducing the value of competitor information). I also examine the extent to which baseline firm performance and firm age drive differential signup rates in the follow-up experiment in Appendix Table L.5, and similarly find noisy results, making it difficult to conclude that managerial (in)competence and (in)experience drive the results.

[59] List raises a fourth criteria for programmatic studies targeted to policymakers: what the factors would be that would affect who should receive the program, how it should be implemented, and whether this would pass a benefit vs. cost test if implemented at scale. Given that this last criteria does not apply to this study, which does not propose an intervention for policymakers, I assess the study according to each of the first three criteria.

information from an online platform that they interact with, and make decisions with no artificial constraints imposed. Their decisions and outcomes are simply observed over a period of 12 months.

On representativeness, the considerations are more nuanced. This study examines thousands of retail firms across hundreds of real markets, making the findings likely to be representative of other retail SMEs that are similarly differentiated and have pricing as one of the key drivers of competition (e.g., drycleaners, florists, and repair services). SMEs like these represent a major segment of the economy, as those with fewer than 20 workers (a lower range than observed in this sample) account for 89% of all U.S. establishments – thus accounting for much of the sample in studies that use Census data to understand firm behavior (e.g., Azoulay, Jone, Kim, and Miranda 2022, Zolas et al 2020), as well as studies of small entrepreneurial businesses that are of increasing interest to many scholars (Camuffo et al 2020, Chatterji et al 2019).

How far they apply to larger firms in other industries is an open question. While this industry's high availability of competitor information, low costs of acquiring it, and high degree of competition across many markets provide a strong test of whether firms know key competitor decisions and its impact, there are only a few large firms in this sample, making it not very representative. In particular, larger firms have more resources and often more dimensions on which to differentiate.

How firms change in response to competitor pricing information—by aligning with their competitors—may be a result that is more generalizable across different contexts. Theoretically, larger size, more resources, or having more dimensions on which to differentiate does not yield clear predictions for why competitor information might lead firms to respond in a different direction. However, it is possible that having more dimensions on which to differentiate may lead firms to be more likely to differentiate in at least one of the dimensions.

The specific finding that firms may not be informed of their competitors' pricing may be less likely to be applicable to larger firms, as they tend to have more resources to devote to tracking competitor prices. However, the broader mechanism of inattention to key competitor decisions may not be limited to small firms, especially given that larger firms also have more complex strategy spaces and many more competitive dimensions beyond pricing that they could be uninformed of. Moreover, while large businesses generally have more resources, strategic decision-making may happen via individual managers who all have bandwidth constraints, as research in cognition and strategy has explored (e.g., Denrell, Fang, and Liu 2017). In fact, 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, Murray, and Henderson 2003), airlines (Hortascu et al 2021), technology (Eggers and Kaplan 2009), and grocery stores; (DellaVigna and Gentzkow 2019), even for pricing specifically (Cho and Rust 2010, DellaVigna and Gentzkow 2019, Hortascu et al 2021)—suggesting that even large firms may have similar problems. Consistent with this notion, Leisten (2021) finds that hotels affiliated with large chains have worse information about market demand when setting prices compared to hotels affiliated with smaller chains. This raises the possibility that large firms may also be uninformed about some key competitor decisions. Whether this is the case and how is an empirical question for future study.

11 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 evidence consistent with the interpretation that this lack of knowledge may be driven by managerial inattention. Personal care firms that are randomly assigned to receive competitor pricing information change their own decisions by increasing alignment with competitor offerings. These findings highlight that limited information processing may be a key problem for firms and a central issue in strategy.

More broadly, data on competitors and the broader market are becoming increasingly available across many markets. A particularly relevant context is online platforms, where the design of information can impact the performance of firms on the platform, as well as the growth of the marketplace (H. Kim and Luca 2019; Rietveld, Schilling, and Bellavitis 2019, Huang 2022). Many are actively introducing information into their marketplaces, often in hope of optimizing their supply side—such as businesses on Google that fail to update their advertising bids when doing so could increase revenues, or Airbnb hosts who fail to adjust their pricing even as demand grows (Airbnb 2017). The findings here suggest that many firms—even in competitive markets—may be farther away from the productivity frontier in their positioning than we may expect, and relatively simple information interventions have the potential to help them improve their decisions. However, simply making information accessible may not be sufficient to change firm decisions. These findings highlight that understanding how managers process information and designing mechanisms to overcome potential inattention may be a fruitful direction for future work.

Finally, these findings raise the possibility that as competitor data become increasingly available and used, firms may make decisions that align more with their competitors. Studies across various industries have documented increasing similarity across competing firms over the past few decades (e.g., Boczkowski 2010), and this paper suggests that competitor information may be a possible driver of this similarity. However, this study focuses on a single context and one type of competitor information on pricing, and exploring the extent to which this pattern generalizes across other settings and other types of information is an important area for future work.

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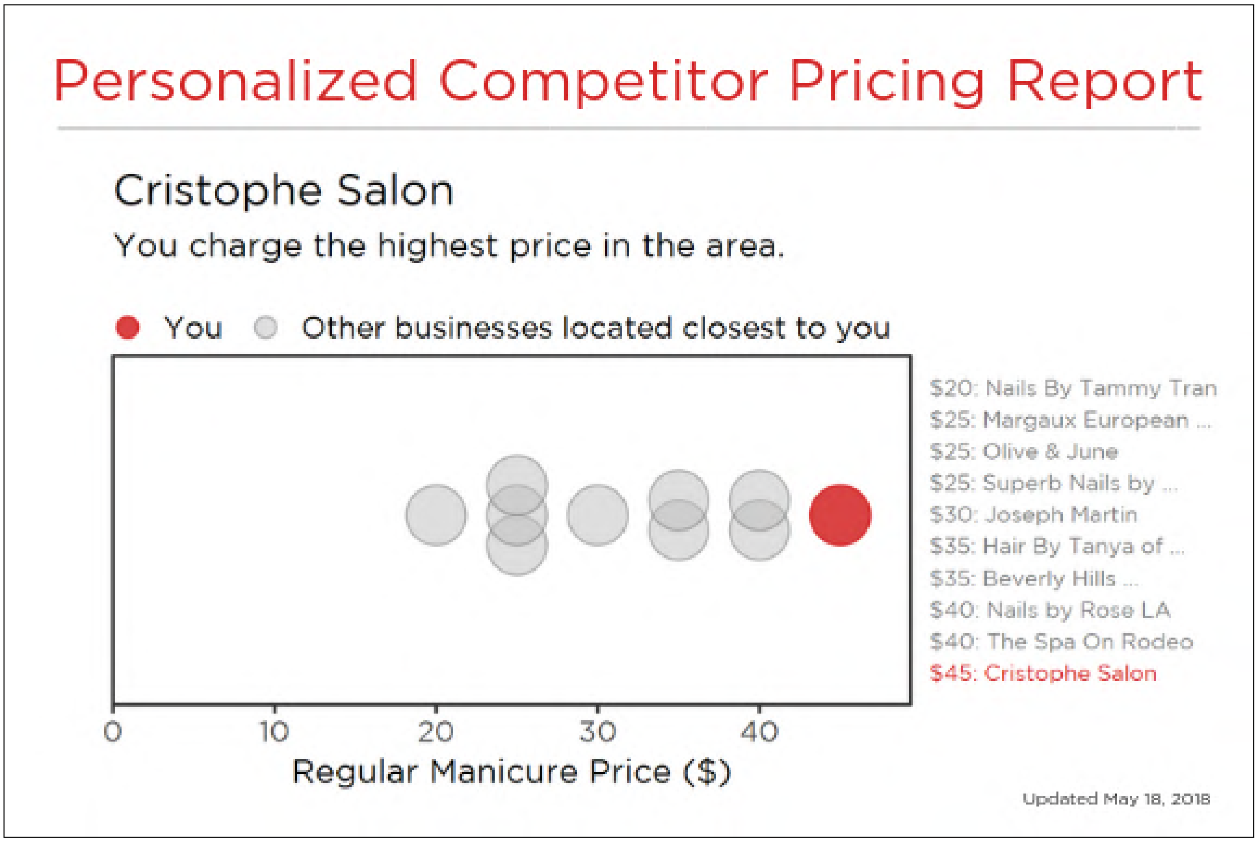
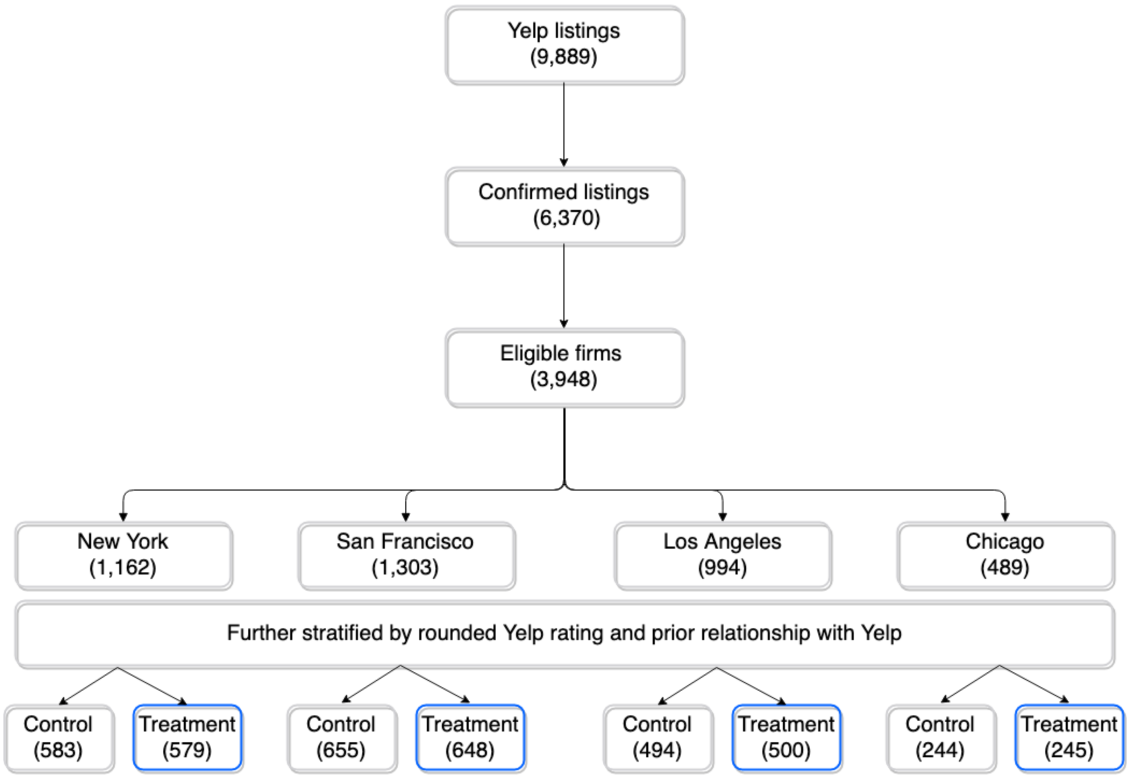
Figure 1: Sample treatment information



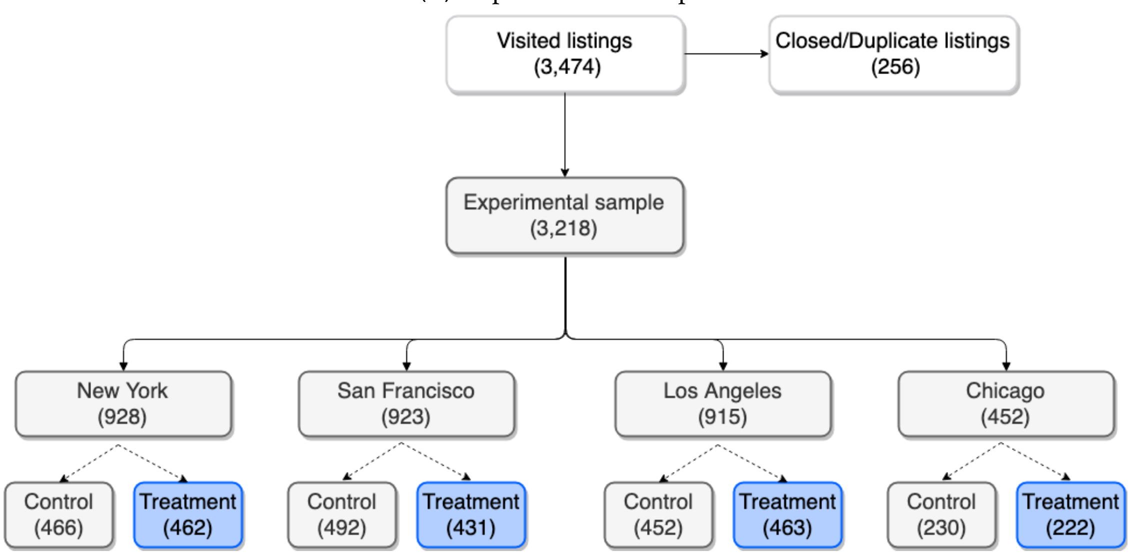
Notes: The back of the marketing postcard for treatment businesses included a personalized competitor pricing report, a sample of which is shown above. The image showed the firm's regular manicure price compared to its nine geographically closest competitors. The right side of the postcard listed the names of each competitor, along with the exact price it charged. The postcard displayed 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: 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.

Figure 2: Randomization and experimental sample

(a) Randomization



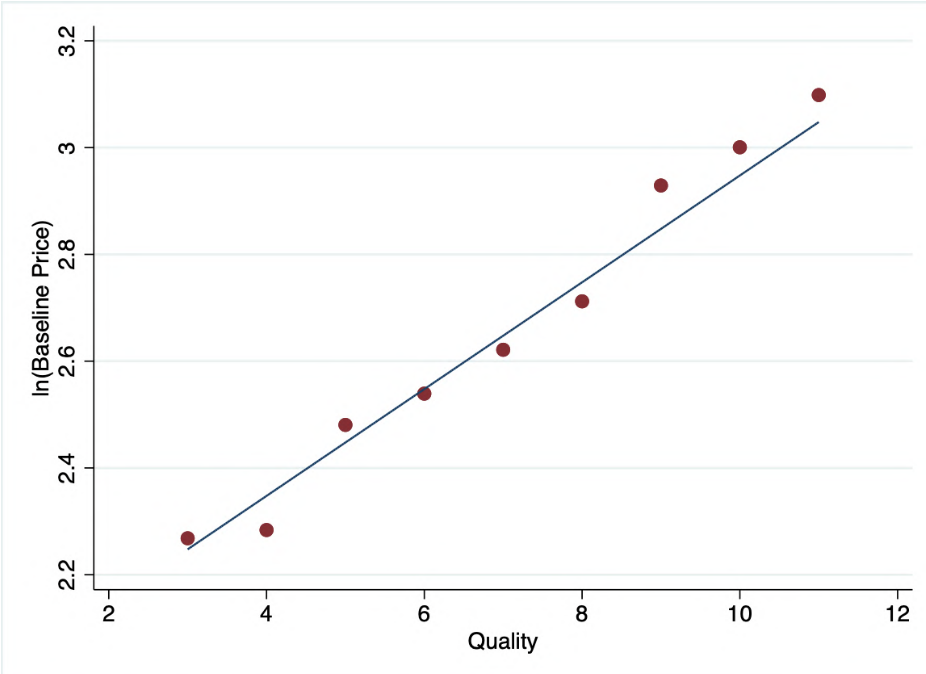
(b) Experimental sample



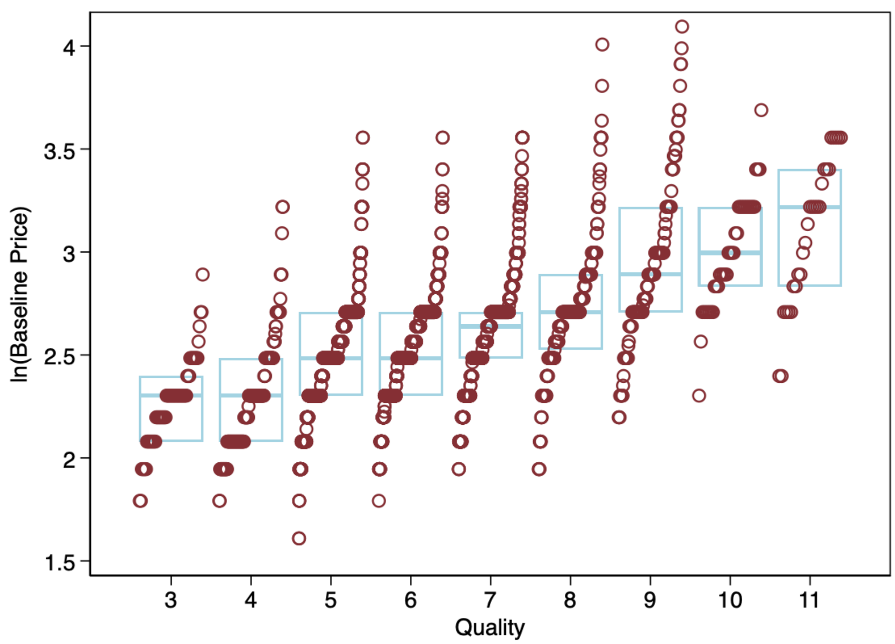
Notes: This figure shows the sample definition and randomization map. (a) All nail salon listings on Yelp across the greater San Francisco Bay Area, New York City, Los Angeles, and Chicago were verified via phone calls and Google Streetview, resulting in 6,370 confirmed firms. This set was further restricted by excluding any salons with Yelp ratings of 1 to 2.5 stars (out of 5) 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) Between June 18 and November 18 of 2018, canvassers were assigned to visit firms and reached 3,474 firms. 256 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 in New York and the Bay Area were reached (excluding the Bronx and north Bay, shown in Appendix Figures A.4-5).

Figure 3: Mapping pricing and quality decisions

(a) Average price by quality

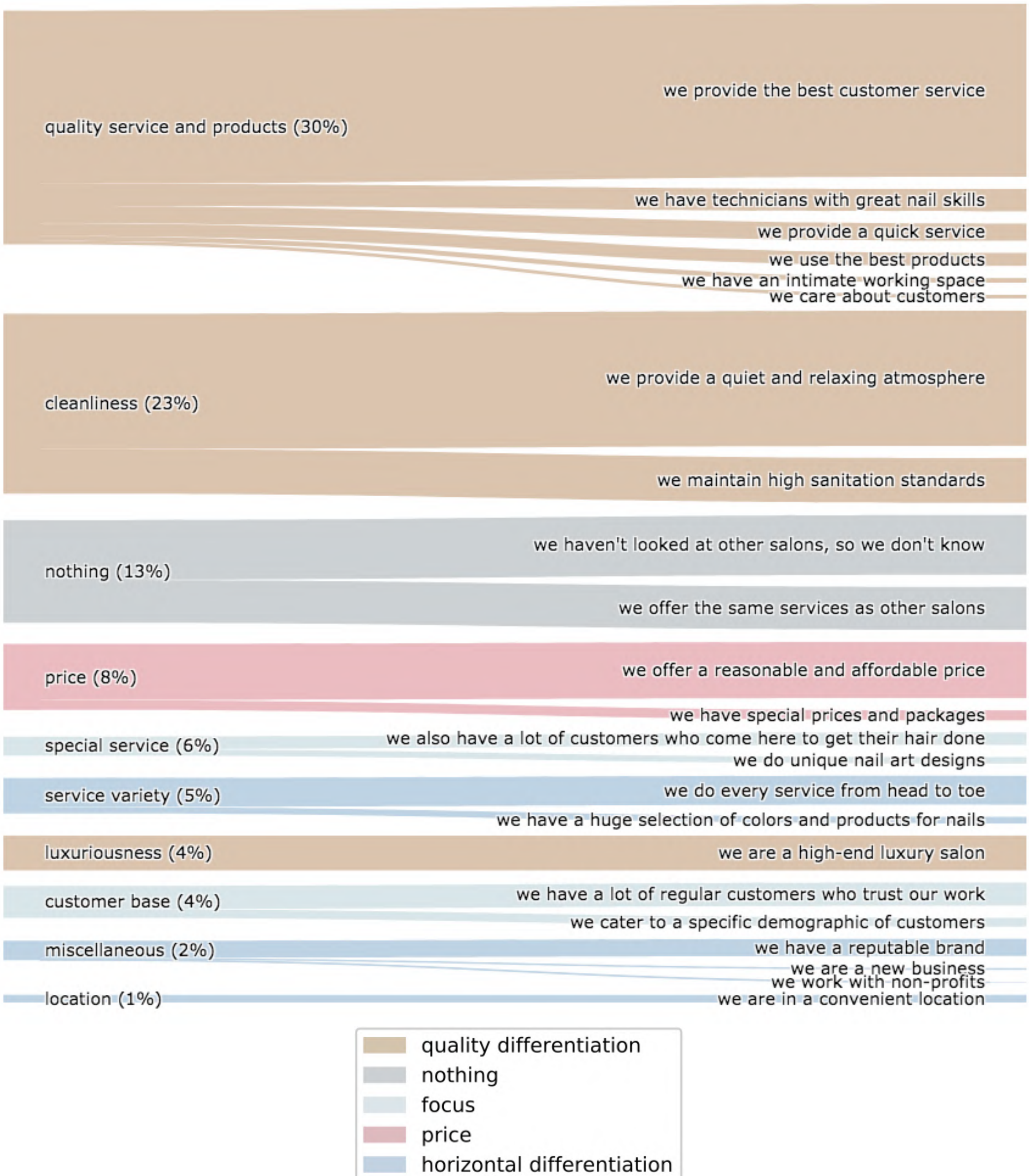


(b) Dispersion in firm pricing by quality



Notes: (a) 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 Appendix Figure D.2-3). (b) plots took a natural log of the baseline price on baseline quality, showing every firm observation (represented by a red circle) 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%.

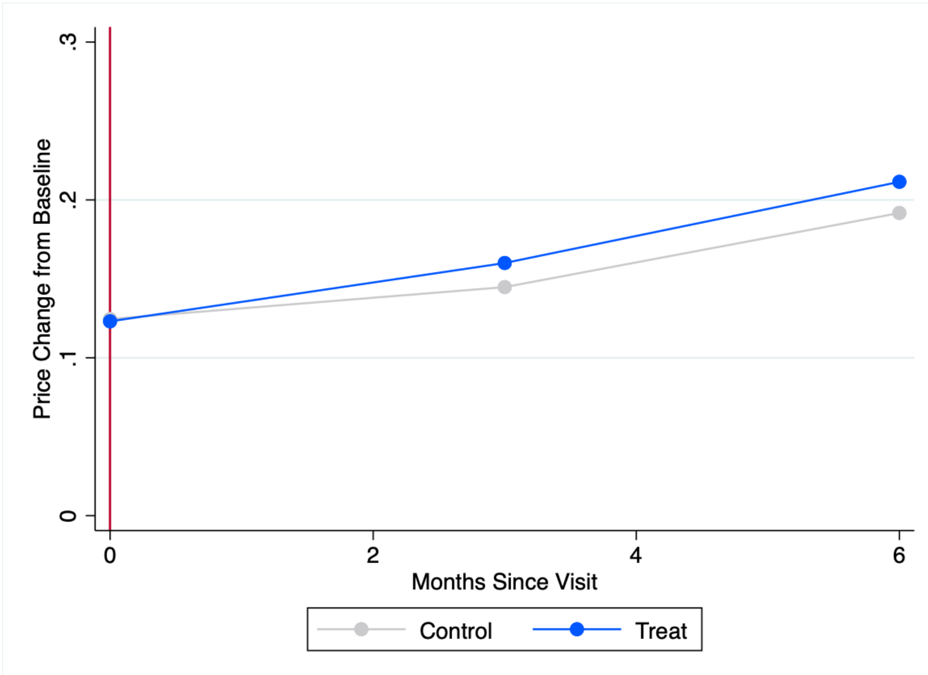
Figure 4: Descriptions of firm positioning



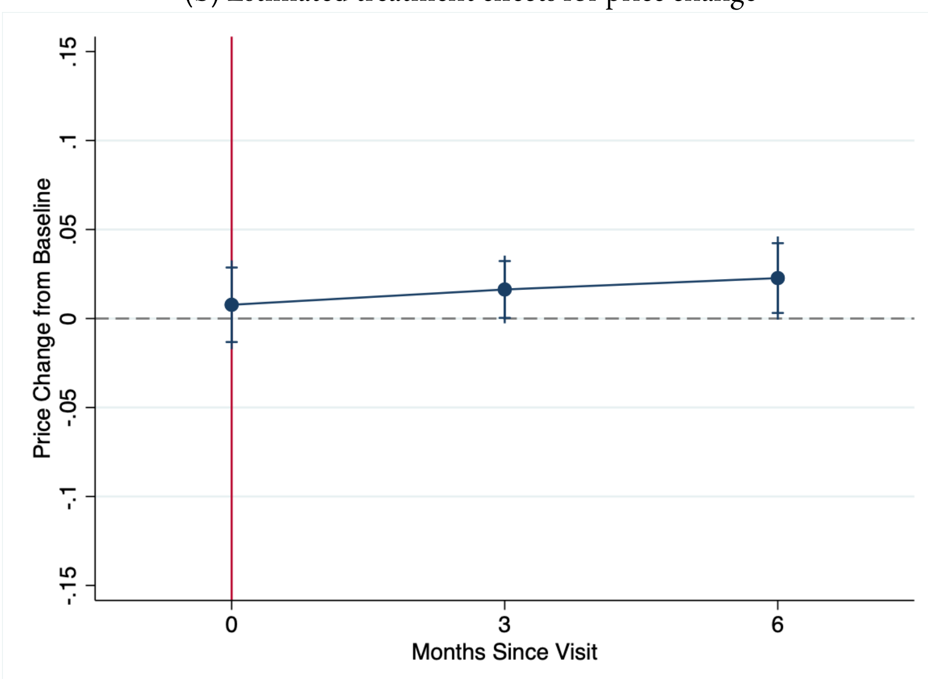
Notes: This figure shows a diagram of the self-descriptions that managers at treatment firms provided on their positioning prior to treatment, prompted by the question, "What sets you apart from your competitors?". Each response was coded into categories by two independent research assistants, with any 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%).

Figure 5: Share of firms that changed prices across months

(a) Raw share of firms that changed prices



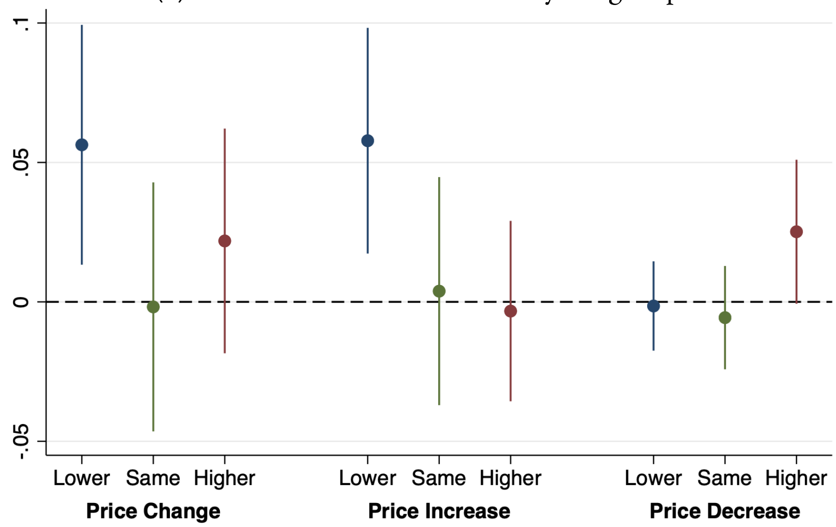
(b) Estimated treatment effects for price change



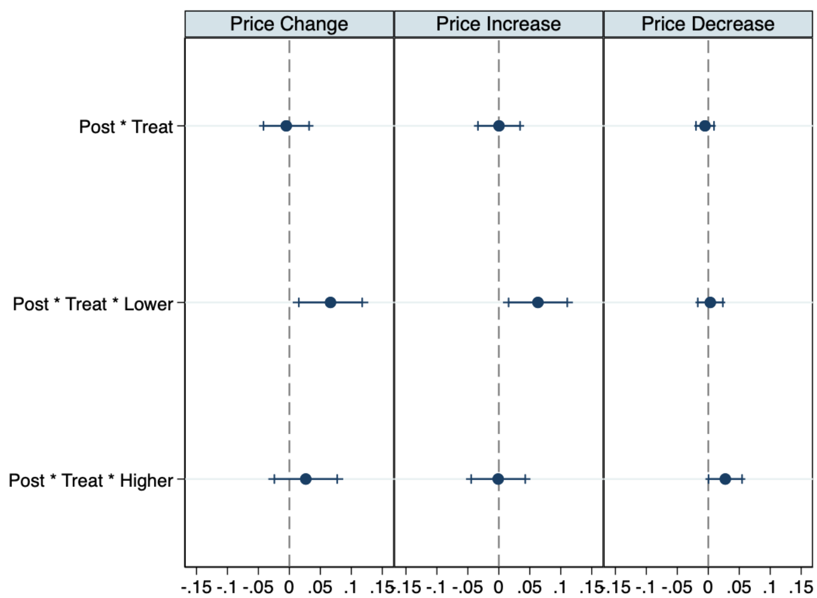
Notes: (a) plots the raw share of control and treatment firms that changed their price from their baseline price by the number of months since the canvassing visit, pooling across months for which data are available for the full sample. Each month begins on the 15th of each calendar month in order to count months following the canvasser visit, which began on June 18, 2018. The figure displays outcomes across the 6 months for which data are available for the full sample: due to the staggered timeline of visits across the 12 months of data collection, firms visited between June 15 - July 14 only had one month of pre-visit data (the baseline price), while firms visited between October 15 - November 14 had only 5 months of post-visit data. (b) plots the estimated treatment effects with 95% confidence intervals.

Figure 6: How firms change prices relative to their nearest competitor

(a) Estimated treatment effects by subgroup



(b) Differences in estimated treatment effects across subgroups



Notes: Figure (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, same, or higher prices compared to its nearest competitor). Figure (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 to its nearest competitor (i.e. the estimate for Post*Treat indicates the treatment effect for firms that charged the same price relative to its nearest competitor; the estimate for Post*Treat*Lower indicates whether the treatment effect for firms that charged less than its nearest competitor is statistically different). Observations are at the firm-month level, and all regressions control for any pre-visit 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 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	3218	-0.19	0.30
Latitude	38.13	38.09	2.95	33.72	42.05	3218	0.04	0.71
Longitude	-102.58	-102.08	21.17	-122.56	-73.68	3218	-0.49	0.51
Baseline Number Of Employees	4.22	4.31	2.53	1.00	25.00	2923	-0.09	0.31
Baseline Number Of Customers	3.68	3.82	3.23	0.00	30.00	2926	-0.13	0.26
Baseline Total Hours Open Weekly	61.89	62.23	10.25	8.00	115.50	3073	-0.33	0.37
Baseline Cleanliness1to4	2.63	2.67	0.70	1.00	4.00	2964	-0.04	0.13
Baseline Luxuriousness1to4	2.37	2.46	0.73	1.00	4.00	2969	-0.10^{***}	<0.01
Baseline Polish Brand Level	1.12	1.12	0.37	1.00	3.00	3018	-0.00	0.74
Baseline Number of Services (Scope)	2.08	2.11	1.24	0.00	7.00	3092	-0.02	0.59
Baseline Availability Next Day 4-5pm	0.75	0.75	0.27	0.00	1.00	3209	-0.00	0.95
Baseline Average Daily Opening Hour	09:44	09:43	00:31	06:00	14:00	3075	00:01	0.40
Baseline Average Daily Closing Hour	19:14	19:15	00:50	13:00	23:25	3074	-00:01	0.42
Baselien Price of Gel Manicure	29.29	29.35	8.06	10.00	105.00	2806	-0.05	0.86
Baseline Price (Dollar Signs) on Yelp	1.77	1.79	0.52	1.00	4.00	3008	-0.02	0.29
Baseline Yelp Rating	3.89	3.88	0.61	3.00	5.00	3142	0.01	0.49
Baseline Number of Yelp Reviews	68.41	69.62	84.68	0.00	1075.00	3218	-1.21	0.69
Yelp Canvass Week	32.95	34.39	5.33	24.00	44.00	3218	-1.44^{***}	<0.01

Notes: This table shows summary statistics and 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 (as randomization happened prior to physical data collection) are excluded from this table due to 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 customers count the total number of employees and customers that were observed at the time of visit. Cleanliness and luxuriousness are coded on a scale of 1 to 4, detailed in Appendix Table C.1. Polish brand level is coded on a scale of 1 to 3, based on the retail price of the most expensive nail polish brand observed. The number of services counts the total types of services that are offered by the firm (e.g. spa services, hair cuts, hair removal, make-up, tanning, and tattoos and piercings). Availability next-day is a binary variable collected by data collectors when inquiring for an appointment between 4-5pm, a peak hour for salon services. Yelp canvass week measures the week that canvassers visited each firm.

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	30142	30142	29552	29552
Mean (control in months after visit)	0.173			
SD (control in months after visit)	0.378			

Panel B: Directions 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	30142	30142	30142
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 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 for the week of the canvasser visit. Columns (2)-(4) in Panel A additionally control for randomization strata fixed effects and/or month fixed effects. Standard errors are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

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	35398	35398	35398	25755	2491	2494
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	35398	35398	35398	35398	35398

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, pageviews, and map directions views, respectively. Column (4) shows treatment effects on a binary indicator of availability for an appointment next day during a peak hour (4-5pm) when asked via phone calls. Columns (5)-(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 logins, (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 Panel A (5)-(6), 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 for the week of the canvasser visit. For Panel A (5)-(6), observations are at the firm level, and regressions control for the week of the canvasser visit. Standard errors are at the firm level.

Table 4: The effect of reevaluating competitor knowledge on demand for information

	(1) Information Signup
Signup Asked Last	0.036*
	(0.022)
Constant	0.201***
	(0.068)
Canvasser FE	Yes
Observations	1405

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 on whether the firm signed up to receive free competitor information. Observations are at the firm level, and includes all control firms who were available for a conversation. Standard errors are robust at the firm level.

Appendices

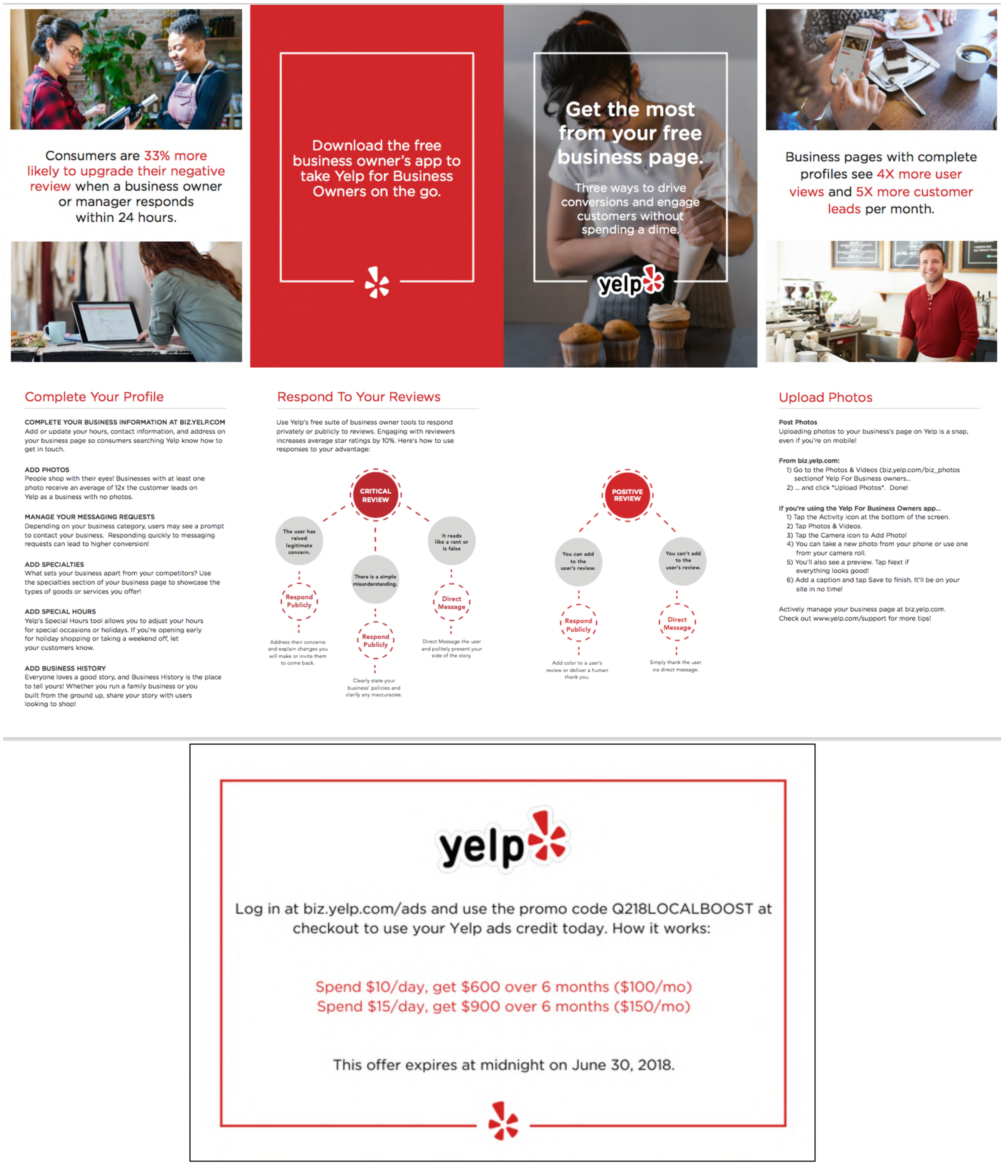
(For Online Publication)

A Experiment details

This appendix provides additional details on the experiment. Figure A.1 displays the standard marketing materials that all firms received, including those assigned to the control condition. Figure A.2 shows the distribution of messages shown on treatment postcards, as well as the distribution of control firms that would have been shown each message if they had been assigned to treatment. Figure A.3 shows the scripts used to train canvassers. Figure A.4 shows a map of all firms in the eligible set across each of the four cities, and Figure A.5 shows the subset of firms in the experimental sample. Table A.1 shows compliance and attrition across experimental conditions. Figure A.6 shows the timeline of data collection and experimental interventions.

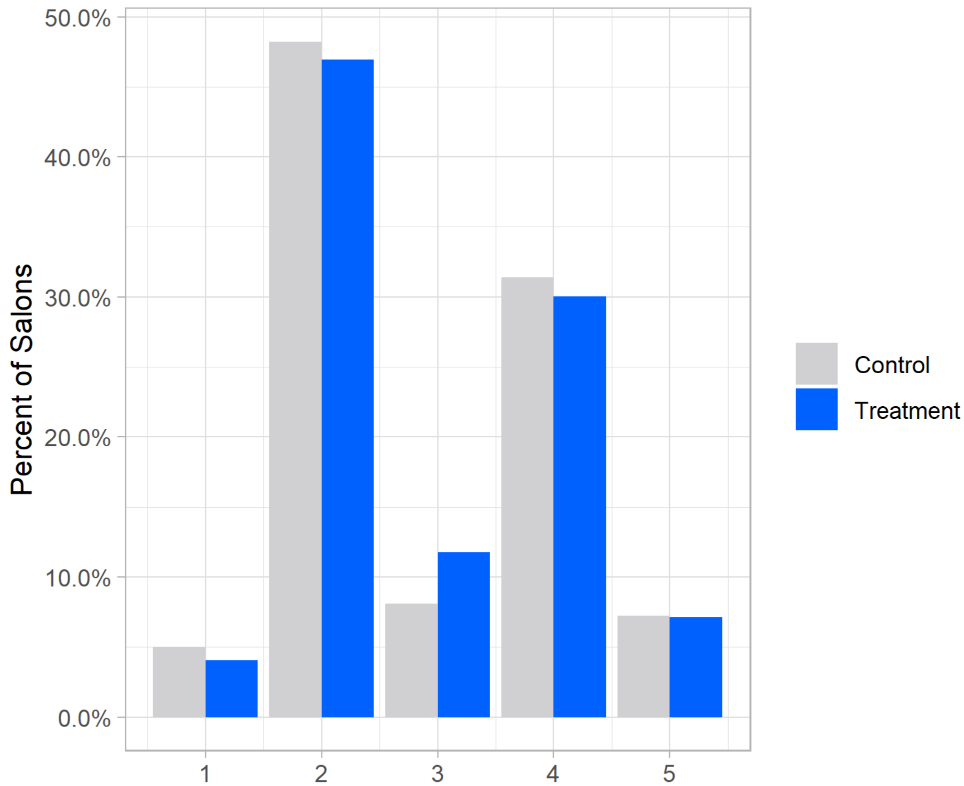
Figure A.6 categorizes notes recorded by canvassers at the time of the treatment, which capture how firms responded to the informational intervention. These notes were categorized by two research assistants, and sent to a third research assistant in the case of conflicts.

Figure A.1: Brochure and postcard provided to all firms



Notes: The top figure shows the brochure that Yelp canvassers provided to all businesses, which includes information on how to edit business details, add photos, and respond to reviews on Yelp's business page. The bottom figure shows a standard marketing postcard that Yelp additionally provided on their visits, which offers free Yelp advertising credits. The back of this postcard is blank for control businesses, and shows the competitor information treatment for treatment businesses.

Figure A.2: Distribution of treatment messages



category_rank	status
1	You charge the lowest price in the area
2	Most businesses nearby charge the same or higher prices as you
3	Most/All businesses nearby charge the same price as you
4	Most businesses nearby charge the same or lower prices as you
5	You charge the highest price in the area

Notes: This figure shows the distribution of treatment messages shown, compared with the counterfactual messages that applied to control firms (which were not provided).

Figure A.3: Canvassing script versions

1. Treatment Version: Price information canvassing

Walk up to the cashier with brochure and postcard for the business in hand

1. Hi, what's the price of your regular manicure? *(Record price.)*
2. Great! I'm from Yelp and I'm here to learn more about your salon and help you manage your free Yelp business page.
3. Are you the manager, or is there a manager I could chat with? *(Record whether they are the manager, owner, or someone else)*
4. *If they ask "what's Yelp?"* Explain that Yelp is the largest local search directory online platform where people go to find great local businesses. Basically the modern day Yellow pages *(Do a live search for their category of business to show them).*

If they say **"OK!"**:

1. To get us started, can you tell me about what you think sets your salon apart from your competitors? *(Record answer)*
2. Who do you consider as your primary competitors? *(Record all names mentioned)*
3. And what do you think they are charging for a regular manicure? *(Record manicure price)*
4. Great. We've collected some information on the prices of nail salons that are located closest to you. *(Show them the price figure on the postcard).*
5. And we've found that [Give the one-line summary written on the postcard.]
6. Would you be interested in continuing to receive this information? *(Record answer).* Got it, thank you for your time!
7. If you have a few more minutes, we would love to help you make sure your free Yelp business page is up to date. Managing your page is free, and it is important to keep it up to date so your information is correct and potential customers can find you.
8. Depending on whether the page has been claimed:
    A. *[If page has not been claimed]:* Great, is this the email you want to use to login? *(show them the email you have if you have one).* I'll make you a temporary password so you can log back in later and change it. *(proceed to sign up)*
    B. *[If page has been claimed]:* Great, is this the email you have as your login? *(show them the email you have if you have one).* If you can log in, I can show you some of the new information options we have, and we can check that all of the information is up to date *(proceed to check the page).*
9. Check their page with them and make sure hours and other information is up to date.
10. Thank you, glad I could help. Have a good day – and feel free to call this 1-800 number with any questions! *(Point to phone # on back of pamphlet)*

If they are **"not interested"**:

- Got it, just as a quick preview, [give the summary one-liner written on the postcard]
- I'll just leave the pricing information here with you *(hand over pamphlet and postcard).*
- "Thank you for your time, have a nice day" and exit the business.

2. Control Version: Standard Canvassing

Walk up to the cashier with brochure and postcard in hand

1. Hi my name is __. I'm from Yelp and I'm here to learn more about your salon and help you manage your free Yelp business page.
2. Are you the manager, or is there a manager I could chat with? *(Record whether they are the manager, owner, or someone else)*
3. *If they ask "what's Yelp?"* Explain that Yelp is the largest local search directory online platform where people go to find great local businesses. Basically the modern day Yellow pages *(Do a live search for their category of business to show them).*

<u>If they say **"OK!"**</u>:

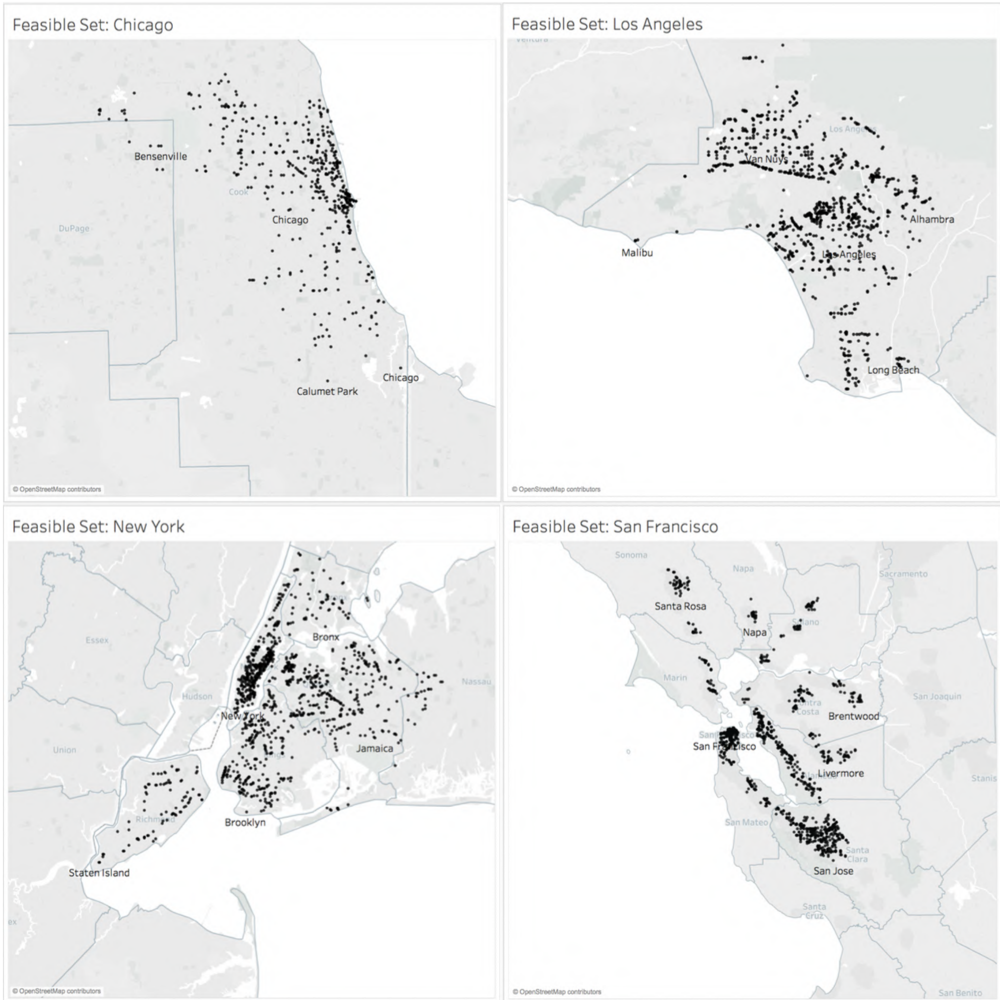
1. We would love to help you make sure your free Yelp business page is up to date. Managing your page is free, and it is important to keep it up to date so your information is correct and potential customers can find you.
2. Depending on whether the page has been claimed:
   A. *[If page has not been claimed]:* Great, is this the email you want to use to login? *(show them the email you have if you have one).* I'll make you a temporary password so you can log back in later and change it. *(proceed to sign up)*
   B. *[If page has been claimed]:* Great, is this the email you have as your login? *(show them the email you have if you have one).* If you can log in, I can show you some of the new information options we have, and we can check that all of the information is up to date *(proceed to check the page).*
3. Check their page with them and make sure hours and other information is up to date.
4. Thank you, glad I could help. Have a good day – and feel free to call this 1-800 number with any questions! *(Point to phone # on back of pamphlet)*

<u>If they are **"not interested"**</u>:

- *(Hand over pamphlet and postcard)*
- "Thank you for your time, have a nice day" and exit the business.

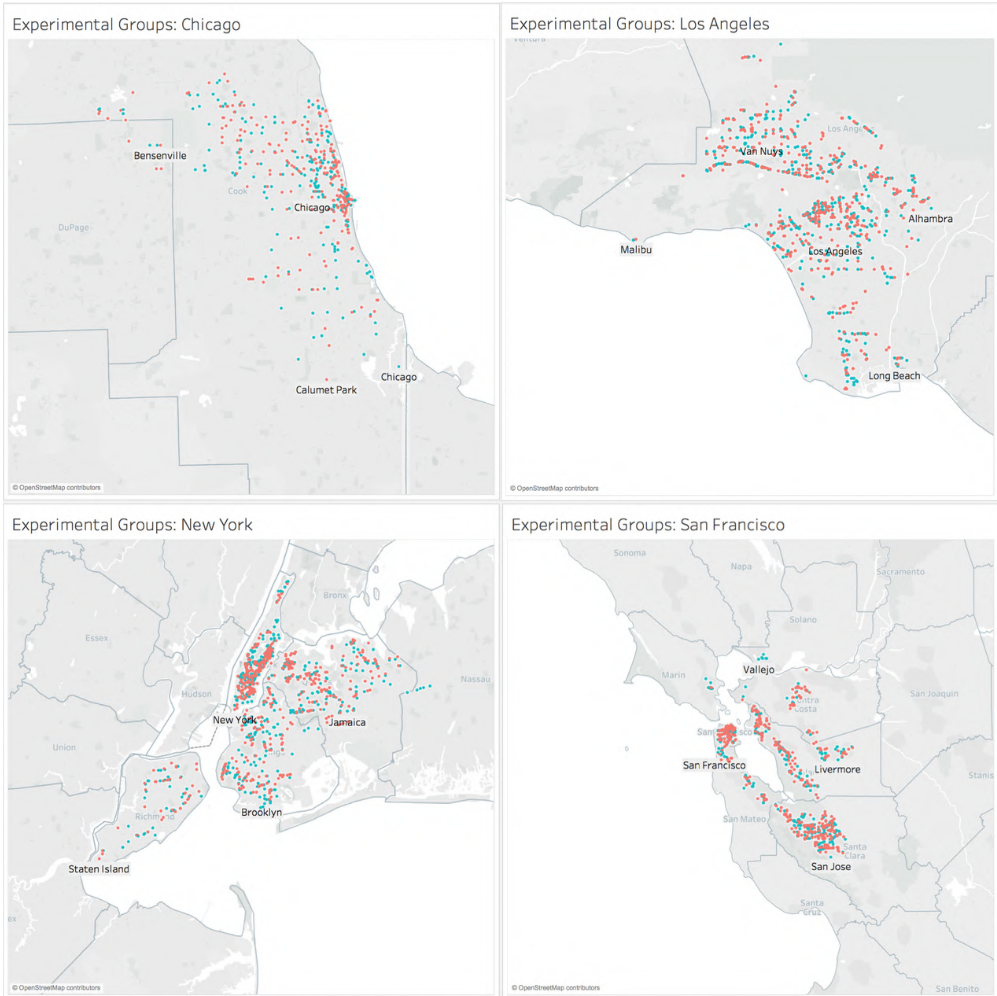
Notes: This shows the scripts that canvassers used in control and treatment conditions. One of the Yelp managers and I individually trained every canvasser by practicing the script with mock scenarios, and canvassing together for at least 3-5 hours. We checked in with every canvasser at the beginning and end of each daily shift, and were in constant communication with them throughout their shift via chat. Canvassers were not aware of the experiment.

Figure A.4: Map of firms in the eligible set



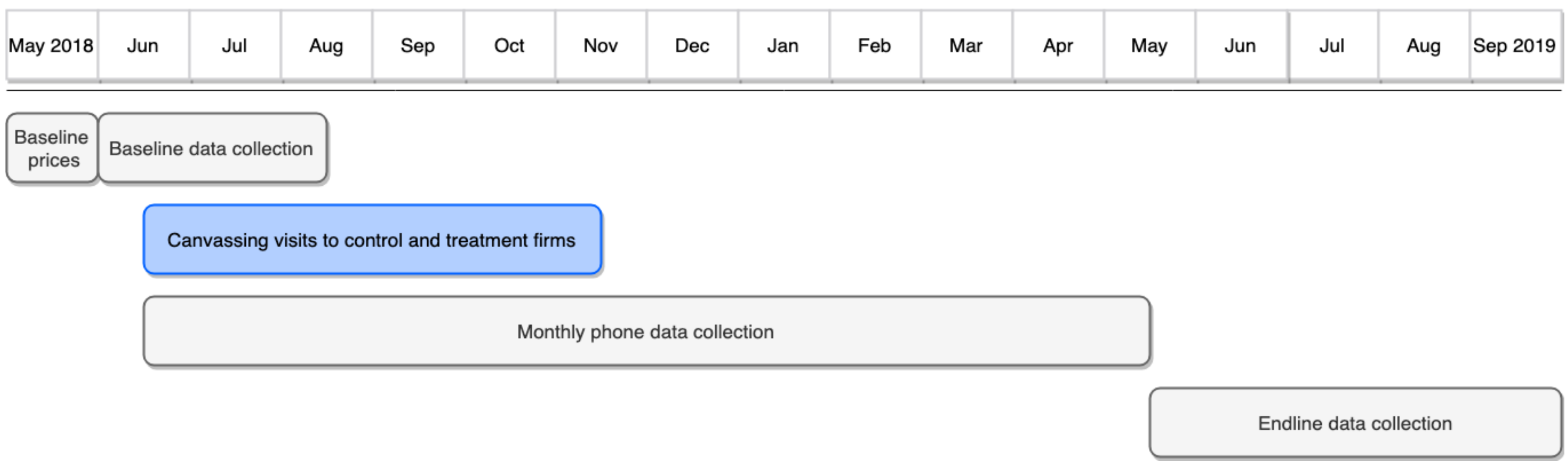
Notes: This map shows all firms in the eligible set across each of the four cities.

Figure A.5: Map of firms in the experimental sample



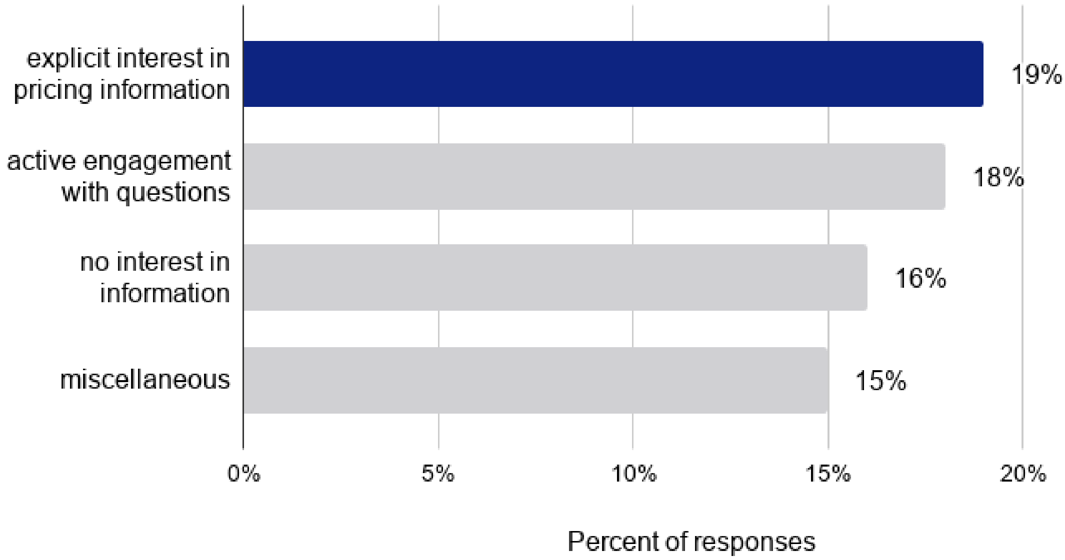
Notes: This map shows all firms in the experimental sample across each of the four cities. Control firms are in red, while treatment firms are in blue. Firms in the Bronx and outer Queens area are missing in New York, and firms in the outer North Bay area are missing for San Francisco, compared to the eligible set.

Figure A.6: Experimental timeline



Notes: This figure shows the experimental timeline. Baseline prices were collected in May 2018, and continued to be collected on a monthly basis until May 2019. Baseline quality data were collected between June - August 2018, where it was staggered by neighborhoods to ensure that data were collected before canvassing visits. Endline data were collected between June and September 2019.

Figure A.7: Comments by treatment firms at the time of treatment



Notes: This figure shows the categories of responses across treatment firms, which were noted by canvassers that delivered the informational treatment. Canvassers recorded comments as close to verbatim as possible. Two research assistants later coded these comments into categories, with any conflicts sent to a third research assistant.

Table A.1: Compliance and attrition across experimental conditions

	(1) Treatment *Number of Firms*	(2) Treatment *% of Firms*	(3) Control *Number of Firms*	(4) Control *% of Firms*	(5) p-value
Non-compliance	25	1.58	33	2.01	0.36
Closed	88	5.58	73	4.45	0.14
No price data	20	1.27	16	0.98	0.43
Observations	1578	1578	1640	1640	3218

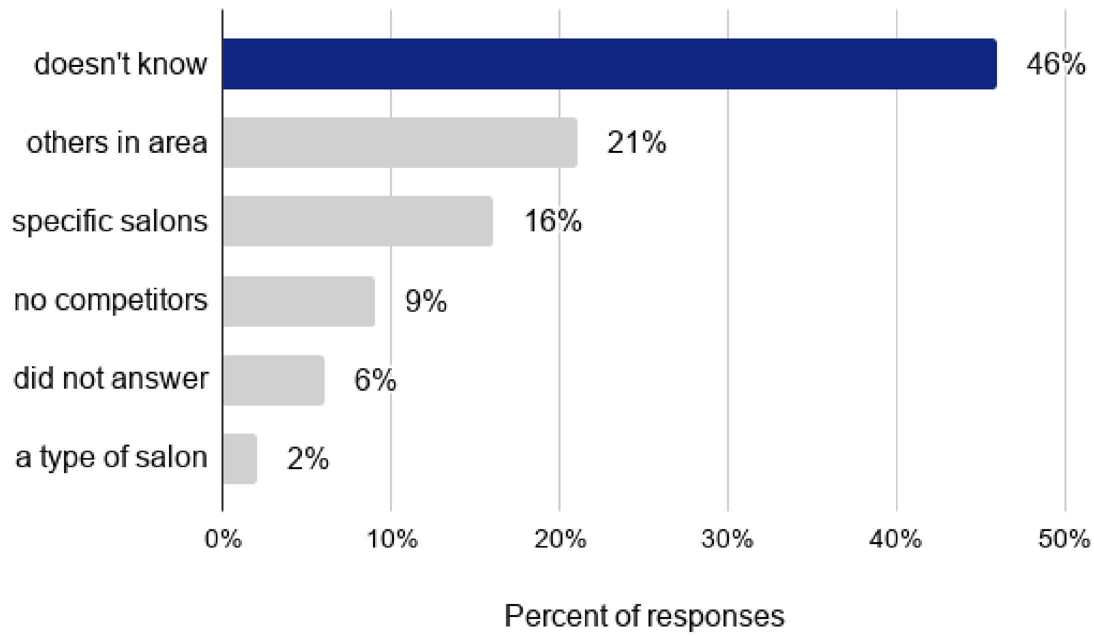
Notes: Non-compliance denotes firms that rejected any conversation with Yelp canvassers when they arrived. In these cases, the firm did not receive any information from the canvassers. "Closed" represents firms confirmed as closed or no longer offering nail services after the canvassing visit. "No price data" represents firms that were no longer reachable after the canvassing visit but not confirmed as closed or no longer offering nail services. Column 5 shows the p-value of the difference between treatment and control firms.

B Baseline knowledge of competitors

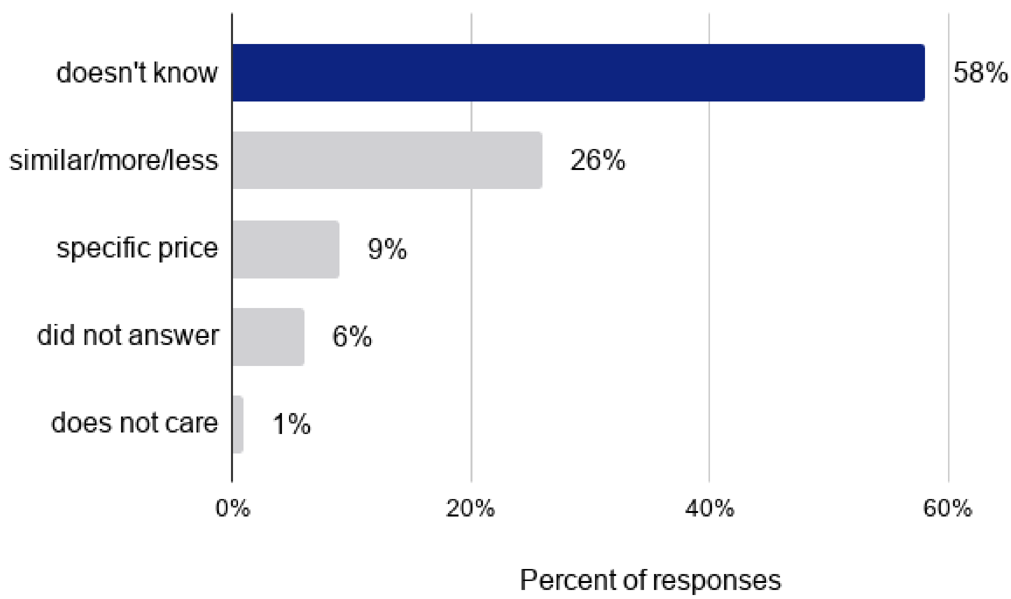
This appendix shows stated measures of baseline knowledge of competitors at treatment firms. Figure B.1 categorizes managers' responses to questions on their primary competitors. Figure B.2 further disaggregates responses in the category, "others in area". Figure B.3-4 analyze how the stated baseline knowledge of competitors varied by the level of competition faced, measured by the firm's distance from the nearest competitor and the baseline price dispersion across its 9 nearest competitors. Figures B.5-7 show how the stated baseline competitor knowledge varied by whether the firm charged higher- or lower-end prices, age, and size.

Figure B.1: Baseline knowledge of competitors

(a) Knowledge of primary competitors across managers at treatment firms

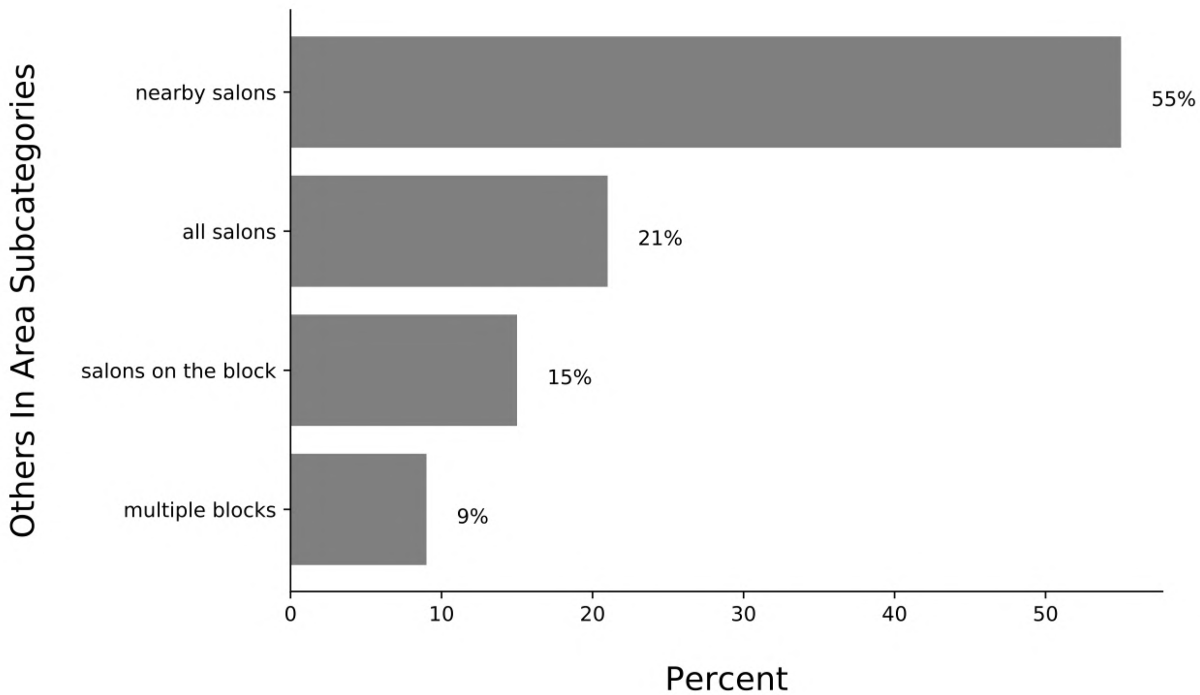


(b) Knowledge of competitor pricing across managers at treatment firms



Notes: Figure (a) shows the breakdown of manager responses to the question "who do you consider as your primary competitors?" across 1,383 (out of 1,578) treatment firms with whom Yelp canvassers were able to have a conversation to deliver pricing information. Any salons unwilling or too busy to answer the question, or disinterested in answering follow-up questions or continuing the conversation, were counted as "did not answer". Figure (b) shows the breakdown of responses to the question "what do you think [your primary competitor(s)] charge for a regular manicure?" asked by Yelp canvassers to treatment firms.

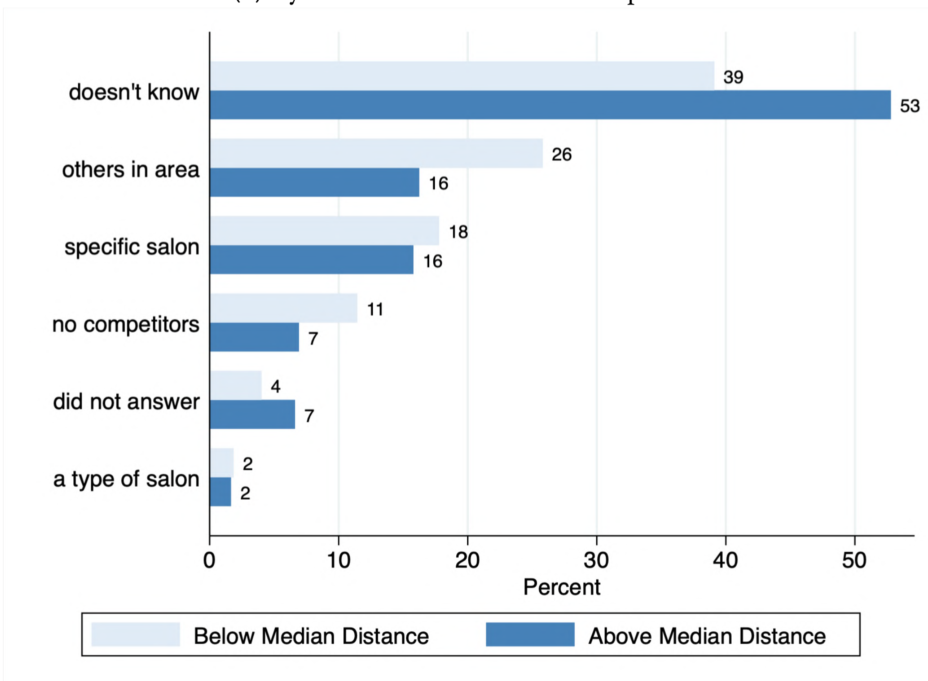
Figure B.2: Breakdown of responses categorized as "others in area" to describe competitors



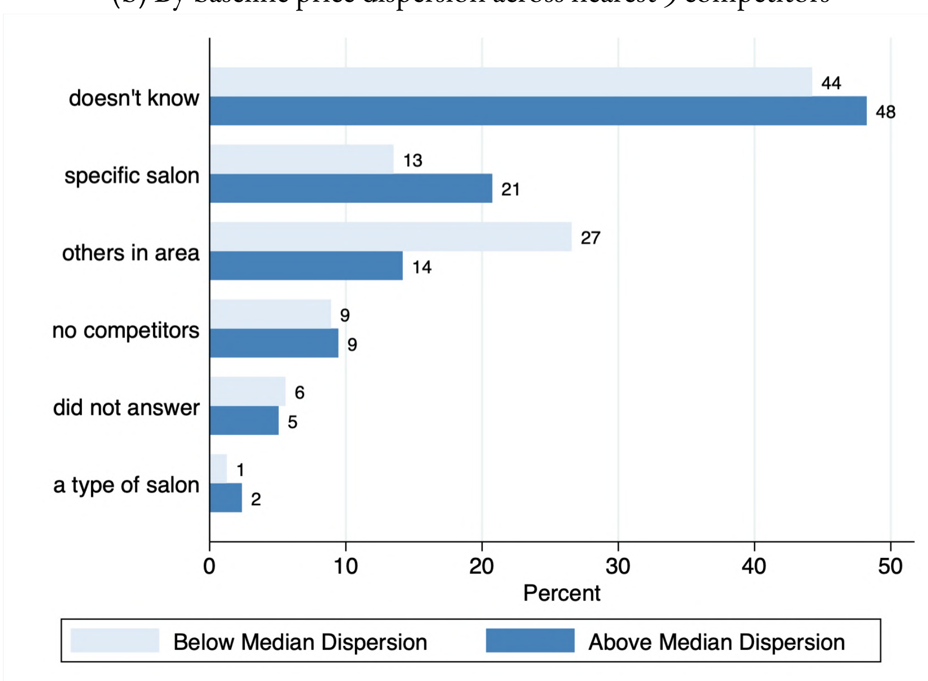
Notes: This figure shows the breakdown of 275 responses in "others in area", based on the four types of phrasing used to describe other competitors in the area: all salons in the area, nearby salons, salons on the block, and multiple blocks.

Figure B.3: Knowledge of primary competitors by level of competition

(a) By distance from nearest competitor



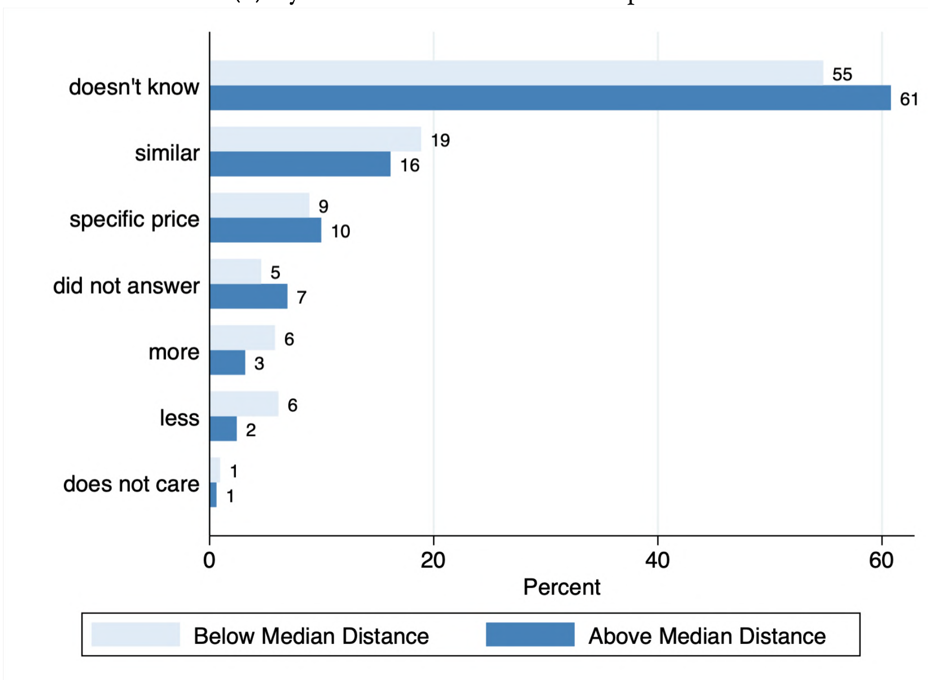
(b) By baseline price dispersion across nearest 9 competitors



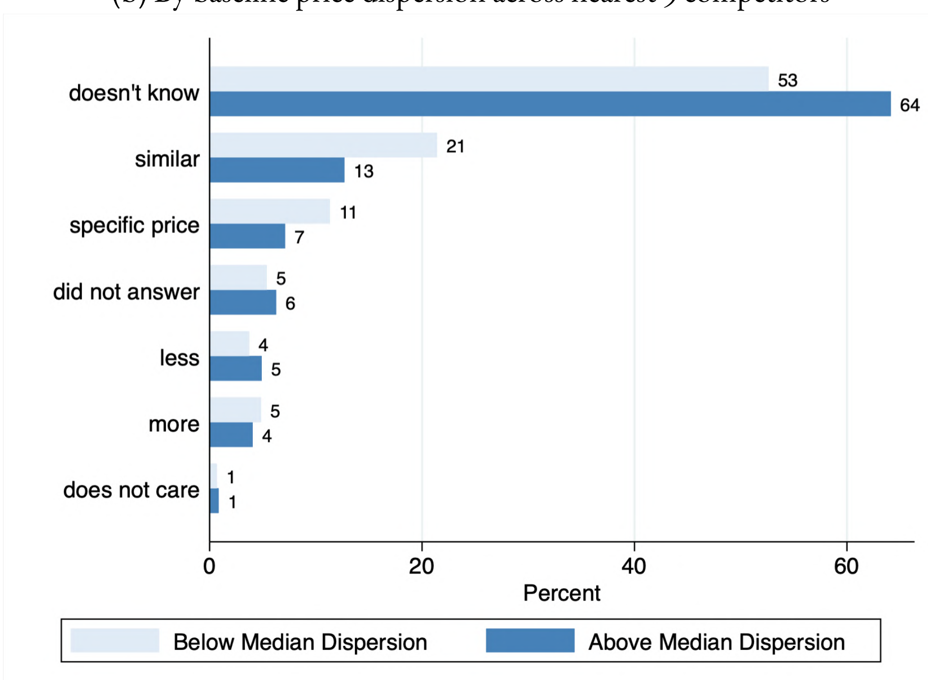
Notes: These figures break down managers' responses on their knowledge of competitors by two measures that proxy the level of competition. (a) uses the firm's distance from its nearest competitor as a measure of competition. (b) uses baseline price dispersion across its nearest 9 competitors as a measure of competition. For both of these measures, "below median" distance and dispersion map to higher levels of competition, as they suggest that competitors are closer by and less dispersed in prices.

Figure B.4: Knowledge of competitor pricing by level of competition

(a) By distance from nearest competitor



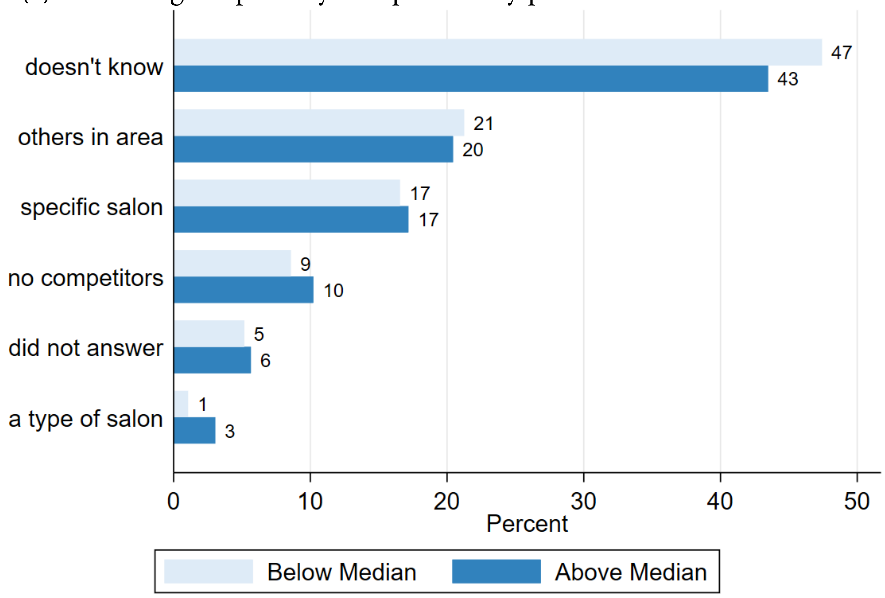
(b) By baseline price dispersion across nearest 9 competitors



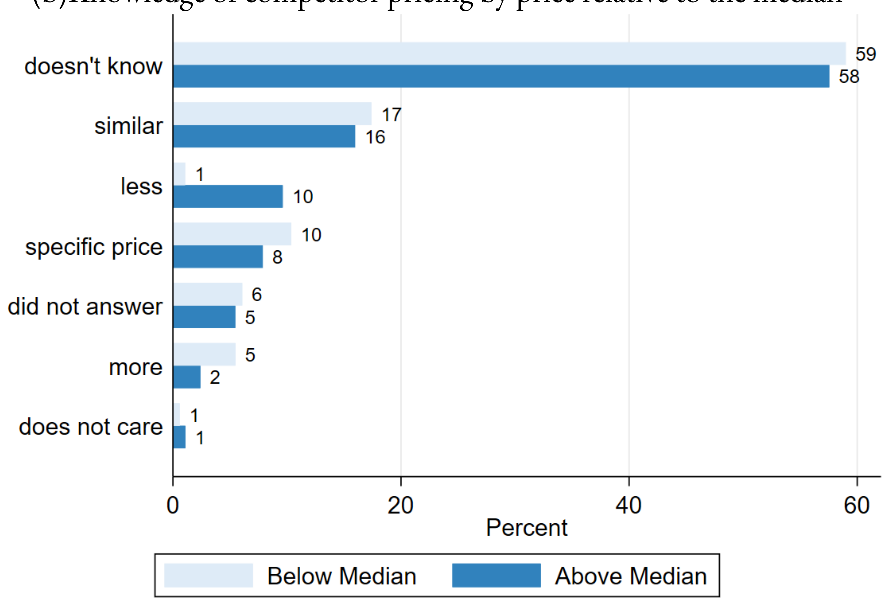
Notes: These figures break down managers' responses on their knowledge of competitor prices by two measures that proxy the level of competition. (a) uses the firm's distance from its nearest competitor as a measure of competition. (b) uses baseline price dispersion across its nearest 9 competitors as a measure of competition. For both of these measures, "below median" distance and dispersion map to higher levels of competition, as they suggest that competitors are closer by and less dispersed in prices.

Figure B.5: Knowledge of competitors across higher- and lower-end firms (relative to median price in ZIP code)

(a) Knowledge of primary competitors by price relative to the median



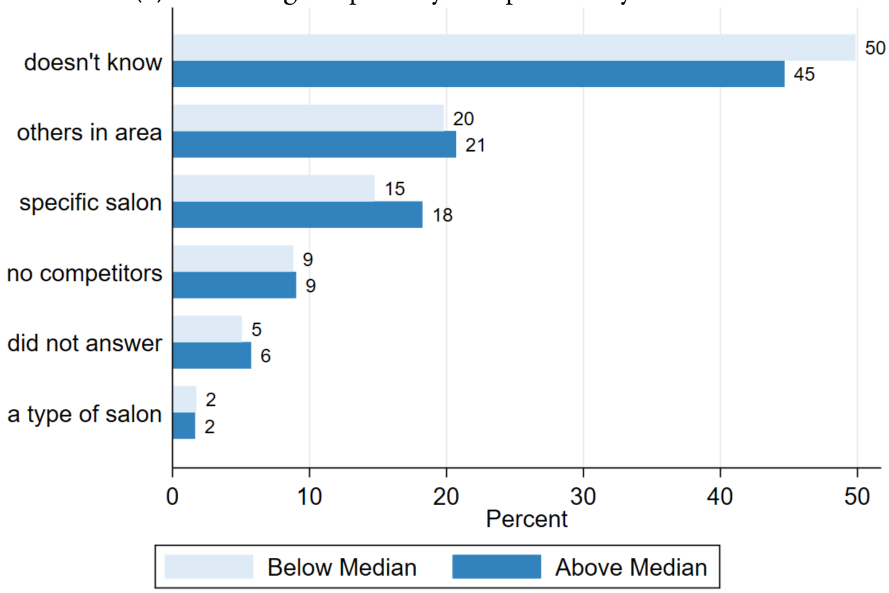
(b)Knowledge of competitor pricing by price relative to the median



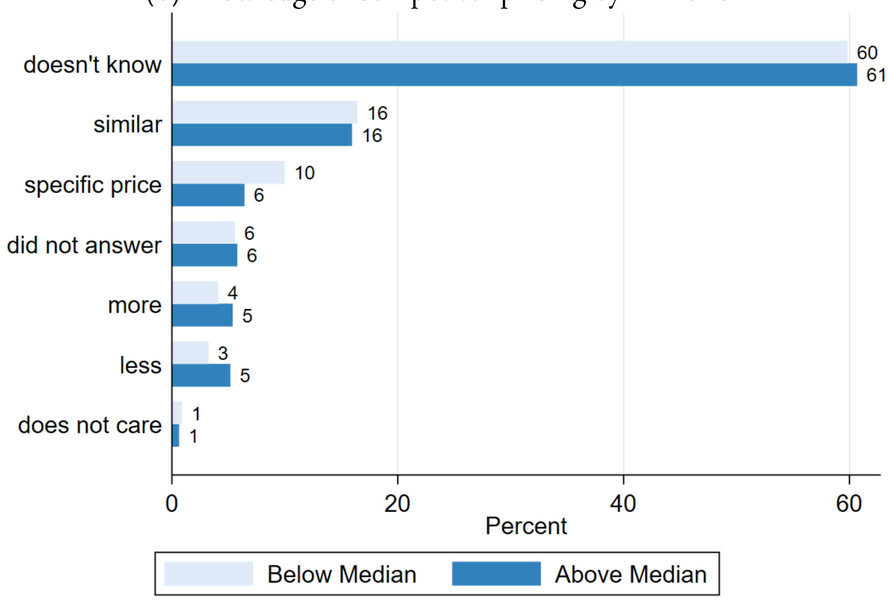
Notes: These figures break down managers' responses on their knowledge of competitors by whether the firm charged above- or below-median price in its ZIP code. (a) displays responses on primary competitors, and (b) displays responses on competitor prices.

Figure B.6: Knowledge of competitors by firm size

(a) Knowledge of primary competitors by firm size



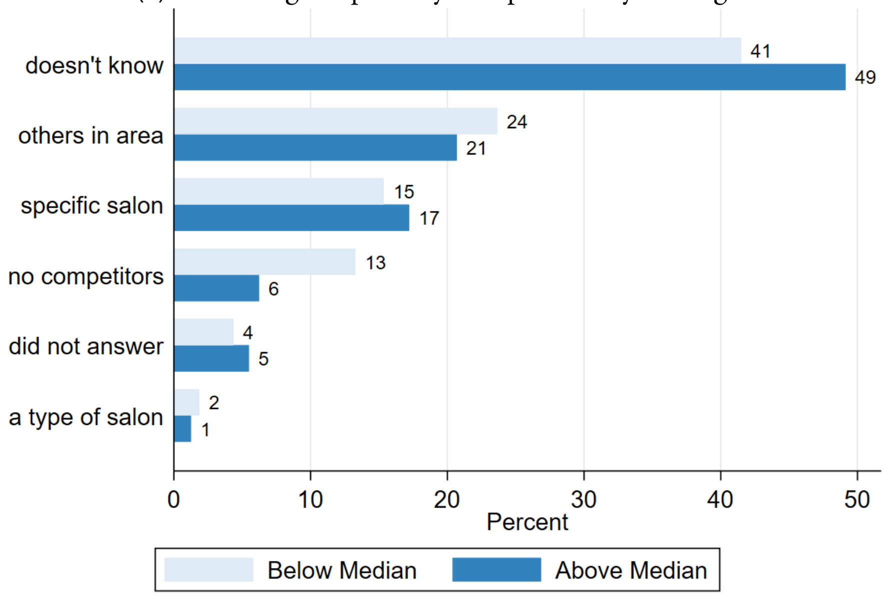
(b)Knowledge of competitor pricing by firm size



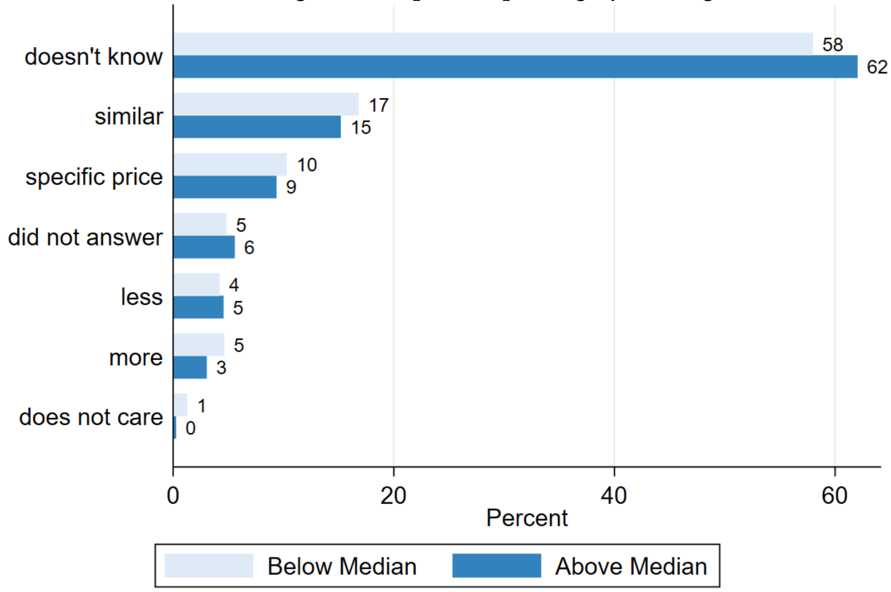
Notes: These figures break down managers' responses on their knowledge of competitors by the number of employees relative to the median size. (a) displays responses on primary competitors, and (b) displays responses on competitor prices.

Figure B.7: Knowledge of competitors by firm age

(a) Knowledge of primary competitors by firm age



(b)Knowledge of competitor pricing by firm age



Notes: These figures break down managers' responses on their knowledge of competitors by the number of years they have been open relative to the median. (a) displays responses on primary competitors, and (b) displays responses on competitor prices.

C Construction of quality measures

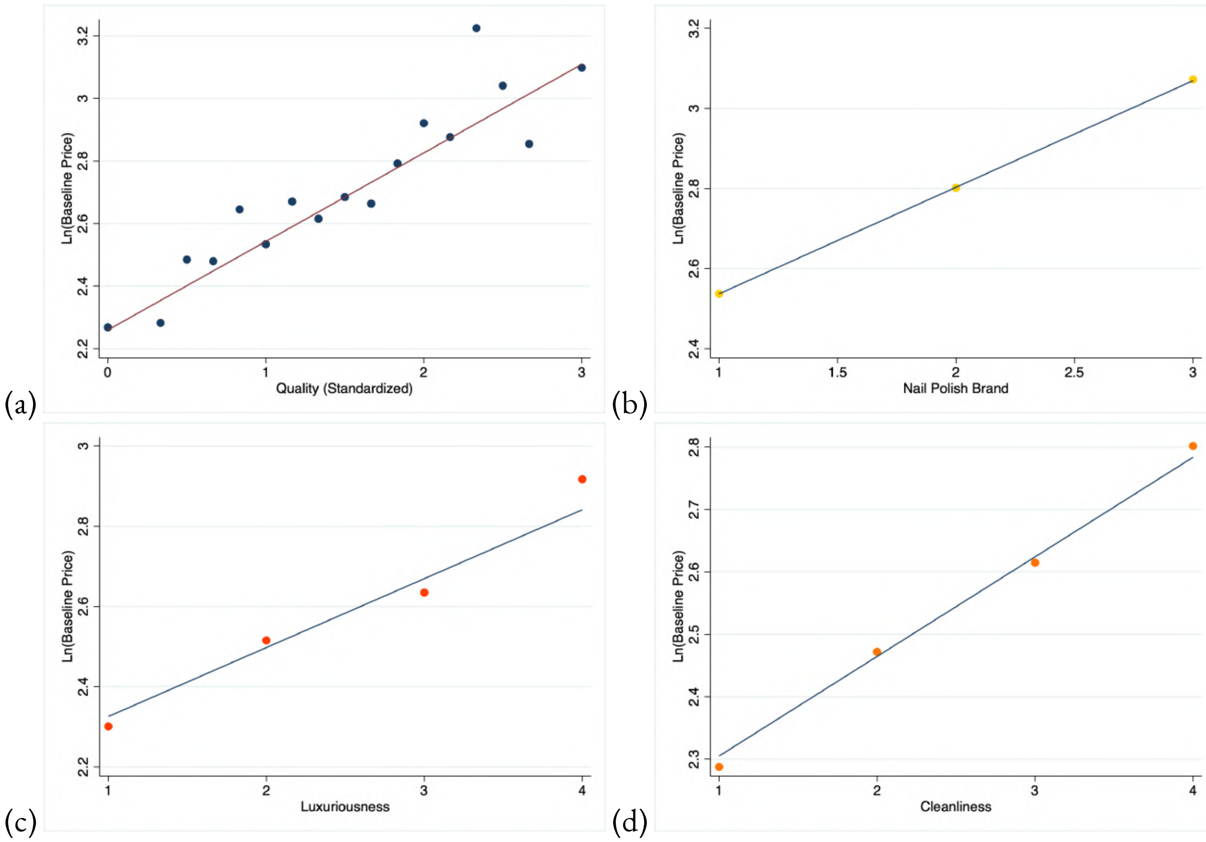
Table C.1: Rubric to code cleanliness and luxuriousness

Instructions: Please rate the salon's cleanliness and luxuriousness, assigning the rating using the following guidelines. If you are in between categories and see any of what is listed for a lower rating, record the lower rating. If for any reason you cannot observe the salon interior, enter NA.	
Cleanliness	
1	Grime on countertops and/or nail clippings on floors, technicians are wearing their own outside clothing and no gloves, technicians are reusing tools after each customer, pedicure bath is reused after a customer finishes
2	General disarray or grime on countertops and floors, technicians are wearing their own outside clothing and no gloves, technicians are using some disinfection (e.g. UV lighting machine), pedicure bath is washed with water after a customer finishes
3	Generally clean countertops and floors, technicians are wearing some type of uniform but may not be wearing gloves, technicians are using liquid disinfection, pedicure bath appears to be disinfected after a customer finishes
4	The floor and surfaces are spotless, technicians are wearing neat clothing and gloves, tools are disposable and/or salon has an autoclave, pedicure area is being disinfected for at least 10min after a customer finishes
Luxuriousness	
1	Small and cramped service area, no waiting area, no investment into decor (furniture, upholstery, or art) with stained walls and/or broken fixtures, no amenities provided
2	Small but comfortable service areas, some reception area even if small and not clearly separate from the rest of the salon, no broken fixtures or wall stains but little investment into decor, basic amenities (e.g. candy) may be provided
3	Spacious service area, small but separate reception area, some investment into decor (furniture, upholstery, or art), some amenities provided (e.g. water, disposable slippers, reading material)
4	Spacious and private or luxurious service area, security and/or spacious waiting area, high investment into decor (furniture, upholstery, or art), many amenities provided (e.g. drinks of choice, snacks, diversity of reading material, slippers/gowns)

Notes: This table shows the rubric that data collectors used to code cleanliness and luxuriousness. Data collectors were required to take accompanying photos of the interior, polish brands, menu, and exterior to validate their codings. 5% of each data collector's photos were checked every week.

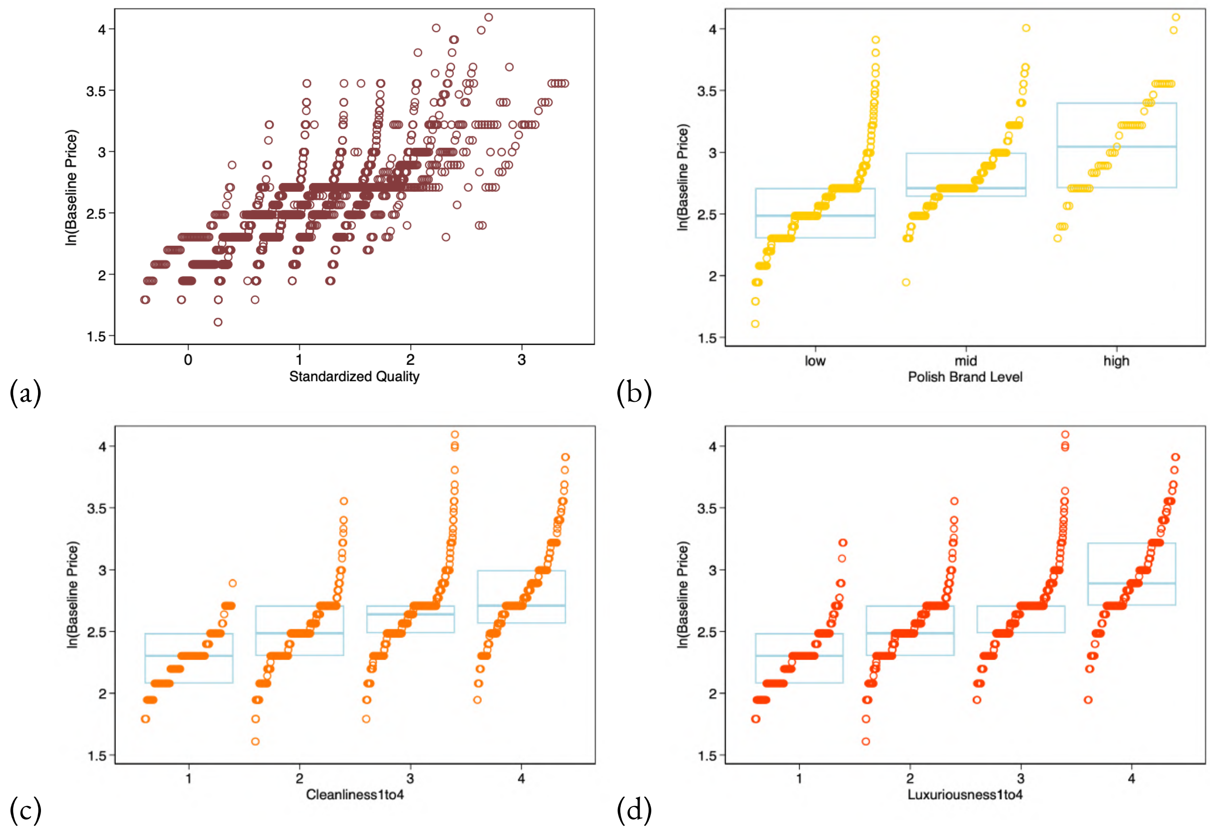
D Baseline pricing and quality

Figure D.1: Average price across quality measures



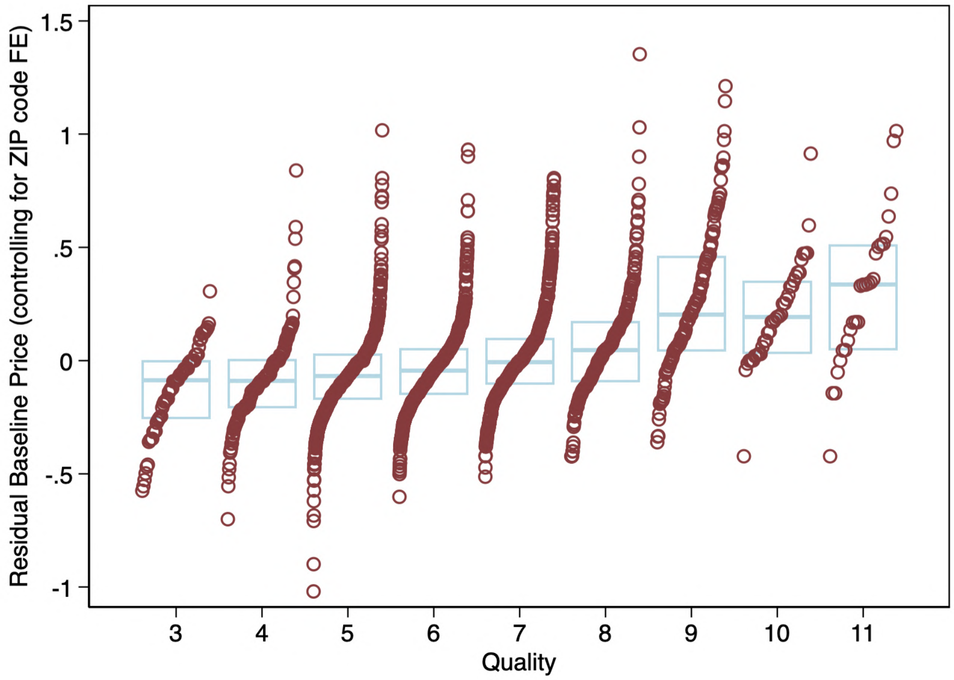
Notes: These figures plot the binscatter of logged baseline price on measures of baseline quality. (a) plots a standardized measure of baseline quality (a standardized sum of polish brands, cleanliness, and luxuriousness), and (b)-(d) plot each individual measure alone.

Figure D.2: Price dispersion across quality measures



Notes: These figures plot logged baseline price on measures of baseline quality, showing every firm observation (represented by a circle) within each quality level sorted by price, along with the interquartile range. (a) plots the standardized sum of polish brands, cleanliness, and luxuriousness, and (b)-(d) plot each individual measure alone.

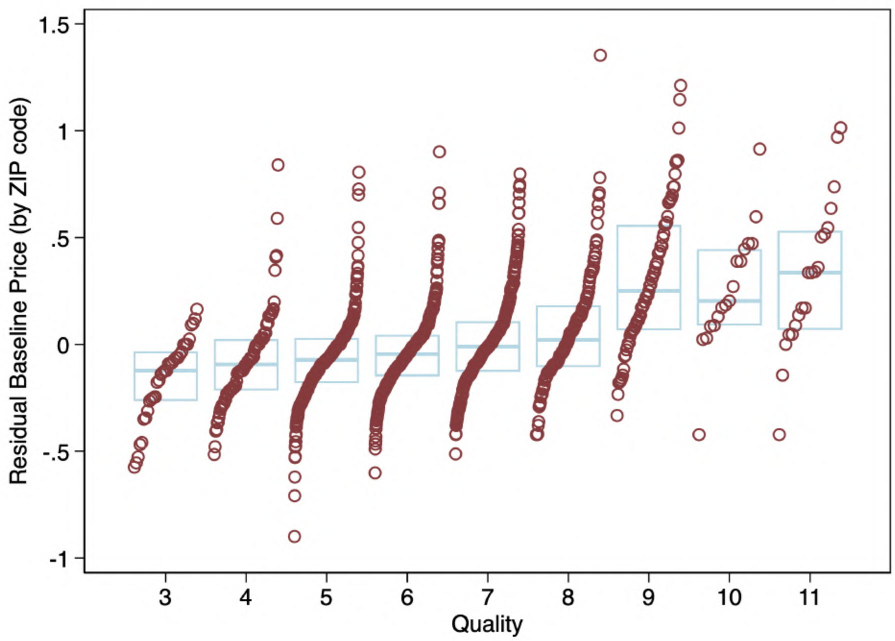
Figure D.3: Residual dispersion in firm pricing by quality level, controlling for ZIP code fixed effects



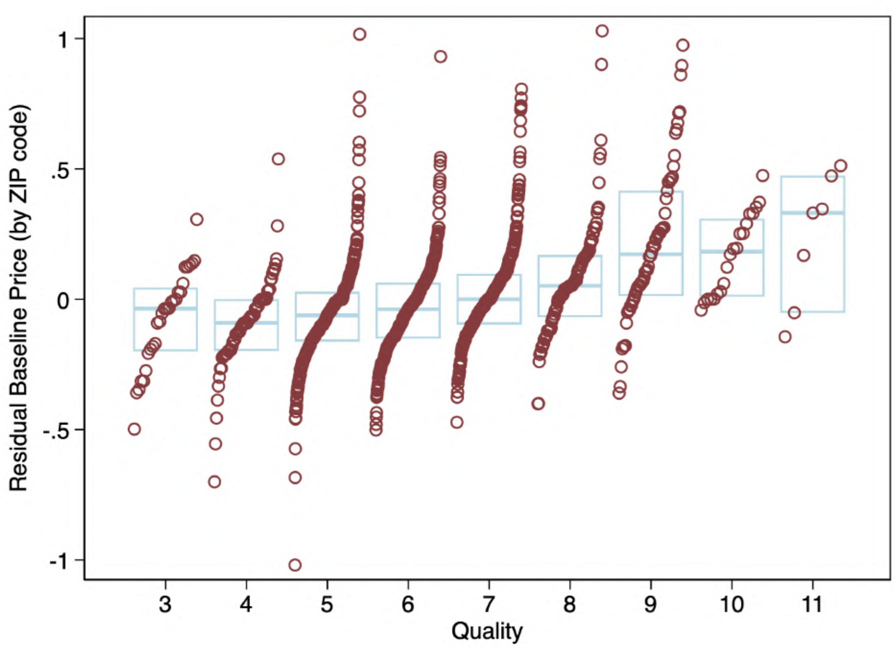
Notes: This figure plots a binscatter of residualized logged baseline price on baseline quality. Quality represents a sum of the firm's polish brand level, cleanliness, and luxuriousness, and ranges from 3 (lowest) to 11 (highest). This is robust to using a standardized sum of polish brands, cleanliness, and luxuriousness, as well as each individual measure alone.

Figure D.4: Dispersion in price-quality positions by level of competition

(a) Below median distance from nearest competitor

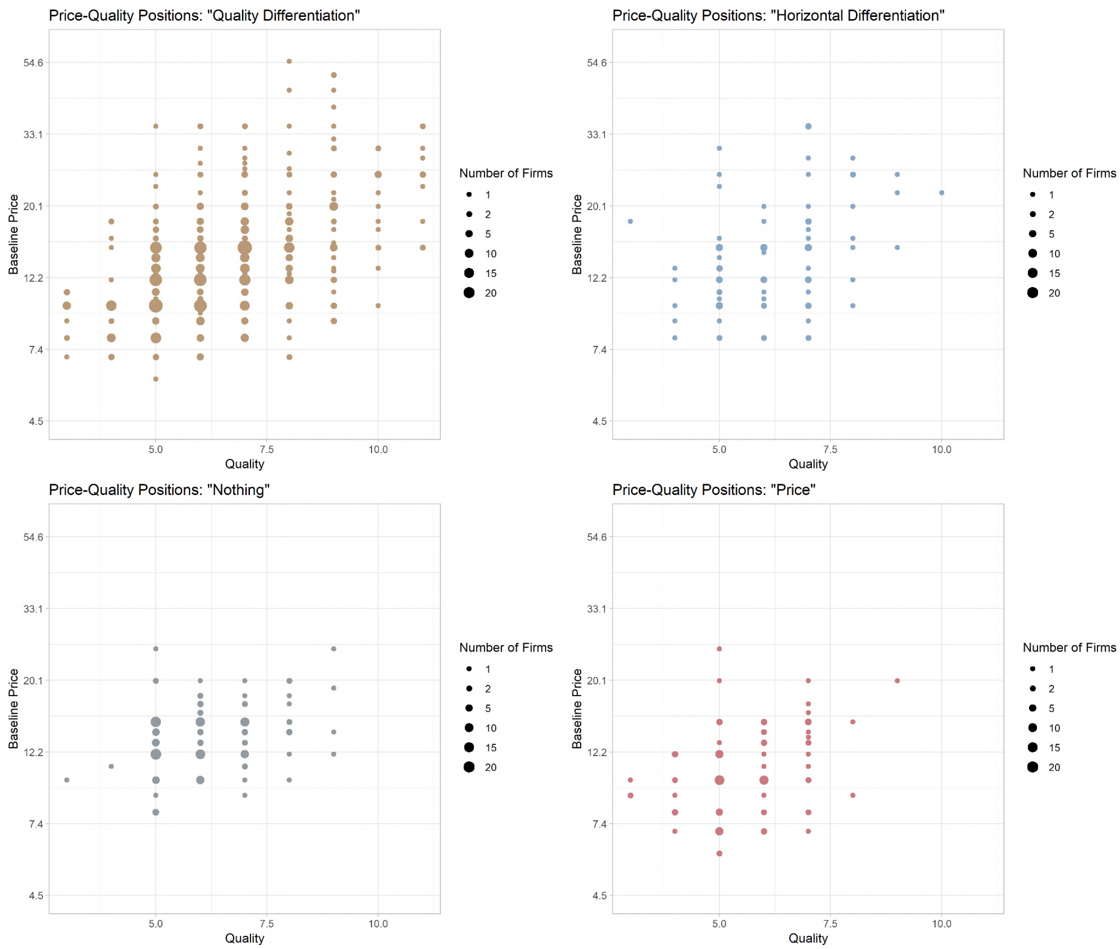


(b) Above median distance from nearest competitor



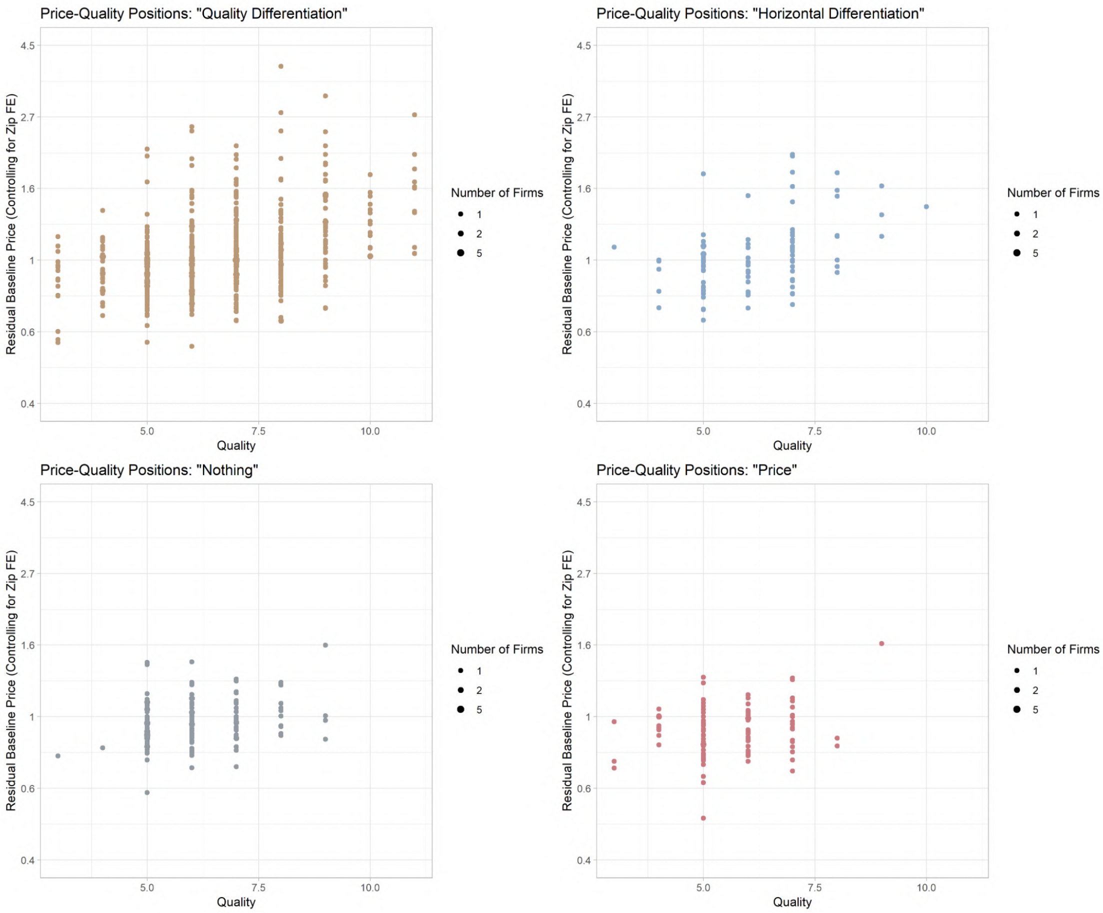
Notes: This figure separates Appendix Figure D.4 into below and above median distance from the nearest competitor to show the level of dispersion in price-quality positions by competition level.

Figure D.5: Price-quality positions by self-descriptions of positioning



Notes: These figures plot firms by managers' stated positioning descriptions for the largest four response types (quality differentiation, variants of horizontal differentiation, nothing, price) and show their actual pricing and quality decisions. The size of the dot indicates the number of firms clustered at a given position.

Figure D.6: Residual price-quality positions by self-descriptions of positioning

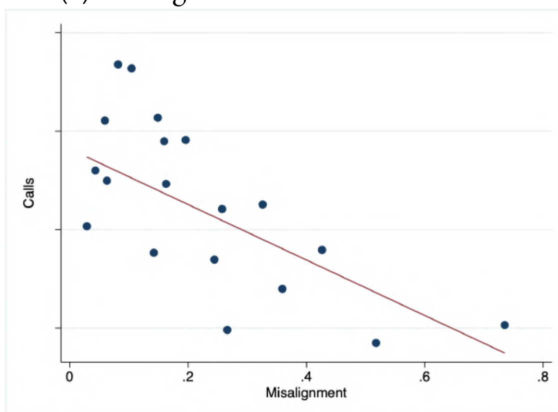


Notes: These figures plot firms by their stated positioning descriptions for the largest four response types (quality differentiation, variants of horizontal differentiation, nothing, price) and show their actual pricing and quality decisions. The y-axis plots residual baseline price, after controlling for ZIP code fixed effects. The size of the dot indicates the number of firms clustered at a given position.

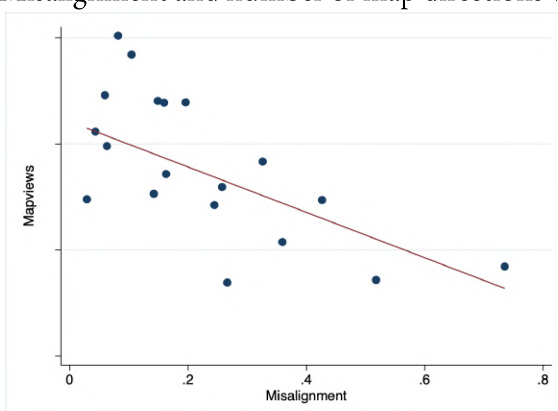
E Baseline price-quality misalignment and performance

Figure E.1: Misalignment and performance

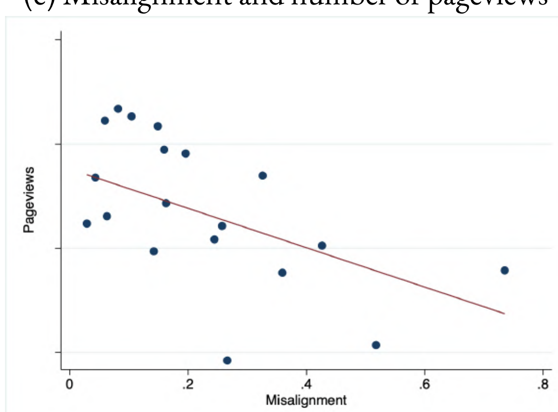
(a) Misalignment and number of calls



(b) Misalignment and number of map directions views



(c) Misalignment and number of pageviews



Notes: These figures plot the binscatter of baseline performance measures on baseline misalignment in pricing and quality, which is measured as the absolute error from the best-fit line regressing baseline price on quality and ZIP code fixed effects. (a)-(c) plot the natural log of the number of calls, map direction views, and page views on Yelp, respectively.

Table E.1: Relationship between price-quality misalignment and performance at baseline

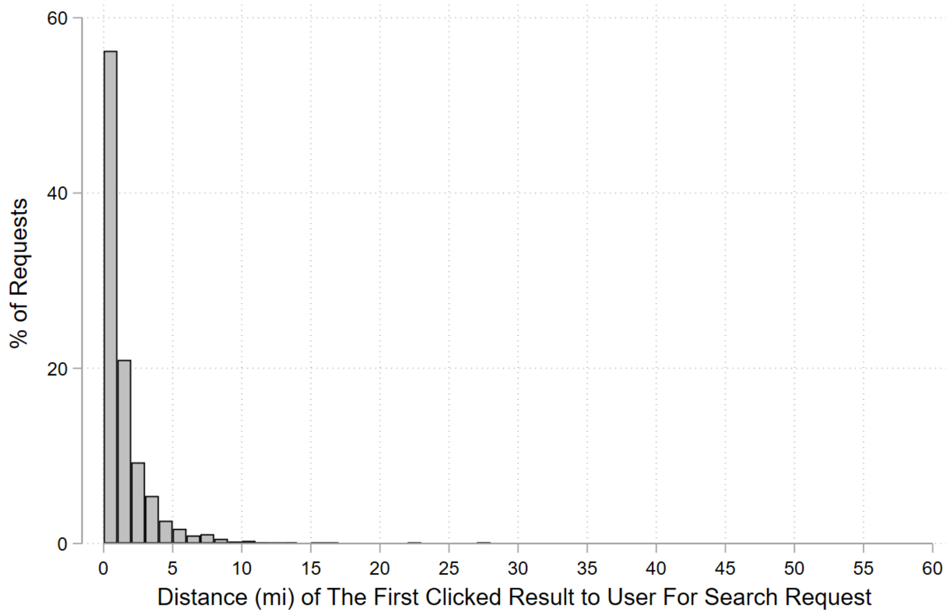
	(1) Calls	(2) Pageviews	(3) Map Directions Views
Misalignment	-0.714***	-0.588***	-0.501**
	(0.226)	(0.171)	(0.216)
Price	-0.010	0.014**	-0.021**
	(0.009)	(0.007)	(0.009)
Rating on Yelp	0.529***	0.424***	0.772***
	(0.068)	(0.053)	(0.066)
Number of Yelp Reviews	0.010***	0.009***	0.009***
	(0.001)	(0.001)	(0.001)
Constant	-0.708	3.060***	-0.916
	(0.564)	(0.368)	(0.597)
Zip Code FE	Yes	Yes	Yes
Year Opened FE	Yes	Yes	Yes
Observations	1965	1965	1965

Notes: This table reports regression results regressing baseline proxies of performance (natural logs of the number of calls, page views, and map direction views on Yelp) on baseline misalignment, price, Yelp rating, Yelp number of reviews, and fixed effects for ZIP code and year opened. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

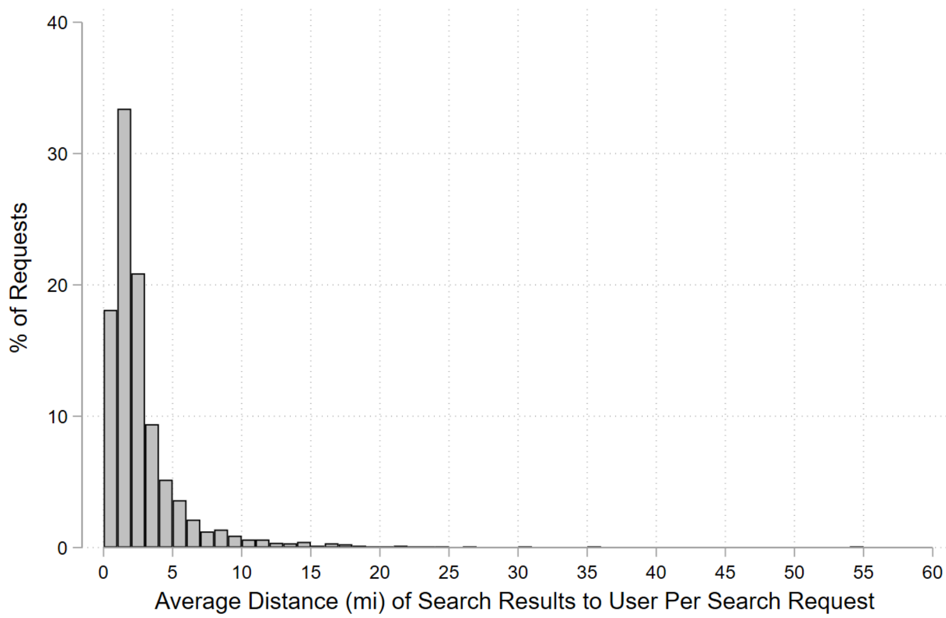
F Consumer search on Yelp

Figure F.1: Customer distance to the first clicked business in Yelp search requests

(a) Customer distance to the first clicked business



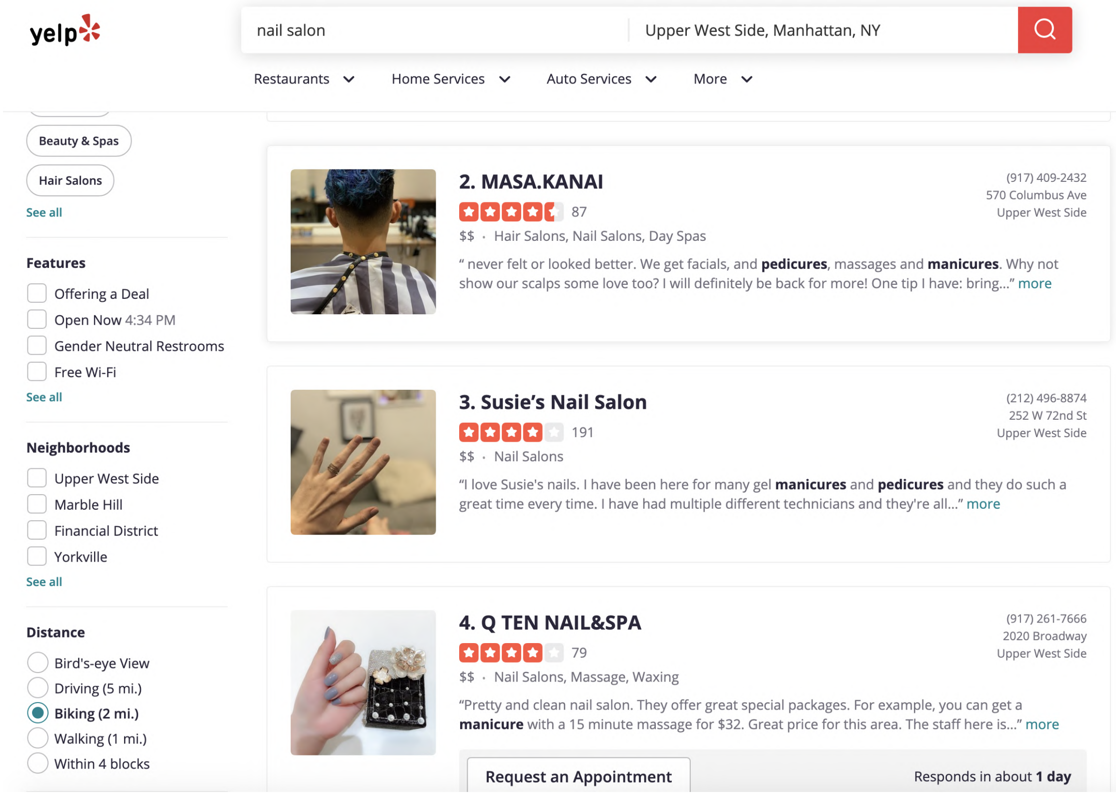
(b) Average customer distance to businesses on first page of search results



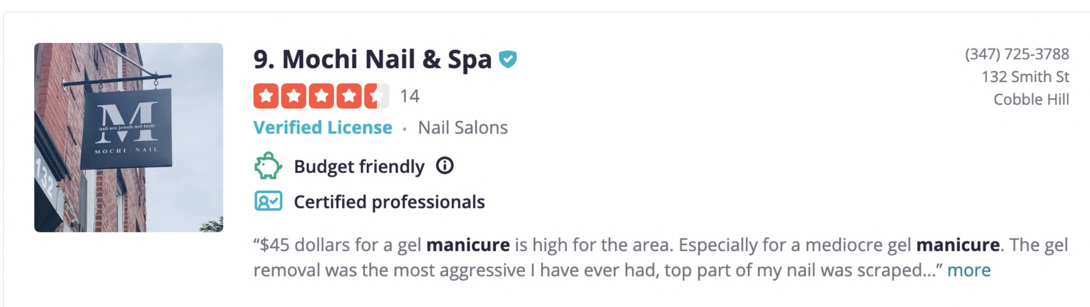
Notes: These figures are based on data on all customer search requests for nail salons in a given week in the city of Los Angeles, which is the geographic market with the most geographically dispersed salons within this experiment. Figure (a) plots a histogram of the distance (in miles) to the business that the user clicks on first after conducting a Yelp search request. Figure (b) plots a histogram of the average distance (in miles) to businesses shown on the first page of Yelp search results, as a comparison point for (a) to better inform how distance features in consumer search behavior.

Figure F.2: Example of a Search Result Page on Yelp

(a) Example of a search result page



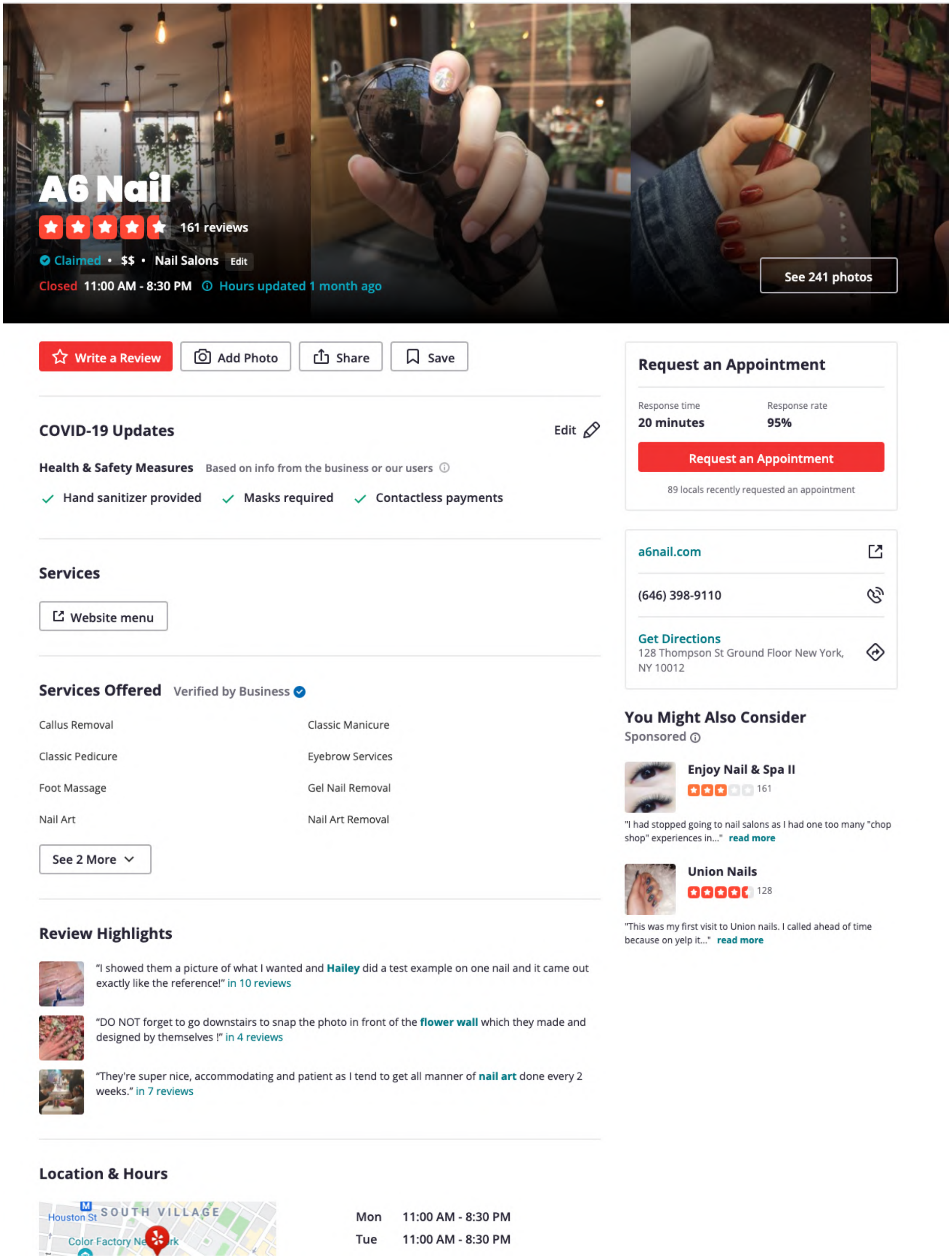
(b) Example of a search result highlighting business prices



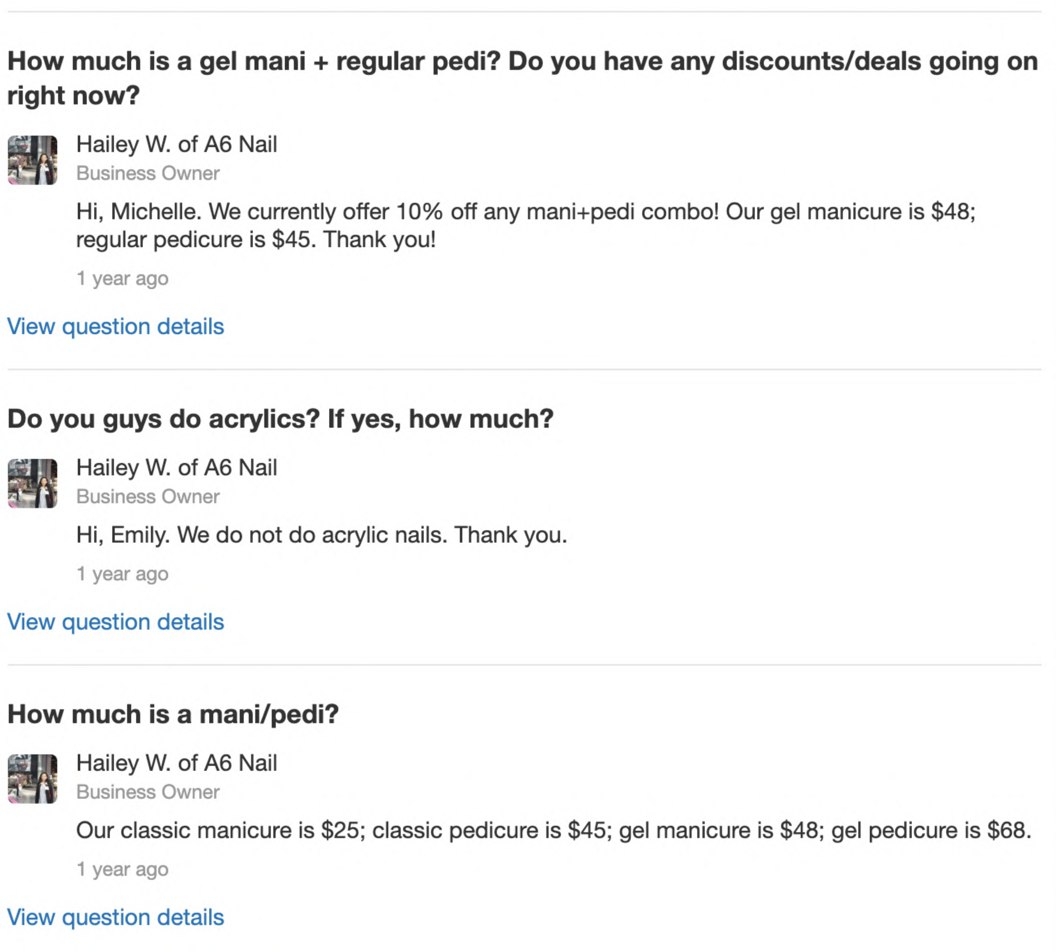
Notes: These figures display an example of search results pages on Yelp. Figure (a) shows an example of a search result page, as conducted using a search for a nail salon in New York in April 2020. Figure (b) highlights a specific search result from this search result page that highlights pricing details.

Figure F.3: Example of a business page on Yelp

(a) Sample business page



(b) Example of a Q&A section on the business page



How much is a gel mani + regular pedi? Do you have any discounts/deals going on right now?

Hailey W. of A6 Nail
Business Owner

Hi, Michelle. We currently offer 10% off any mani+pedi combo! Our gel manicure is $48; regular pedicure is $45. Thank you!

1 year ago

View question details

Do you guys do acrylics? If yes, how much?

Hailey W. of A6 Nail
Business Owner

Hi, Emily. We do not do acrylic nails. Thank you.

1 year ago

View question details

How much is a mani/pedi?

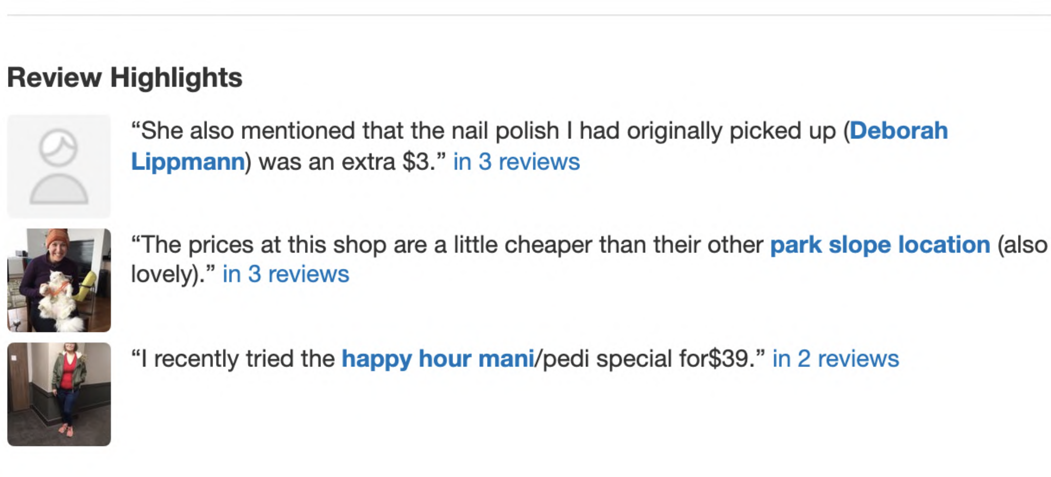
Hailey W. of A6 Nail
Business Owner

Our classic manicure is $25; classic pedicure is $45; gel manicure is $48; gel pedicure is $68.

1 year ago

View question details

(c) Example of a Review Highlights section on the business page



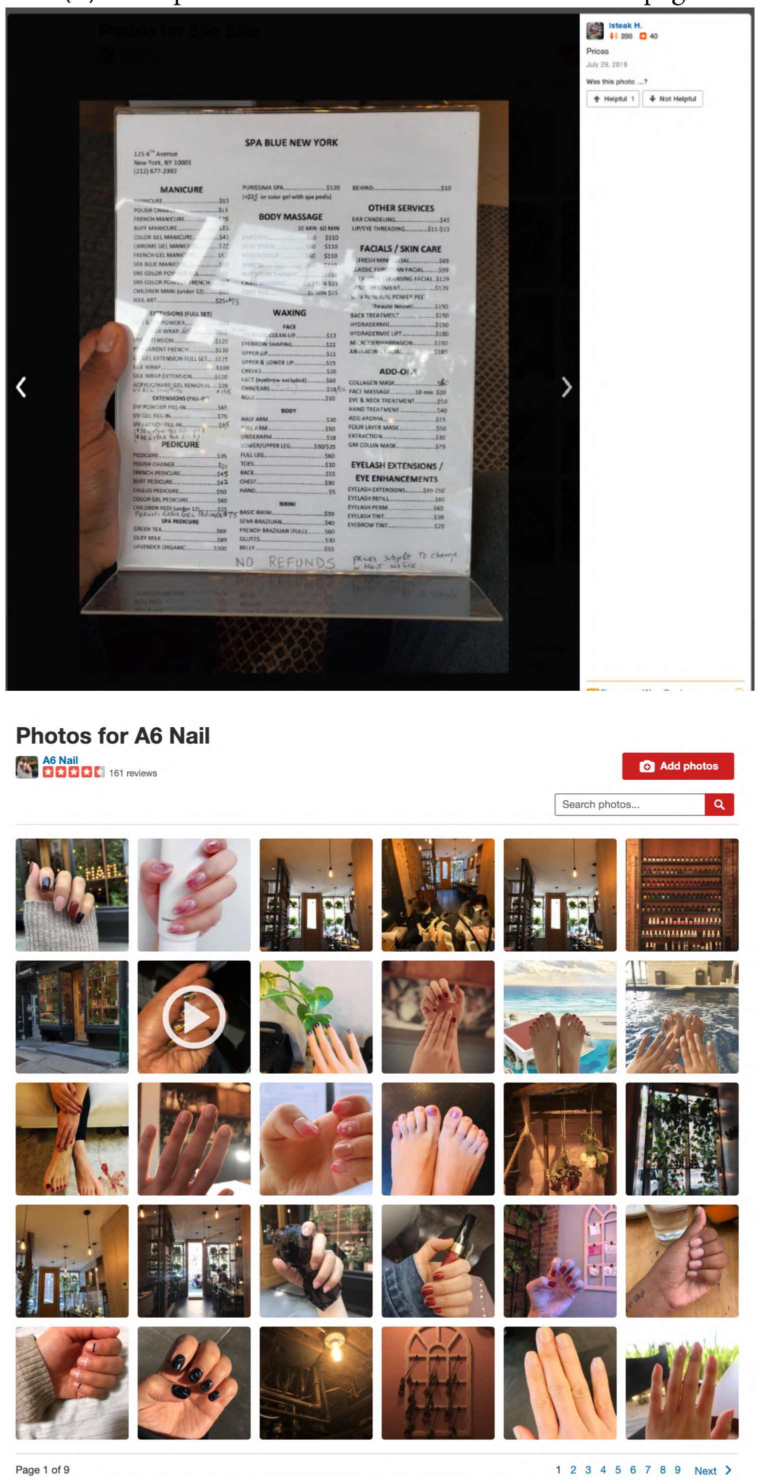
Review Highlights

"She also mentioned that the nail polish I had originally picked up (**Deborah Lippmann**) was an extra $3." in 3 reviews

"The prices at this shop are a little cheaper than their other **park slope location** (also lovely)." in 3 reviews

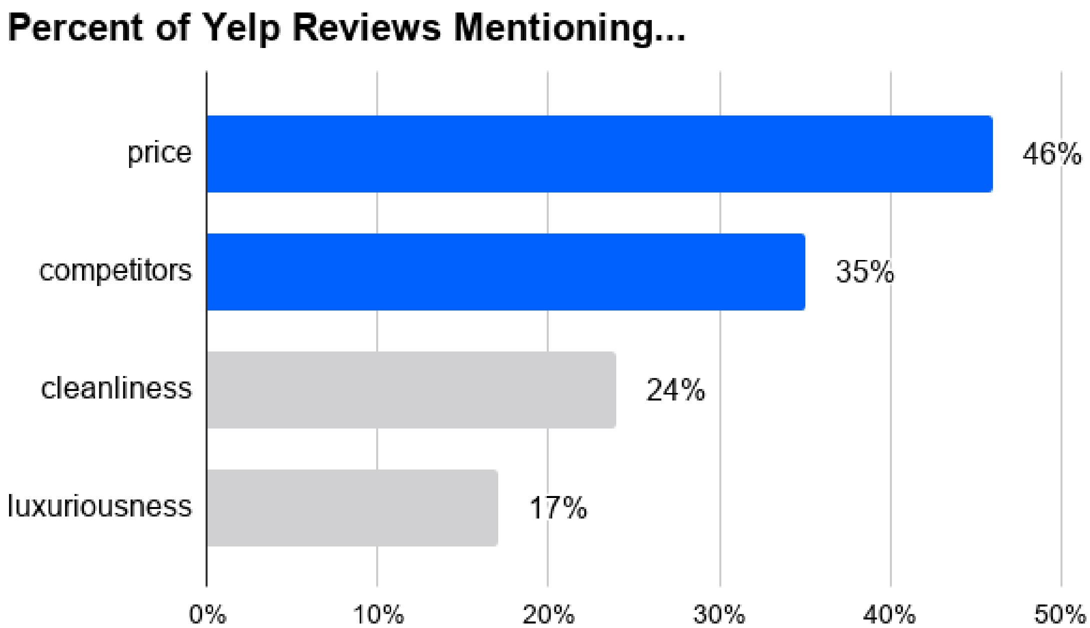
"I recently tried the **happy hour mani**/pedi special for$39." in 2 reviews

(d) Example of a Photos section on the business page



Notes: These figures display examples of business pages on Yelp. Figure (a) shows the top of a sample business page, of a nail salon in New York (screenshot taken in April 2020). Figure (b) displays a specific section of the business page that shows questions and answers about the business that often highlight specific services and prices. Figure (c) shows another section of the business page that highlights certain reviews, often highlighting prices. Figure (d) shows examples of photos uploaded by consumers to the business page, showing (1) the menu of services and prices of the business, and (2) examples of service quality, including the decor and interior of the salon, as well as nail polish brands used.

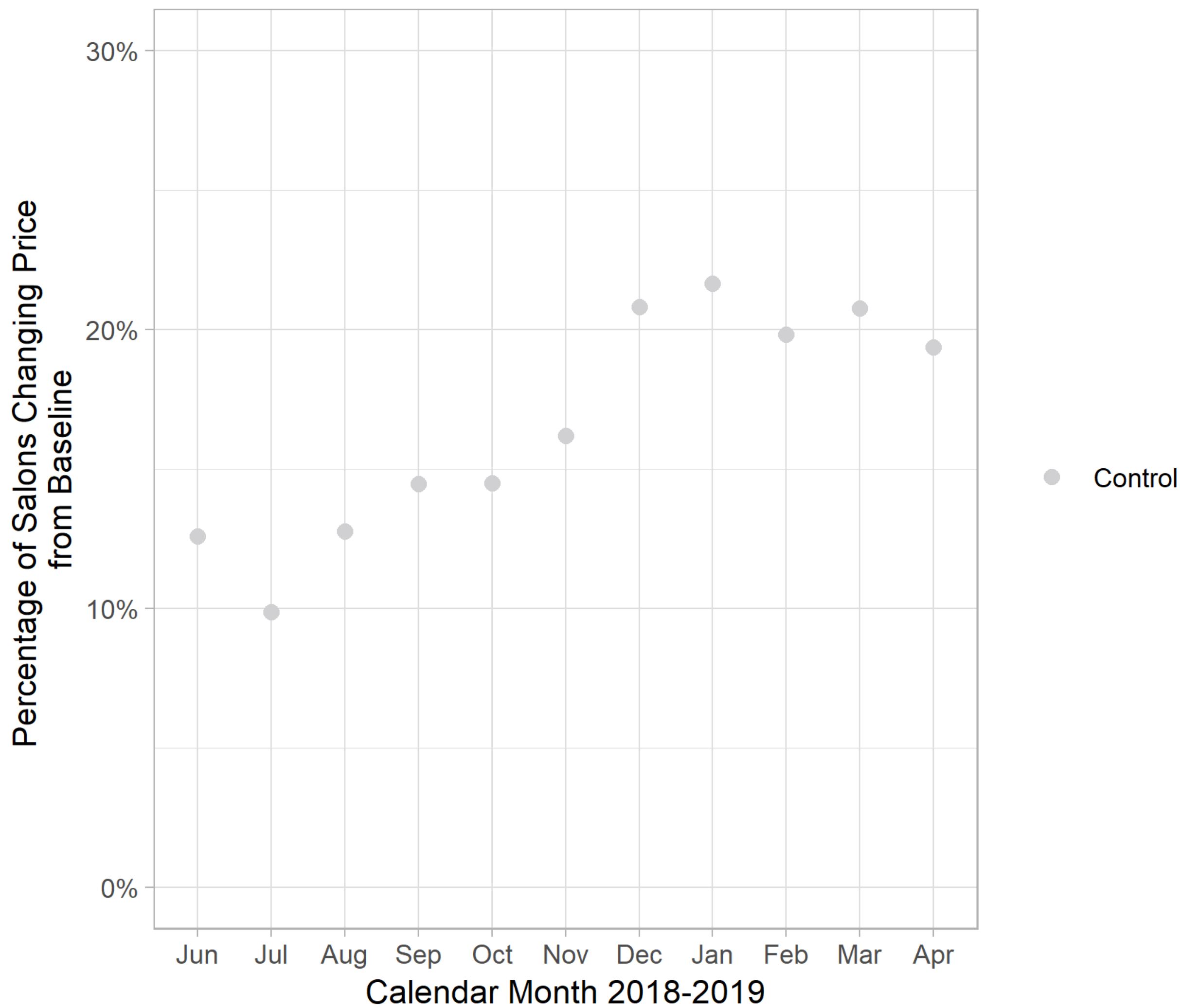
Figure F.4: Review text analysis on Yelp prior to the experiment



Notes: This figure displays a breakdown of topics mentioned in Yelp reviews across all nail salons in the sample prior to running the experiment. A model called word2vec was used to identify topic categories, which uses a neural network to learn word associations from text. All reviews were combined as a string and subsequently tokenized using NLTK (Natural Language Toolkit in python), stop words were removed, and words lemmatized. word2vec was used to create a model with words with a minimum frequency of 50000, a dimensionality of word vectors of 300, a window of 4, a learning rate alpha of 0.03, based on a skip-gram training algorithm. Lastly, the '.wv.most_similar' function was run on the seed words to identify the most similar words to a set of seed words within the reviews data. The resulting output was reviewed by a research assistant to cull any words that did not fit into the category. The seed words used for the categories were as follows, where words in brackets were jointly applied to the function. Price: price, tip, expensive, pay, affordable, charge, money, card, cash, (price, tip, pay, expensive, charge). Competition: place, different, business, back, (competition, place, other, than, back, different). Cleanliness: dirty, sterilization, sterilized, clean, cleanliness, hygiene, sanitary. Luxuriousness: atmosphere, decor, music, relax(ing), luxurious(ness), extra(s), (iced)/(bottle of) water, vibe, modern, deluxe.

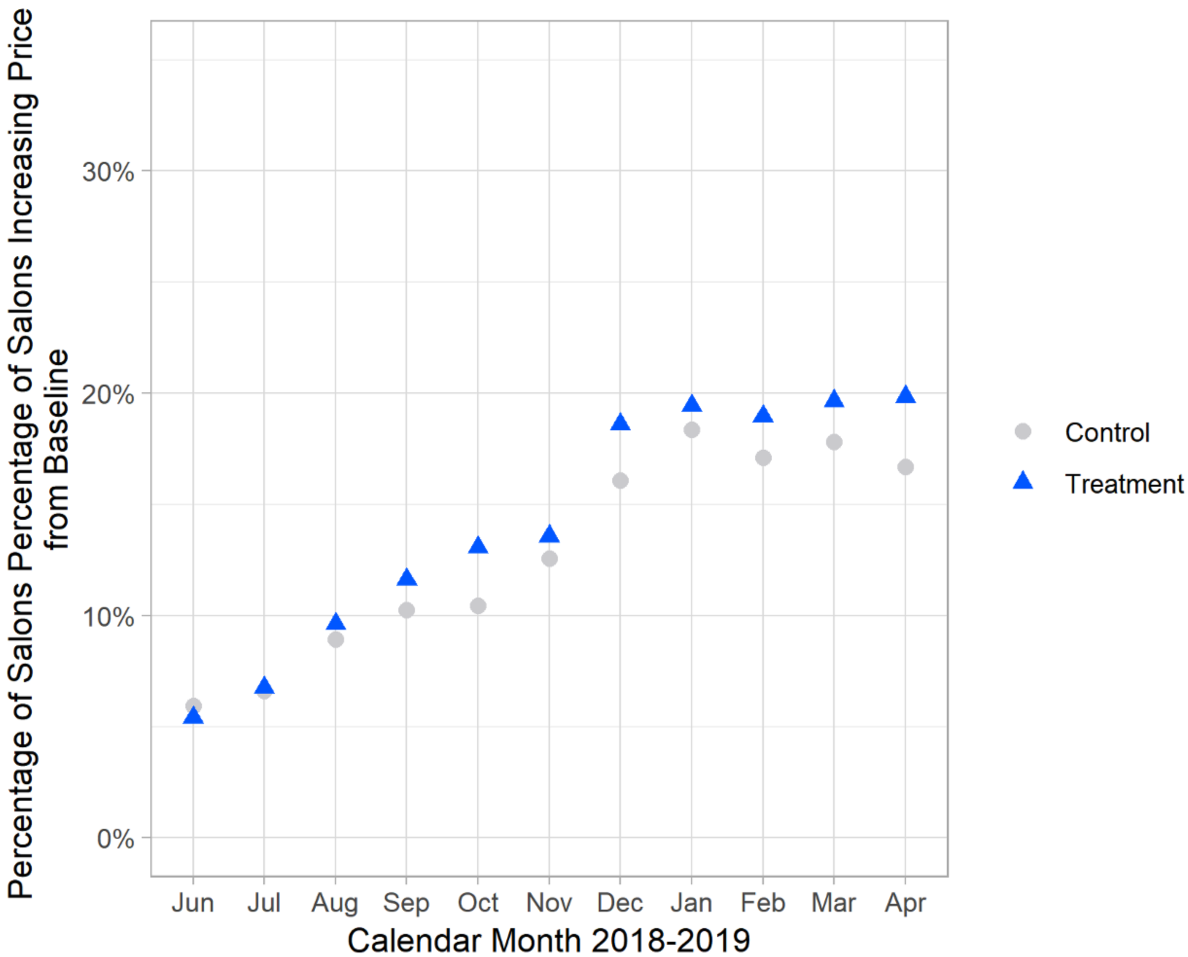
G Timing of price changes

Figure G.1: Seasonality in price changes



Notes: This figure plots the percentage of control firms with a different regular manicure price from their baseline price by calendar month. Firms appear to display seasonality in when they change prices, using more promotions in slower months (fall and winter) and changing menu prices at the end of the year. These patterns are consistent with those documented in industry magazines and confirmed by salon managers and owners.

Figure G.2: Treatment effects across calendar months

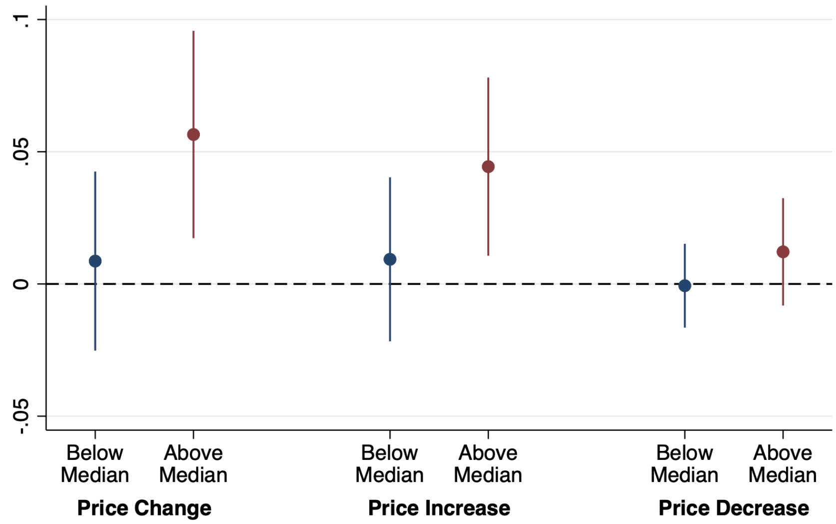


Notes: This figure plots the percentage of control and treatment firms with a different regular manicure price from their baseline price by calendar month. Both firms assigned to control and treatment are more likely to change their prices in December (between December 15 and January 15 given the data collection cycle).

H Heterogeneous treatment effects on price change

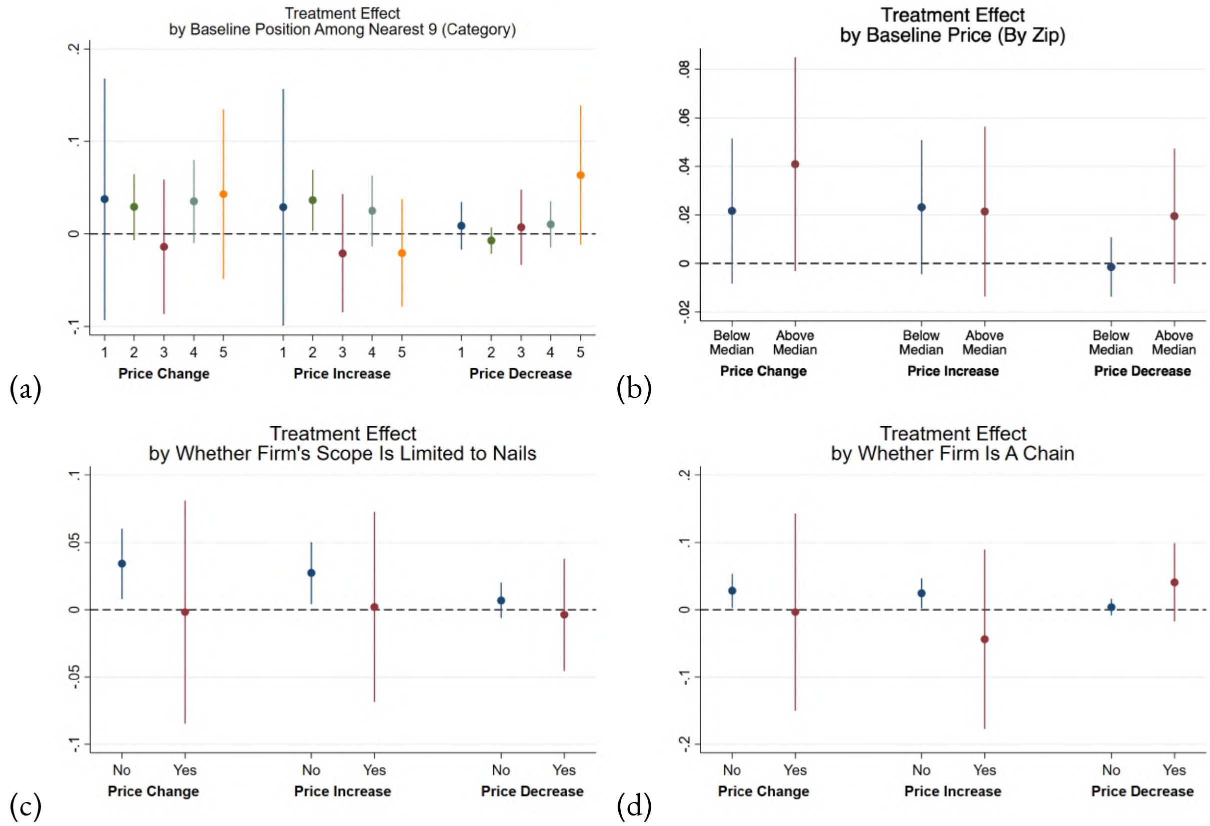
This appendix provides additional exploratory analyses on heterogeneous treatment effects, and reports the regression results in table form for the main dimensions in the paper.

Figure H.1: Price change by baseline misalignment



Notes: This figure shows estimates of treatment effects on price change, increase, and decrease by subsamples based on firms' baseline degree of misalignment in pricing and quality (measured by the absolute error from the best-fit line regressing baseline price on quality and ZIP code fixed effects). Observations are at the firm-month level, and all regressions control for any pre-visit 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.

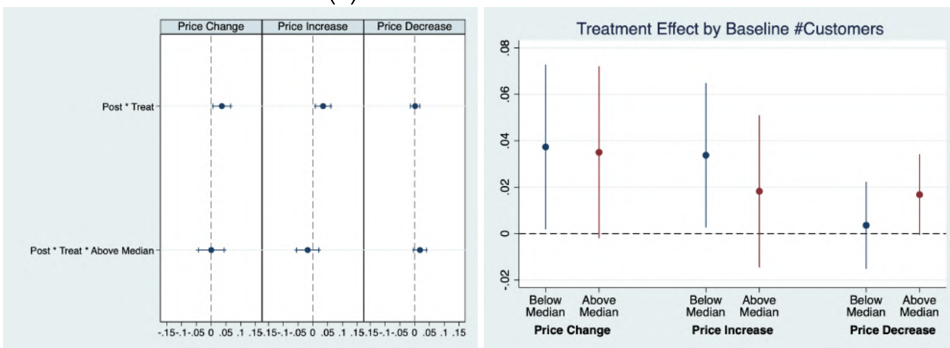
Figure H.2: Treatment effects across subsamples



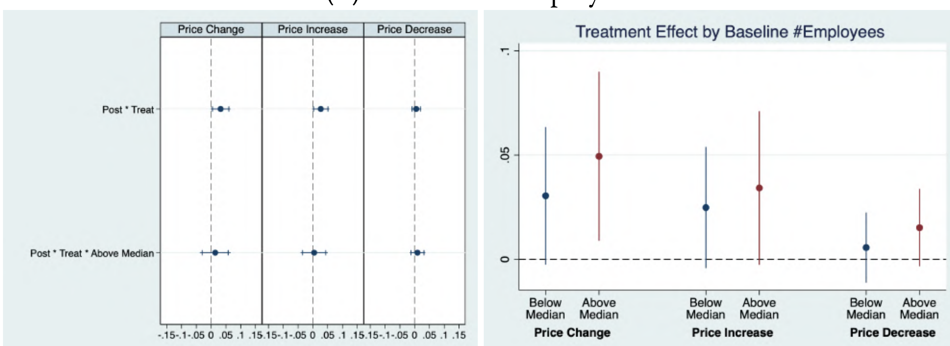
Notes: These figures plot estimates of treatment effects on price change, increase, and decrease, respectively (with 95% confidence intervals), by subsamples. Figure (a) examines subsamples by summary descriptions shown at the top of the firm's postcard, which were algorithmically generated. 1 represents "You charge the lowest price in the area," 2 represents "Most businesses nearby charger higher prices than you," 3 represents "Most/All businesses nearby charge the same prices as you," 4 represents "Most businesses nearby charge lower prices than you," and 5 represents "You charge the highest price in the area." For all regressions, observations are at the firm-month level, and control for any pre-visit 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.

Figure H.3: Heterogeneous treatment effects by baseline performance and age

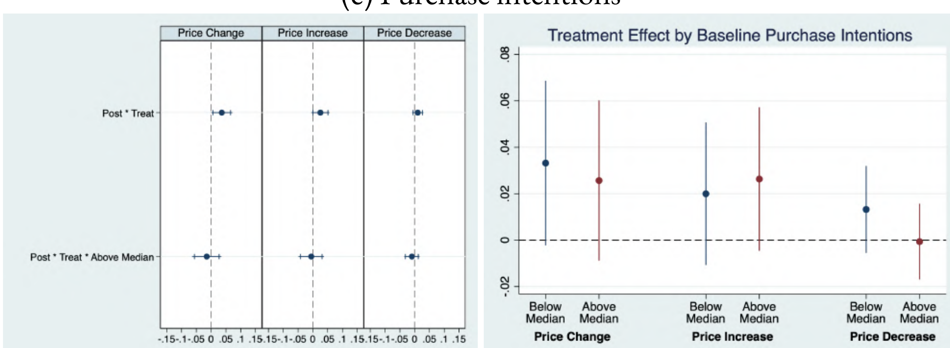
(a) Number of customers



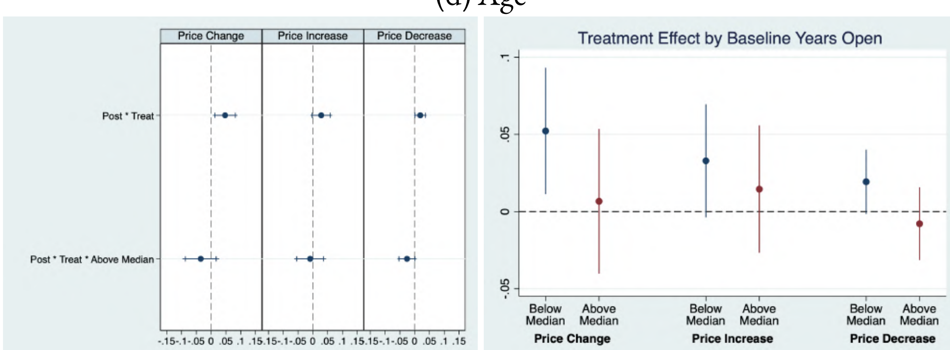
(b) Number of employees



(c) Purchase intentions



(d) Age



Notes: The figures on the left plot estimates of heterogeneous treatment effects on price change, increase, and decrease, respectively (with 95% confidence intervals), by interacting the Post * Treat indicator with the baseline attribute. The coefficient on Post * Treat identifies the effect of treatment, and the coefficient on Post * Treat * Above Median identifies the differential effect of treatment for firms with above-median baseline performance or age. The figures on the right plot treatment effect estimates by subgroups. For all regressions, observations are at the firm-month level, and control for any pre-visit 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 H.1: Price change by baseline price position from nearest competitor

Panel A: By subgroups

	Price Change			Price Increase			Price Decrease		
	(1) Lower	(2) Same	(3) Higher	(4) Lower	(5) Same	(6) Higher	(7) Lower	(8) Same	(9) Higher
Post * Treat	0.056**	-0.002	0.022	0.058***	0.004	-0.003	-0.001	-0.006	0.025*
	(0.022)	(0.023)	(0.021)	(0.021)	(0.021)	(0.016)	(0.008)	(0.009)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11118	7302	11132	11118	7302	11132	11118	7302	11132
Mean (control)	0.177	0.159	0.178	0.158	0.141	0.114	0.019	0.017	0.064
SD (control)	0.382	0.365	0.382	0.365	0.348	0.318	0.136	0.130	0.245

Panel B: By triple interaction

	(1) Price Change	(2) Price Increase	(3) Price Decrease
Post * Treat	-0.005	0.000	-0.005
	(0.022)	(0.021)	(0.009)
Post * Treat * Lower	0.066**	0.063**	0.003
	(0.031)	(0.029)	(0.012)
Post * Treat * Higher	0.027	-0.001	0.028*
	(0.031)	(0.027)	(0.016)
Visit Week FE	Yes	Yes	Yes
Observations	30142	30142	30142
Mean (control - smaller than competitor)	0.177	0.158	0.019
Mean (control - same as competitor)	0.159	0.141	0.017
Mean (control - greater than competitor)	0.178	0.114	0.064

Notes: Panel A shows treatment effect estimates by subsamples based on firms' baseline price positioning compared to their nearest competitor (whether the firm charged lower, same, or higher prices compared to its nearest competitor). The dependent variable for columns (1)-(3) is price change, a binary indicator of whether the firm's regular manicure price in a given month is different from its baseline price. The dependent variable for columns (4)-(6) is price increase, and for columns (7)-(9) is price decrease – which are binary indicators of whether the firm's regular manicure price in a given month is higher or lower than its baseline price. Panel B shows treatment effect estimates by triple interaction (where Post*Treat indicates the estimate for firms that charged the same price as the nearest competitor at baseline). For both panels, observations are at the firm-month level. All regressions control for any pre-visit differences between control and treatment groups, an indicator for months post-canvasser visits, and fixed effects for the week of the canvasser visit. The last rows show the means of the dependent variable for control firms across post-canvasser visit months. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table H.2: Price change by baseline misalignment

Panel A: By subgroups

	Price Change		Price Increase		Price Decrease	
	(1) Low Misalign	(2) High Misalign	(3) Low Misalign	(4) High Misalign	(5) Low Misalign	(6) High Misalign
Post * Treat	0.009	0.057***	0.009	0.044***	-0.001	0.012
	(0.017)	(0.020)	(0.016)	(0.017)	(0.008)	(0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14015	13663	14015	13663	14015	13663
Mean (control)	0.157	0.181	0.131	0.137	0.027	0.043
SD (control)	0.364	0.385	0.337	0.344	0.161	0.203

Panel B: By triple interaction

	(1) Price Change	(2) Price Increase	(3) Price Decrease
Post x Treat	-0.005	0.006	-0.011
	(0.018)	(0.016)	(0.009)
Post x Treat x Misalignment	0.081***	0.045*	0.036***
	(0.026)	(0.023)	(0.014)
Visit Week FE	Yes	Yes	Yes
Observations	27678	27678	27678

Notes: Panel A shows treatment effect estimates by subsamples based on firms' baseline misalignment in pricing and quality (measured by the absolute error from the best-fit line regressing baseline price on quality and ZIP code fixed effects). The dependent variable for columns (1)-(2) is price change, a binary indicator of whether the firm's regular manicure price in a given month is different from its baseline price. The dependent variable for columns (3)-(4) is price increase, and for columns (5)-(6) is price decrease – which are binary indicators of whether the firm's regular manicure price in a given month is higher or lower than its baseline price. The last two rows show the mean and standard deviation of the dependent variable for control firms across post-canvasser visit months. Panel B shows treatment effect estimates by triple interaction, where Post * Treat indicates the estimate for firms with below-median misalignment at baseline, and Post * Treat * Misalignment indicates the estimate for firms with above-median misalignment at baseline. For both panels, observations are at the firm-month level. All regressions control for any pre-visit 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. * p<0.10, ** p<0.05, *** p<0.01.

Table H.3: Price change across control and treatment firms by distance from nearest competitor

	Price Change	
	(1) Below Median Distance	(2) Above Median Distance
Post * Treat	0.045**	0.011
	(0.018)	(0.018)
Controls	Yes	Yes
Visit Week FE	Yes	Yes
Observations	15050	15092
Mean (control in months after visit)	0.172	0.174
SD (control in months after visit)	0.377	0.379

Notes: This table shows treatment effect estimates by subsamples based on firms' distance from their nearest competitor as a proxy of the level of competition it faces (below median distance represents higher levels of competition). Observations are at the firm-month level. The dependent variable is price change, a binary indicator of whether the firm's regular manicure price in a given month is different from its baseline price. All regressions control for any pre-visit differences between control and treatment groups, an indicator for months post-canvasser visits, and fixed effects for the week of the canvasser visit. The last two rows show the mean and standard deviation of the dependent variable for control firms in the subsample across post-canvasser visit months. Standard errors are clustered at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

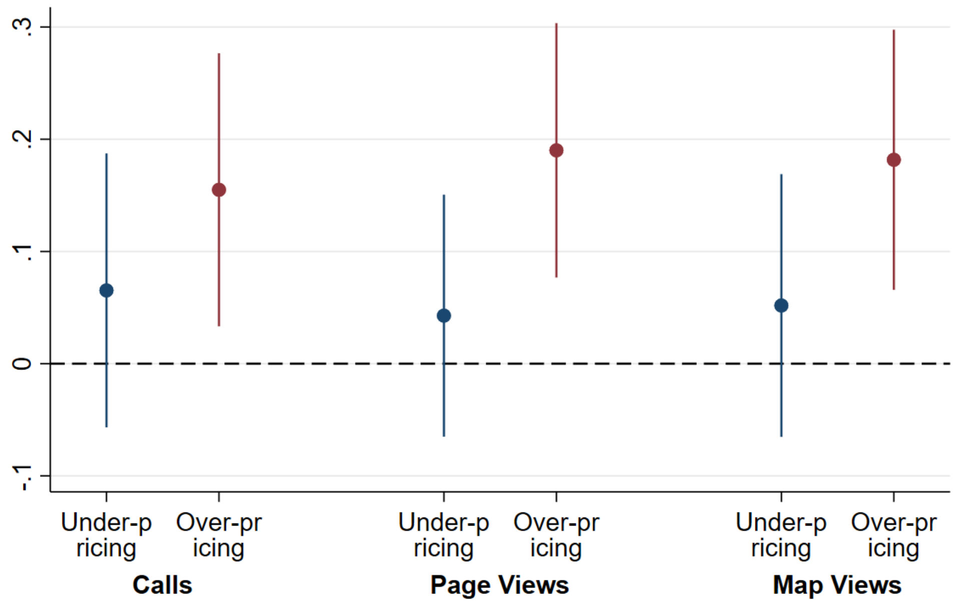
Table H.4: Price change across control and treatment firms by baseline use of promotions

	Price Change from Baseline	
	(1) No Promotions	(2) Used Promotions
Post * Treat	0.032**	-0.002
	(0.013)	(0.045)
Controls	Yes	Yes
Visit Week FE	Yes	Yes
Observations	27010	3132
Mean (control in months after visit)	0.170	0.191
SD (control in months after visit)	0.376	0.394

Notes: This table shows treatment effect estimates by subsamples based on firms' baseline use of demand-based promotions as a proxy of their pricing capabilities. Observations are at the firm-month level. The dependent variable is price change, a binary indicator of whether the firm's regular manicure price in a given month is different from its baseline price. All regressions control for any pre-visit differences between control and treatment groups, an indicator for months post-canvasser visits, and fixed effects for the week of the canvasser visit. The last two rows show the mean and standard deviation of the dependent variable for control firms in the subsample across post-canvasser visit months. Standard errors are clustered at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

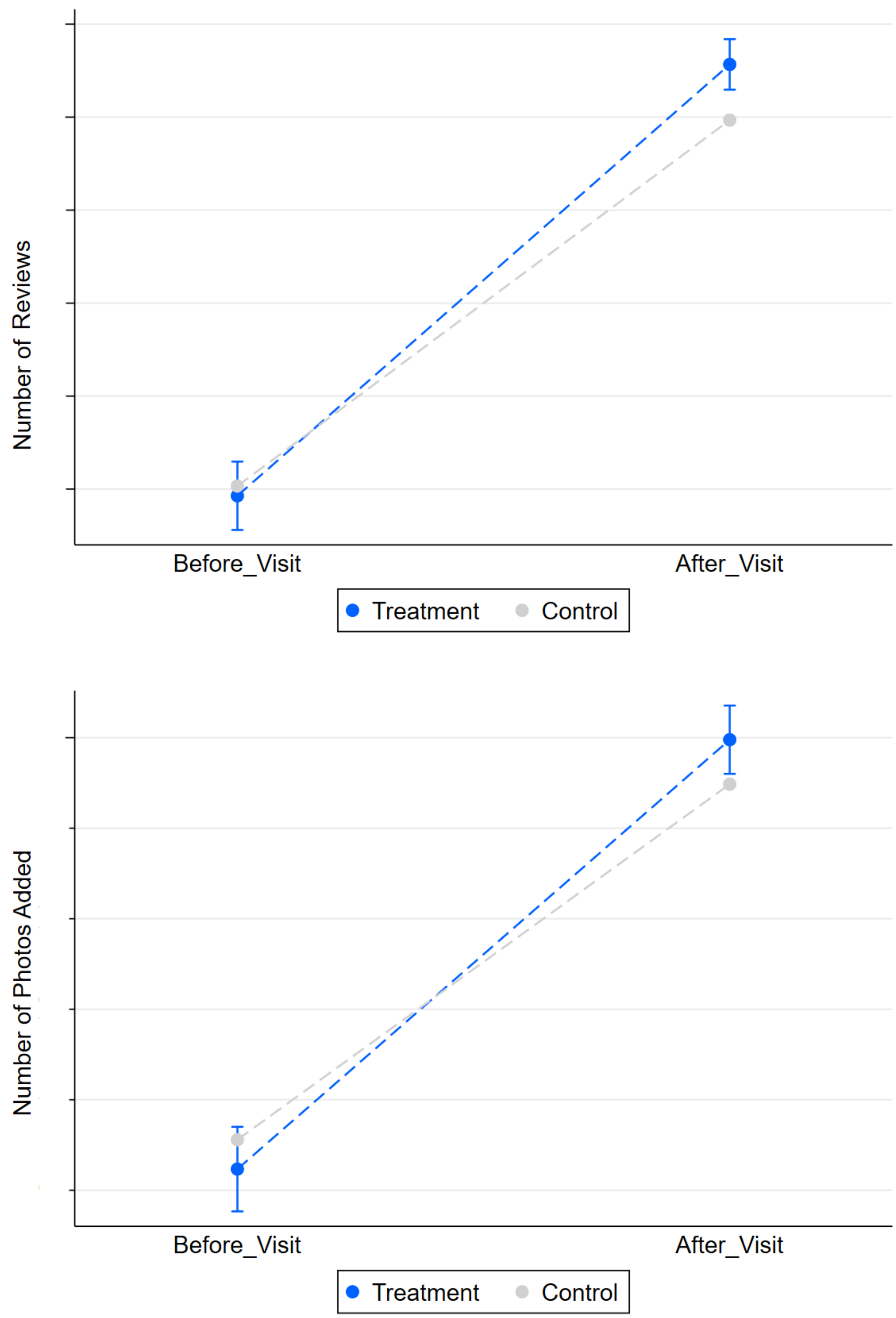
I Treatment effects on proxies of performance

Figure I.1: Performance effect by baseline over- or under-pricing



Notes: This figure plots estimates of treatment effects on logged calls, page views, and map directions views, respectively (with 95% confidence intervals), by subgroups of whether the firm was under- or over-pricing at baseline. Under- or over-pricing was coded based on whether the firm was above or below the average price for its quality level.

Figure I.2: The number of Yelp reviews and photos across control and treatment firms



Notes: These figures plot (the natural log of) the number of reviews and photos uploaded to Yelp by users by experimental condition. They show that treatment firms see 6.6% more customer reviews and 5.9% more photos uploaded to Yelp by endline compared to control firms, respectively. Raw numbers are redacted due to the data sharing agreement.

Table I.1: Estimated Revenue Across Control and Treatment Firms

	(1) ln(Revenue Calls)	(2) ln(Revenue Pageviews)	(3) ln(Revenue Map Views)
Post * Treat	0.191^{***}	0.162^{***}	0.182^{***}
	(0.070)	(0.046)	(0.068)
Controls	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes
Observations	30142	30142	30142

Notes: This table shows ITT estimates on estimated revenues based on Yelp purchase intentions (as a form of back-of-the-envelope calculations), in order to explore the concern that firms may observe lower revenues even with higher purchase intentions, especially if they are decreasing prices. As dependent variables, I construct proxies of revenues using the price that firms charge each month and the number of purchase intentions (calls, pageviews, or map direction views) observed. Interpreting these measures as revenues requires the assumption that (1) each purchase intention is independent and leads to a sale–which likely overestimates the effect, and (2) that every customer purchases a regular manicure and not any other services–which likely underestimates the effect. Therefore, these estimates are useful as a directional test rather than to evaluate the magnitude of effects. 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 for the week of the canvasser visit. Standard errors are clustered at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Another back-of-the-envelope calculation relying on prior studies' estimates of correlations between purchase intentions and revenues (Dai et al 2021 who use revenue data from the Washington State Department of Revenue find that a 10% increase in quarterly page views is correlated with a 3.3% increase in quarterly revenue) suggests that treatment firms observe 4.8% higher revenues compared to control firms from pageviews.

Table I.2: Performance across control and treatment firms by baseline price position from nearest competitor

	ln(Calls)			ln(Pageviews)			ln(Map Directions Views)		
	(1) Lower	(2) Same	(3) Higher	(4) Lower	(5) Same	(6) Higher	(7) Lower	(8) Same	(9) Higher
Post * Treat	0.154**	-0.016	0.113*	0.112**	0.079	0.175***	0.163***	-0.012	0.151***
	(0.061)	(0.065)	(0.059)	(0.047)	(0.053)	(0.052)	(0.058)	(0.063)	(0.057)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12859	8437	13266	12859	8437	13266	12859	8437	13266

Note: This table shows estimates of treatment effects on performance by subsamples based on firms' baseline relative price positioning compared to their nearest competitor (whether the firm charged lower, same, or higher prices compared to its nearest competitor). Observations are at the firm-month level. All regressions control for any pre-visit 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table I.3: Performance across control and treatment firms by baseline misalignment

	ln(Calls)		ln(Pageviews)		ln(Map Directions Views)	
	(1) Low Misalign	(2) High Misalign	(3) Low Misalign	(4) High Misalign	(5) Low Misalign	(6) High Misalign
Post * Treat	0.155^{**}	0.085	0.174^{***}	0.086	0.136^{**}	0.109^{*}
	(0.061)	(0.064)	(0.054)	(0.060)	(0.058)	(0.062)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15972	16005	15972	16005	15972	16005

Note: This table shows estimates of treatment effects on performance by subsamples based on firms' baseline alignment in pricing and quality (measured by the absolute error from the best-fit line regressing baseline price on quality and ZIP code fixed effects). Observations are at the firm-month level. All regressions control for any pre-visit 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table I.4: Performance across control and treatment firms by baseline distance from nearest competitor

	ln(Calls)		ln(Pageviews)		ln(Map Directions Views)	
	(1) Below Median	(2) Above Median	(3) Below Median	(4) Above Median	(5) Below Median	(6) Above Median
Post * Treat	0.235***	0.055	0.225***	0.067	0.239***	0.046
	(0.061)	(0.057)	(0.056)	(0.051)	(0.059)	(0.054)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17699	17699	17699	17699	17699	17699

Note: This table shows estimates of treatment effects on performance by subsamples based on firms' distance from their nearest competitor as a proxy of the level of competition it faced. "Below median" distance represents higher levels of competition, and "Above median" distance represents lower levels of competition. Observations are at the firm-month level. All regressions control for any pre-visit 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

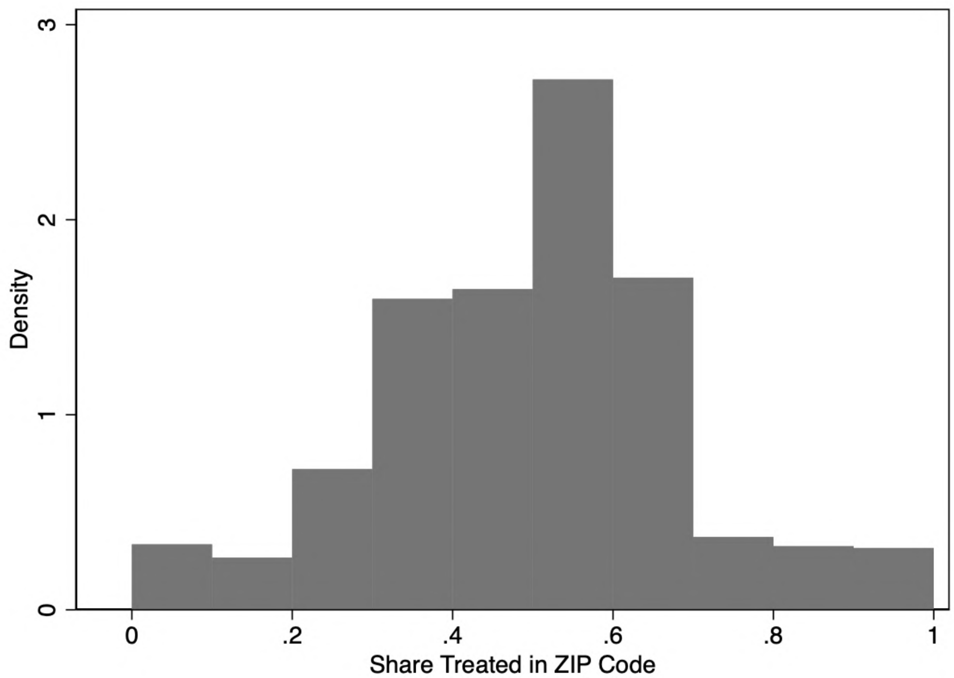
Table I.5: Performance across control and treatment firms by baseline use of promotions

	ln(Calls)		ln(Pageviews)		ln(Map Directions Views)	
	(1) No Promotions	(2) Used Promotions	(3) No Promotions	(4) Used Promotions	(5) No Promotions	(6) Used Promotions
Post * Treat	0.170***	-0.147	0.179***	-0.209**	0.168***	-0.129
	(0.044)	(0.111)	(0.041)	(0.090)	(0.042)	(0.107)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31812	3586	31812	3586	31812	3586

Note: This table shows estimates of treatment effects on performance by subsamples based on firms' baseline use of demand-based promotions as a proxy of their pricing capabilities. Observations are at the firm-month level. All regressions control for any pre-visit 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

J Spillover effects

Figure J.1: Variation in share treated across markets



Notes: This figure plots a histogram of the share of treated firms within each ZIP code.

Table J.1: Control salons' awareness of treatment

Response Type	Count	Percent
No	1385	70.56
Yes heard from another salon	28	1.43
Yes heard something about postcards	72	3.67
Closed or could not reach	478	24.35
Observations	1963	100.00

Notes: This table shows the breakdown of answers by control firms when asked by data collectors after endline (September 2019) if they heard anything about Yelp providing information on salon prices the previous summer. "Yes heard from another salon" include responses where the control salon stated that they had heard about informational postcards from Yelp from another salon, even if they had not seen the information. "Yes heard something about postcards" includes all responses where the control salon stated that it sounded familiar but were not aware of what they were. "Closed or could not reach" include businesses that were closed, or could not be reached for a conversation.

Table J.2: Price change across control firms by the share of treated firms in ZIP code

	Price Change from Baseline	
	(1) Continuous	(2) Binary
Post * Share Treated	-0.004	
	(0.050)	
Post * Above Median Share Treated		-0.009
		(0.018)
Visit Week FE	Yes	Yes
Observations	15394	15394

Notes: All regressions are run across control firms only, and estimates whether the likelihood of price change from baseline differs depending on the share of treated firms in its ZIP code. Model (1) explores this using a continuous variable of the share of treated firms ("Share Treated"), while Model (2) constructs a binary variable indicating whether the share of treated firms is above or below the median ("Above Median Share Treated"). Post is a binary indicator that equals 1 for firms starting the month they are visited by a Yelp canvasser until the end of the study and 0 otherwise. All regressions include the full set of interaction terms between Post and Share Treated / Above Median Share Treated, and cluster standard errors at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table J.3: Performance across control firms by the share of treated firms in ZIP code

	Number of Calls		Number of Page views		Number of Map Directions Views	
	(1)	(2)	(3)	(4)	(5)	(6)
Post * Share Treated	0.229		-0.051		0.106	
	(0.182)		(0.162)		(0.170)	
Post * Above Median Share Treated		0.057		-0.002		0.024
		(0.071)		(0.065)		(0.066)
Visit Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18040	18040	18040	18040	18040	18040

All regressions are run across control firms only, and estimates how proxies of firm performance vary depending on the share of treated firms in its ZIP code. Columns (1), (3), and (5) explore this using a continuous variable of the share of treated firms ("Share Treated"), while Columns (2), (4), and (6) construct a binary variable indicating whether the share of treated firms is above or below the median ("Above Median Percent Treated"). Post is a binary indicator that equals 1 for firms in either control or treatment starting the month they are visited by a Yelp canvasser until the end of the study and 0 otherwise. All regressions include the full set of interaction terms between Post and Share Treated / Above Median Share Treated, and cluster standard errors at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

K Pre-registration differences

This study was pre-registered in the AEA Randomized Controlled Trials registry with a pre-analysis plan. The key differences between the paper and the pre-registration are:

- The pre-analysis plan specifies the same econometric specification as the paper, but without canvasser visit week fixed effects. I observed that the timing of canvassing visits were delayed among treatment firms by 1.4 weeks, and thus controlled for this in all specifications in the paper.
  - The pre-analysis plan additionally specifies exploring robustness across a model that adds ZIP code fixed effects. I did not run any specifications with ZIP code fixed effects, because I realized that this substantially reduced the sample and also introduced challenges in interpreting treatment effects due to differential percentages of treated businesses across ZIP codes. Instead, I explored robustness across a model that added randomization strata fixed effects.
- The pre-analysis plan describes all possible primary and secondary outcomes, some of which I noted at the time as potentially not being available due to partner and budget constraints. I was indeed not able to obtain some of the outcomes. I also report effects on three outcomes that were not available at pre-registration, do not report effects on two of the outcomes, and transform one of the pre-registered variables:
  - I pre-specified a sales outcome measuring the annual taxable gross receipts for the business as recorded in city tax records. I had discussed access to this data with one of the city governments prior to the experiment, but this did not materialize due to challenges with the COVID-19 pandemic in summer 2020 when this data was planned to become available.
  - I obtained additional variables to measure business engagement with the Yelp platform. I pre-registered account claim and account activity (referred to as "logins" in the paper), and additionally was able to obtain data on whether businesses purchased advertising, whether they responded to inbound consumer messages, and whether they commented on consumer reviews – which I was not aware were available and accessible at the time of pre-registration. I included these results as these variables provide more insight into how businesses interacted with the Yelp platform.
  - I pre-specified additional variables to measure changes in pricing, which I do not report in this paper: total number of price changes and size of price changes. I found that at least a quarter of the businesses use promotions and typically appear to change menu prices once (or not at all) in the experimental period, so the number of changes mostly captured noise from promotions or measurement error.
  - I take a natural log of price, as raw price was significantly right-skewed.
- I added a dimension for heterogeneity in treatment effects that was not pre-registered: baseline misalignment in pricing and quality. This misalignment in pricing and quality decisions only became apparent to me once I began analyzing the data, and appeared to be an important dimension that could provide insight into how firms changed prices.

Other than these differences, all aspects of the experimental sample, design, location, outcomes, and analyses that were described in the pre-registration were implemented in the paper without deviation.

L Endline questions and follow-up experiment details

Table L.1: Number of firms reached by condition in follow-up mechanism experiment

	(1) Ask First *# of Firms*	(2) Ask First *% of Firms*	(3) Ask Last *# of Firms*	(4) Ask Last *% of Firms*	(5) Difference *p-value*
Reached	703	71.15	702	71.27	0.95
Closed	83	8.39	71	7.19	0.32
Not Available	205	20.73	214	21.68	0.60
Observations	989	989	987	987	1976

Notes: This table shows the number of firms reached and thus included in the followup experiment.

Table L.2: Balance of baseline variables across reached firms in follow-up experiment

	Ask First Mean	Ask Last Mean	Difference	p-value
Baseline Price	13.80	13.87	-0.07	0.80
Baseline Number Of Employees	4.25	4.38	-0.13	0.42
Baseline Number Of Customers	3.65	3.95	-0.30	0.13
Baseline Total Hours Open Weekly	62.51	61.48	1.03	0.08
Baseline Cleanliness1to4	2.63	2.67	-0.04	0.38
Baseline Luxuriousness1to4	2.36	2.43	-0.07	0.09
Baseline Polish Brand Level	1.11	1.10	0.01	0.67
Baseline Number of Services (Scope)	2.17	2.06	0.11	0.12
Baseline Yelp Rating	3.88	3.89	-0.00	0.89
Baseline Number of Yelp Reviews	71.51	74.60	-3.09	0.55
Baseline Availability Next Day 4-5pm	0.76	0.75	0.00	0.89
Baseline Average Daily Opening Hour	09:44	09:44	0:00	0.85
Baseline Average Daily Closing Hour	19:15	19:12	0:03	0.40
Yelp Canvass Week	33.00	32.71	0.30	0.36

Notes: This table shows the balance of variables at baseline between firms asked first and firms asked last, across the sample of firms that were reached by data collectors.

Table L.3: Attrition in endline guesses across experimental conditions

	(1) Treatment *# of Firms*	(2) Treatment *% of Firms*	(3) Control *# of Firms*	(4) Control *% of Firms*	(5) Difference *p-value*
Closed	90	5.70	100	6.10	0.64
Did Not Answer Any Questions	18	1.14	16	0.98	0.65
Did Not Answer Question (1)	370	23.45	280	17.07	$<$0.01
Did Not Answer Question (2)	626	39.67	650	39.63	0.98
Did Not Answer Question (3)	125	7.92	57	3.48	$<$0.01
Observations	1578	1578	1640	1640	3218

Notes: This table shows attrition rates by question for endline questions: (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 to your two nearest nail salons?".

Table L.4: Correct answers on competitor prices across control and treatment firms

	(1) Competitor Name	(2) Competitor Price	(3) Relative Price to 2 Nearest Competitors
Treatment	0.032*	0.070***	-0.015
	(0.019)	(0.022)	(0.018)
Constant	0.289***	0.368***	0.393***
	(0.013)	(0.015)	(0.013)
Observations	2384	1908	2869

Notes: This table shows the share of correct answers by control and treatment firms at endline for three questions: (1) Column 1: "what salon is located closest to you?" (2) Column 2: "what do you think they are charging for a regular manicure?" (3) Column 3: "How do you think your price compares to your two nearest nail salons?". Dependent variables are binary variables indicating whether the firm's answer was correct. Observations are at the firm level, and includes all firms who were available for a conversation and answered the question. Robust standard errors are reported. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

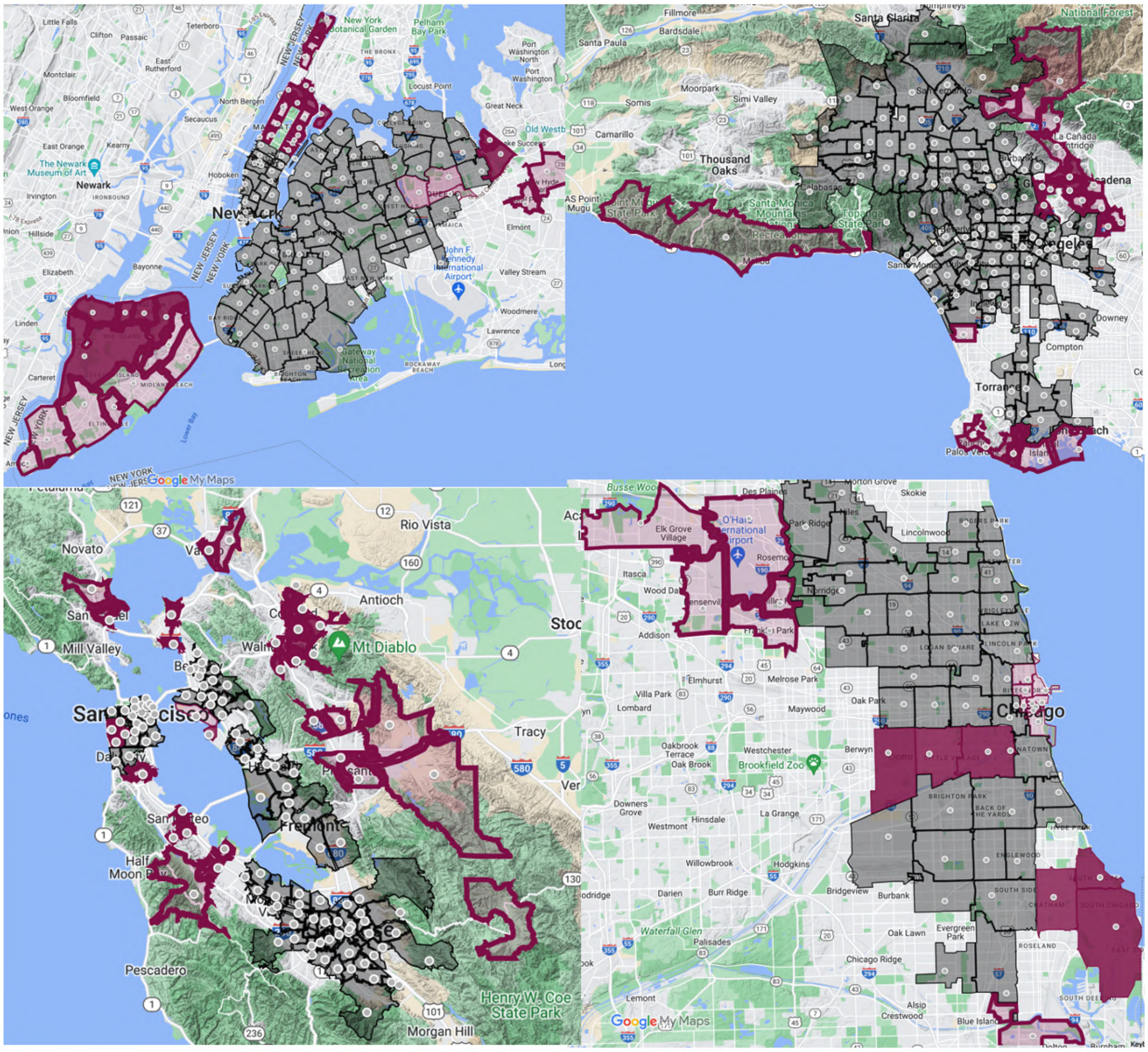
Table L.5: Heterogeneous treatment effects of asking Information Signup last

	(1) Signup	(2) Signup	(3) Signup	(4) Signup	(5) Signup
Signup Asked Last	0.015	0.023	0.040	0.041	0.041
	(0.033)	(0.035)	(0.035)	(0.041)	(0.030)
Last * Above Median # Employees	0.031				
	(0.054)				
Last * Above Median # Customers		0.010			
		(0.053)			
Last * Above Median Purchase Intentions			-0.006		
			(0.050)		
			(0.034)		
Last * Above Median Years Open				0.005	
				(0.060)	
Constant	0.239***	0.238***	0.234***	0.245***	0.226***
	(0.023)	(0.024)	(0.024)	(0.029)	(0.021)
Observations	1134	1133	1239	828	828

Notes: This table interacts the treatment indicator with baseline firm attributes.

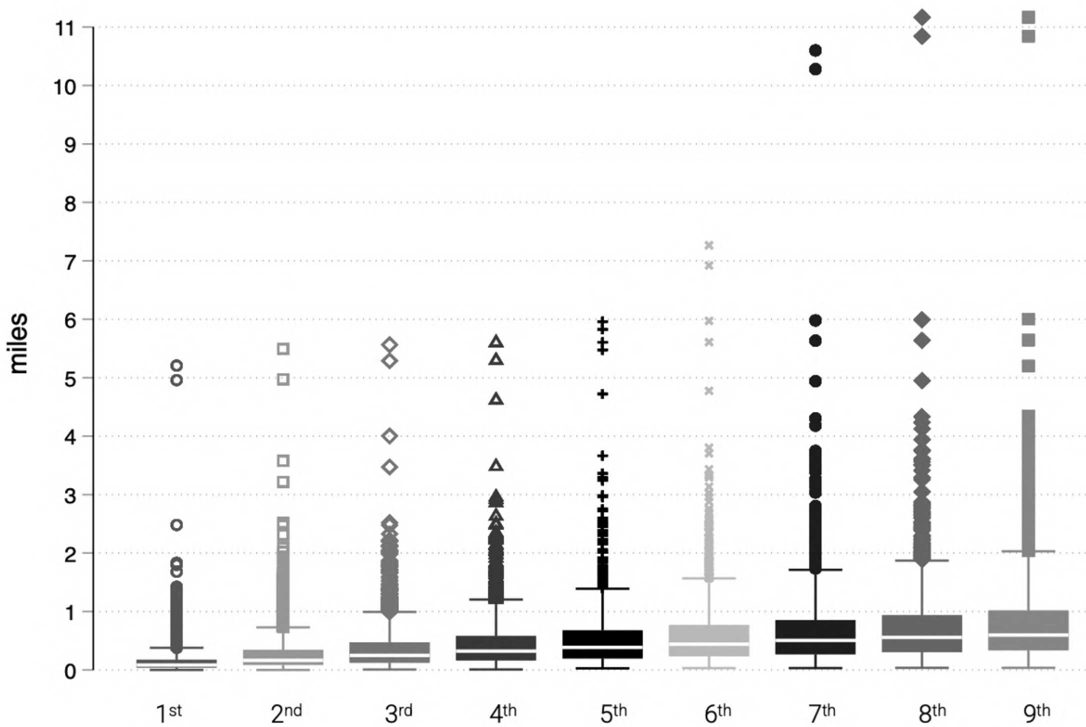
M Mechanims driving treatment effects

Figure M.1: Isolated markets at the ZIP code level



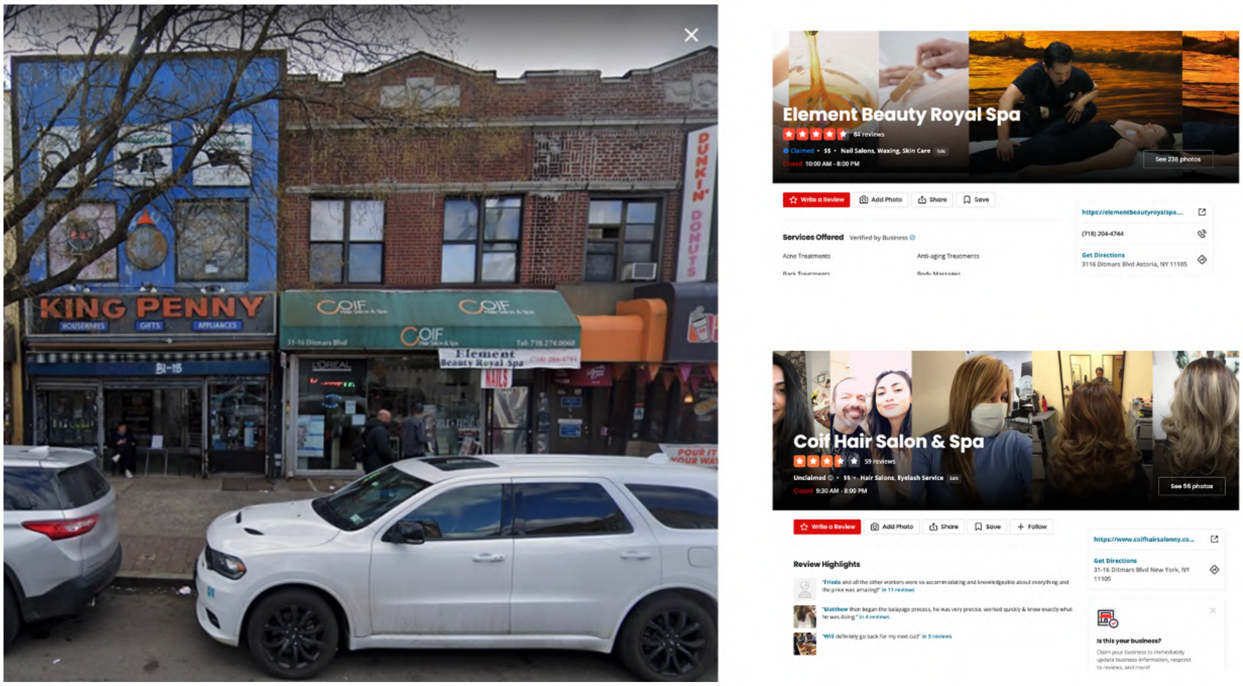
Notes: This figure plots 31 plausibly isolated markets across 118 ZIP codes that include 745 firms in the experimental sample (23%) by metropolitan area (from top left to bottom right: New York, Los Angeles, San Francisco, and Chicago). For each metropolitan area, isolated markets are marked in maroon, and all other markets in the experiment in black. The boundaries show ZIP code boundaries, and groups of ZIP codes that together make up an isolated market are demarcated by whether the area is filled in. For example, Staten Island in New York has two isolated markets, separated by a green belt of parks that split the island into two.

Figure M.2: Distribution of distance from the nearest 9 competitors



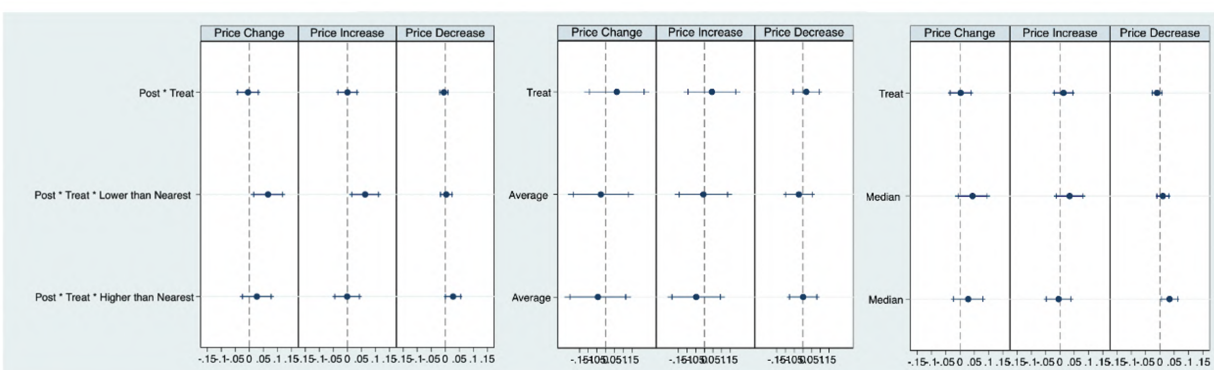
Notes: This figure plots the distribution of the distance from competitors for firms in the experimental sample, with the x-axis identifying whether the plot is for the 1st-9th nearest competitor.

Figure M.3: An example of business colocation



Notes: This figure shows an example of how close the nearest competitors can be, whose decisions may be most salient: Element Beauty Royal Spa is located upstairs from Coif Hair Salon & Spa.

Figure M.4: How firms change prices relative to their nearest, average, and median competitors



Notes: This figure compares treatment effects in terms of whether and when treated firms change their decisions relative to the nearest competitor at baseline, compared to their average or median competitor. The first panel interacts the post-treatment indicator with whether it was lower or higher than the nearest competitor; the second panel with whether it was lower or higher than the average competitor; and the third panel with whether it was lower or higher than the median competitor.

Table M.1: Treatment effect in isolated markets by baseline number of firms

	(1) Price Change	(2) Price Change	(3) Price Change	(4) Price Change
Post * Treat	0.006	0.008	0.004	-0.003
	(0.039)	(0.038)	(0.037)	(0.037)
Post * Treat * Above Median # Firms	0.040	0.034		
	(0.057)	(0.057)		
Post * Treat * Top Quartile # Firms			-0.072	-0.031
			(0.087)	(0.090)
Post * Treat * 2nd Top Quartile # Firms			0.129^{**}	0.127^{**}
			(0.062)	(0.061)
2017 Population (in 100K)	0.016	0.006	0.023	0.007
	(0.015)	(0.017)	(0.019)	(0.023)
Controls	Yes	Yes	Yes	Yes
Visit Week FE	No	Yes	No	Yes
Month FE	No	Yes	No	Yes
Observations	6790	6790	6790	6790

Notes: This table shows the treatment effect among firms located in plausibly isolated markets interacted with the number of firms at baseline as a measure of the number of competitors, holding demand (proxied by baseline population in the area) constant. Columns (3) and (4) segment the above-median firms into two quartiles.

Table M.2: Treatment effect in isolated markets by population change

	(1) Price Change	(2) Price Change	(3) Price Change	(4) Price Change
Post*Treat	0.035	0.037	0.026	0.028
	(0.038)	(0.038)	(0.036)	(0.036)
Post*Treat*Above Median Population Change	-0.016	-0.023	-0.014	-0.013
	(0.057)	(0.056)	(0.056)	(0.056)
Baseline Number of Firms	0.004	0.003	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Controls	Yes	Yes	Yes	Yes
Visit Week FE	No	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Strata FE	No	No	Yes	Yes
Observations	6842	6842	6729	6729

Notes: This table shows the treatment effect among firms located in plausibly isolated markets interacted with the degree of population change as a measure of change in demand, holding the number of competitors (proxied by the number of firms in the area) constant.

Table M.3: Price changes Across control and treatment firms by treatment month

	(1) Price Change	(2) Price Change
Post * Treat	0.078**	0.078**
	(0.032)	(0.032)
Post * Treat * Jul	-0.037	-0.037
	(0.036)	(0.036)
Post * Treat * Aug	-0.072*	-0.072*
	(0.037)	(0.037)
Post * Treat * Sep	-0.085*	-0.086*
	(0.045)	(0.045)
Post * Treat * Oct	-0.000	-0.001
	(0.054)	(0.054)
Controls	Yes	Yes
Visit Week FE	Yes	Yes
Month FE	No	Yes
Strata FE	Yes	Yes
Observations	29552	29552

Notes: This table breaks out the treatment effect by the month of treatment, which were randomly assigned across firms. Each month starts on the 15th (e.g., July spans from July 15 - August 14).

Table M.4: Percentage of firms that change prices by baseline manager responses on competitors

	Did Not Change Prices	Changed Prices	*Total Number*	*Total Percentage*
did not answer	62.32	37.68	69	100
doesn't know	61.96	38.04	602	100
no competitors	60	40	120	100
others in area	55.11	44.89	274	100
specific salon	52.27	47.73	220	100
a type of salon	47.83	52.17	23	100

This table shows the percentage of firms that changed prices by manager responses prior to treatment on who their competitors are.

Table M.5: Manager responses to competitor information treatment

Competition-related responses
Fri Aug 17 2018 - she was surprised that her salon charges the lowest price in the area. she was thinking to raise their price up to match others
Wed Aug 08 2018 - she is thinking to change their price since her salon is the cheapest.
Wed Jul 11 2018 - He did not know about his place was the cheapest price.
Wed Aug 01 2018 - she has supposed that her salon charges the cheapest price!!
Thu Aug 23 2018 - she was very surprised that her salon charged the lowest. she is thinking to raise her price up
Fri Jun 29 2018 - They want to change the price since they are so cheap [compared to] other nail salons.
Wed Jul 11 2018 - was surprised that their nail salon was the cheapest price they charge for the regular manicure.
Mon Aug 20 2018 - she realized that the competitor salon charged less than them.
Mon Aug 13 2018 - she was surprised that they charge more than other salons in this area.
Fri Oct 12 2018 - liked info and thought it was interesting she could see other competitors and what they charge.
Tue Oct 30 2018 - liked seeing where she compared to others
Wed Sep 05 2018 - manager says they could use more information on competition price
Demand-related responses
Thu Aug 23 2018 - since this area's nail salons charge very low, she can't raise her price. she knew about her competitor's price.
Mon Oct 01 2018 - left with DM, she thought they were very good price for area.
Mon Aug 27 2018 - owner doesn't believe they really have any competition but would still like info on pricing
Wed Aug 29 2018 - interested in future pricing info, feels they don't have competition

Notes: This table shows notes taken by Yelp canvassers on manager responses to the competitor information treatment that relate to competition or demand.

N Treatment effect on quality decisions

Table N.1: Change in quality across control and treatment firms

	(1) Quality Change	(2) Quality Increase	(3) Quality Decrease
Treatment	0.040**	0.024	0.017
	(0.019)	(0.017)	(0.014)
Constant	0.572***	0.343***	0.229***
	(0.030)	(0.029)	(0.026)
Visit Week FE	Yes	Yes	Yes
Observations	3218	3218	3218

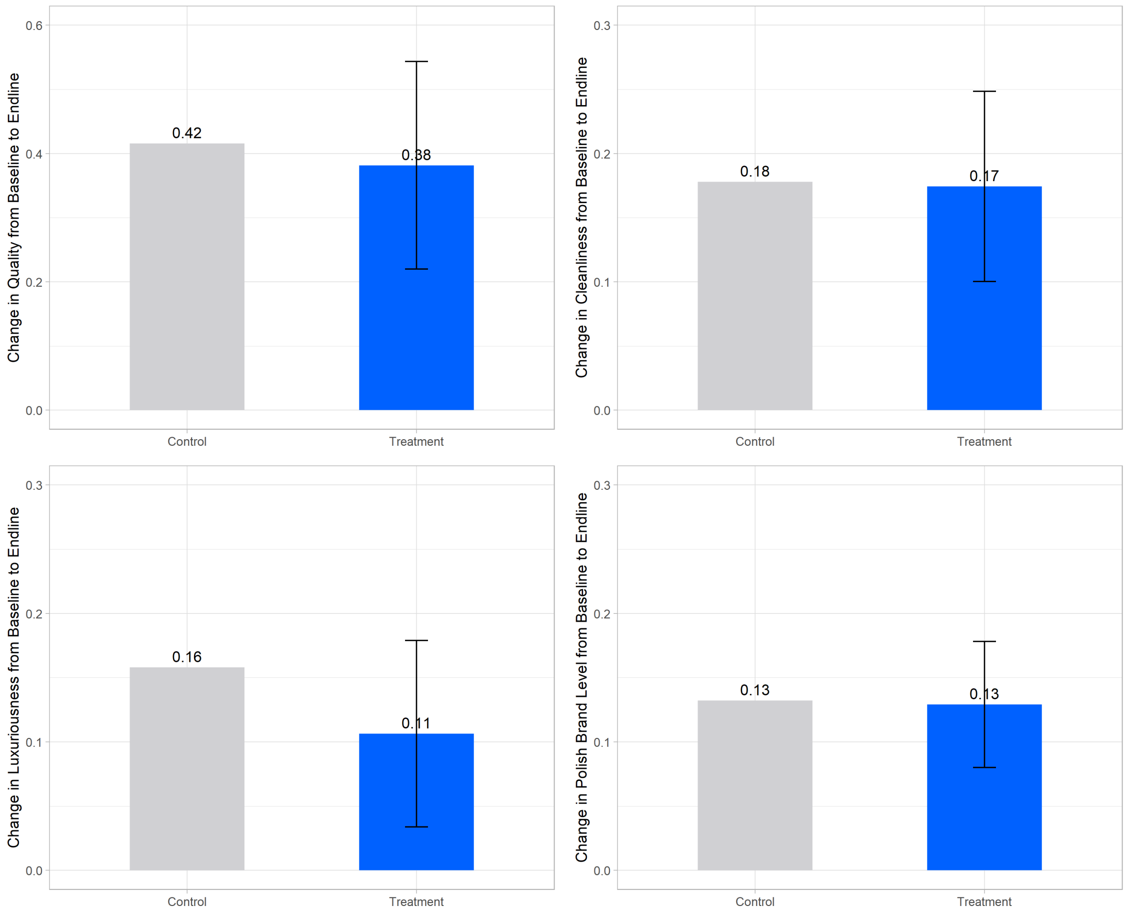
Notes: This table shows the treatment effect on binary indicators of quality change, quality increase, and quality decrease.

Table N.2: Change in quality across control and treatment firms by baseline pricing

	(1) Quality Change	(2) Quality Increase	(3) Quality Decrease
Treatment	0.063*	0.044	0.019
	(0.036)	(0.032)	(0.027)
Treat * Higher	-0.051	-0.024	-0.027
	(0.045)	(0.040)	(0.033)
Treat * Lower	-0.007	-0.028	0.021
	(0.045)	(0.041)	(0.034)
Constant	0.539***	0.313***	0.226***
	(0.037)	(0.035)	(0.030)
Visit Week FE	Yes	Yes	Yes
Observations	3218	3218	3218

Notes: This table shows heterogeneous treatment effects by baseline pricing on binary indicators of quality change, quality increase, and quality decrease.

Figure N.1: Differences in magnitude of change in quality across control and treatment firms



Notes: These figures plot average changes in quality measures between baseline and endline across control and treatment firms.

O Robustness: controlling for canvasser fixed effects

Table O.1: Price changes across control and treatment firms with canvasser fixed effects

	(1) Price Change	(2) Price Change	(3) Price Change	(4) Price Change
Post * Treat	0.028**	0.028**	0.031**	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
Canvasser FE	Yes	Yes	Yes	Yes
Observations	27056	27056	26501	26501
Mean (control in months after visit)	0.173			
SD (control in months after visit)	0.378			

Notes: This table shows ITT estimates of the competitor information treatment on a binary indicator of whether the firm's regular manicure price in a given month is different from its baseline price. 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 and the assigned canvasser. Standard errors are clustered at the firm level.

Table O.2: Directions of price change with canvasser fixed effects

	(1) Price Decrease	(2) Price Increase	(3) ln(Price)
Post * Treat	0.007	0.021*	0.002
	(0.007)	(0.011)	(0.007)
Controls	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes
Canvasser FE	Yes	Yes	Yes
Observations	27056	27056	27056
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 competitor information on a binary indicator of whether the firm's regular manicure price is lower or higher than its baseline price, (columns 1-2) and 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 for the week of the canvasser visit and the assigned canvasser. Standard errors are clustered at the firm level.

Table O.3: Platform engagement across control and treatment firms with canvasser fixed effects

	(1) ln(Login Days)	(2) Account Claimed	(3) Advertising	(4) Responses	(5) ln(Review Comments)
Post * Treat	-0.007	-0.006	0.004	0.007	0.001
	(0.023)	(0.011)	(0.005)	(0.005)	(0.006)
Controls	Yes	Yes	Yes	Yes	Yes
Visit Week FE	Yes	Yes	Yes	Yes	Yes
Canvasser FE	Yes	Yes	Yes	Yes	Yes
Observations	31812	31812	31812	31812	31812

Notes: This table shows ITT estimates of the competitor information treatment on firms' engagement with the Yelp platform. Dependent variables are the number of days a business logs in to Yelp (column 1), whether a business has claimed its page on Yelp (column 2), whether a business has purchased advertising (column 3), the number of responses the business has made to consumer questions on quotes or appointments (column 4), and the number of comments the business has made on consumer reviews (column 5). All 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 for the week of the canvasser visit and the assigned canvasser. Standard errors are clustered at the firm level.

Table O.4: The effect of reevaluating competitor knowledge on demand for information

	(1) Competitor Information Signup
Signup Asked Last	0.036*
	(0.022)
Constant	0.201***
	(0.068)
Canvasser FE	Yes
Observations	1405

Notes: This table shows results from the follow-up experiment among control firms that tested whether having managers re-evaluate their knowledge of competitors impacted their demand for free competitor information. The dependent variable is a binary variable indicating whether the firm signed up to receive free competitor information. The treatment, "Signup Asked Last," is relative to a control group where firms were first asked whether they were interested in signing up to receive competitor information, before being asked questions to re-evaluate their knowledge. Observations are at the firm level, and includes all control firms who were available for a conversation. Standard errors are clustered at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.