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Delivery in the City: Evidence on Monopolistic Competition from New York Restaurants*

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We examine the response to entry in a large market with differentiated products using a novel longitudinal dataset of over 550,000 New York City restaurant menus from 68 consecutive weeks. We compare "treated" restaurants facing a nearby entrant to "control" restaurants with no new competition, matching restaurants using location characteristics and a pairwise distance measure based on menu text. Restaurants frequently adjust prices and product offerings but we find no evidence that they respond differentially to new competition. However, restaurants in the top entry decile are 5% more likely to exit after a year than restaurants in the lowest entry decile.
JEL codes: D22, D43, L13

1 Introduction

Firms in many industries compete in markets with a large number of competitors and substantial product differentiation. To study these markets, a vast literature in trade, industrial organization, and many other fields uses models of monopolistic competition, especially the Dixit-Stiglitz constant elasticity of substitution (CES) model (1977). While recent models allow for more flexible preferences, a key feature of all models of monopolistic competition is that firms do not respond strategically to local competitors. This is in stark contrast to spatial competition models (e.g. Salop (1979)), in which firms mostly compete with only a small subset of close competitors. These two approaches, aspatial and spatial, are both commonly used and yet they imply very different answers to a fundamental question: how does a firm respond to new competition in markets with many differentiated competitors?

When firms have differentiated products they may compete for customers in multiple dimensions; a close competitor could be a firm located a few blocks away, a firm with a fairly similar product, or both. Unless researchers have very detailed product information, it can be difficult to infer which firms are likely competitors and to measure competitive responses that may be spread across many products. In this paper we use a novel panel of restaurant menus in New York City to study the responses of incumbent restaurants to competition from new entrants in both physical space and product space. We collected menus from a large online food delivery service every week for 68 consecutive weeks, giving us a panel of about 550,000 menus from 11,700 unique restaurants. This dataset allows us to precisely define the distance between competitors

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in arguably the most salient aspects of restaurant product differentiation, location and menu, and to measure competitive responses over a firm's full set of products. We are also able to assess competition along several other margins, such as quality ratings and hours of operation, and examine the effect of new firm entry on the likelihood of incumbent firm exit.

While our analysis is limited to a single industry, the restaurant industry—with many firms, substantial product differentiation, and low barriers to entry–is perhaps the canonical example of monopolistic competition[1]. This industry also provides a simple and intutive context for comparing the implications of aspatial and spatial models. If a new restaurant opens on the same block as an existing restaurant, or opens nearby with a similar menu, how does the existing restaurant respond? Do they lower prices or change their menu items, or is the market so large and competition so diffuse that they can ignore this new local competitor? Further, the restaurant industry is also one of the largest employers of minimum wage labor and therefore the competitiveness of this industry has direct implications for the effects of recent increases in the minimum wage[2].

A challenge in studying the response to entry is that firm location choice is endogenous. In our context, an entering restaurant may choose a specific site because of attractive location characteristics, or because none of the incumbent restaurants offer a similar menu. If the unobserved determinants of location choice are correlated with factors affecting the measured outcomes of the incumbents, then this introduces selection bias. For example, if new entrants tend to move into areas with rapidly increasing incomes and commercial rents, then incumbent restaurants may be raising prices independent of entry, thus biasing upwards estimates of the response to competition. A related issue is that different types of restaurants may change their menus with different frequencies, or respond differently to changes in city wide input prices; a labor shortage of sushi-chefs should not have the same effect on Japanese and Italian restaurants. If entry frequency is correlated with restaurant characteristics—and we present evidence suggesting that it is—then this may also lead to bias. Lastly, the incumbent response to entry may be a function of characteristics of both the incumbent and the entrant: the same Italian restaurant could respond differently to the entry of a new sushi restaurant versus a new Italian restaurant.

To address these issues we use a matching technique that exploits the unusual degree of product information in our dataset. We match "treated" incumbent restaurants facing competition from a new entrant with a "control" group of incumbent restaurants that have very similar menus and location characteristics, but face no changes to the competitive environment. A central challenge in implementing this matching technique is how to determine the product similarity of two restaurants from the text of their menus. We employ a text processing technique from computer science to calculate a scalar measure of the similarity of two restaurant menus, "cosine similarity," and use this as a metric for distance in product space. We compare this measure with a set of observable restaurant characteristics and find that it is a strong predictor of pairwise similarity in restaurants' product features. Using this measure and additional location characteristics, we compile a set of treatment and control observations and examine incumbent responses to entry in a number of channels and settings. We also use this measure to define treatment in terms of menu similarity, and thus an important contribution of our paper is to provide systematic evidence on spatial competition in two different dimensions.

Our results suggest that restaurants facing competition from a new entrant do *not* change their prices, products, or service differently from restaurants without new competition. The restaurant industry is notori-

[1]The Wikipedia article on monopolistic competition declares "Textbook examples of industries with market structures similar to monopolistic competition include restaurants, cereal, clothing, shoes, and service industries in large cities" (Wikipedia 2018).

[2]If firms are monopolistically competitive then the full amount of the increase in labor costs should be passed on to the consumer, output will fall, and employment will decline. However, if firms are competing as oligopolists and making positive profit in equilibrium, then an increase in the minimum wage may lower profitability while having only small effects on prices, output, and employment. See discussion in Aaronson and French (2007) and Draca, Machin and Van Reenen (2011).

ously competitive, prices may be sticky (literally, "menu costs"), and so it's natural to wonder if restaurants have the capacity to adjust menus in response to entry[3]. In our sample restaurants change their menus with high frequency; the median duration between price changes is two weeks. Therefore it's worth emphasizing that our results show frequent menu changes but no *differential* change in response to entry. This finding is consistent across a battery of specifications, including cases where we expect new competition to elicit the largest incumbent response. However, we find a relationship between high intensity of nearby entry and a higher rate of exit, which suggests that competition does affect firm profit. Our results thus broadly support the weak strategic interaction assumptions of aspatial monopolistic competition models, and are relevant for a variety of related subjects, including retail competition, firm clustering, and location choice.

The remainder of the paper is organized as follows. First, we discuss differences between spatial and aspatial competition in a conceptual framework to illustrate our empirical strategy, and then briefly review the empirical literature on imperfect competition in differentiated markets. Next we describe our data, provide a definition of new competition, and present descriptive statistics. After, we discuss the potential endogeneity in our estimation and our implementation of a matching strategy to account for this. The strategy includes the construction of a measure of product distance from our menu data. We then present our main results on the causal response to competition in physical space and evaluate the robustness of these findings. As an extension, we repeat our main analysis but define competition in characteristics (menu) space. In a further extension, we conduct a Monte Carlo exercise to examine how the menus of incumbent restaurants affect the location choices of entrants. Lastly, we estimate the effect of entry intensity on the likelihood of incumbent restaurant exit. We conclude with a summary and interpretation of our results.

1.1 Conceptual framework: local versus global competition

What does economic theory suggest should be the response of an incumbent restaurant to competition from a new entrant? In their textbook, Mas-Colell, Whinston, Green et al. (1995, p. 400) write, "In markets characterized by monopolistic competition, market power is accompanied by a low level of strategic interaction, in that the strategies of any particular firm do not affect the payoff of any other firm." They then follow this with a footnote: "In contrast, in spatial models, even in the limit of a continuum of firms, strategic interaction remains. In that case, firms interact locally, and neighbors count, no matter how large the economy is." Anderson and de Palma (2000) refer to this distinction as "local" versus "global" competition: are restaurants competing directly with their neighbors in physical or product space, or do they simply compete indirectly for a share of a consumer's expenditure with all other restaurants in the market?

We use the demand structure from Anderson and de Palma (2000) to provide a conceptual framework for our empirical analysis of the response to entry. Their model combines discrete choice logit demand with an explicit distance between a consumer and each firm, thus allowing for both spatial and aspatial competition. We focus on how parameters of the consumer's utility function determine the degree to which a new entrant captures demand from a nearby incumbent.

There are n restaurants in the market and each consumer must choose a single restaurant at which to eat. The indirect utility to consumer i from eating at restaurant j is:

$$V_{ij} = \nu(p_j) + \varepsilon_{ij} \tag{1}$$

[3]There is some evidence of price competition in the literature, with both Thomadsen (2007) and Kalnins (2003) studying local competition among hamburger restaurants. There are also many reports of restaurant competition in the media. For a recent example in the The Wall Street Journal, see "McDonald's Focus on Low Prices Brings in Customers" (March 21, 2019, (Gasparro 2019)). For an amusing account of New York City restaurant competition, see "In Manhattan Pizza War, Price of Slice Keeps Dropping," The New York Times, March 30, 2012 (Kleinfield 2012).

The term $\nu(p_j)$ represents the net consumer surplus to any consumer eating at j when the restaurant charges price p_j. The term ε_{ij} is a match value between the consumer and the restaurant. Adapting this slightly to our context, we assume it takes the form:

$$\varepsilon_{ij} = -t^g d^g_{ij} - t^m d^m_{ij} + \mu e_{ij} \tag{2}$$

Equation 2 allows the match value to depend on the geographic distance, d^g_{ij}, between consumer i and restaurant j (e.g., measured in km), and a distance in characteristics space, d^m_{ij}, representing how close the menu of the restaurant is to the consumer's ideal menu. The importance of these two distances is determined by the transportation cost parameters, t^g and t^m, which we assume are positive but for which we make no other assumptions. The e_{ij} is the idiosyncratic match between the consumer and the restaurant, which could be interpreted as the consumer's preference for characteristics of that restaurant not already captured in the two distance terms, such as service quality or decor. This term is distributed extreme value type 1 and i.i.d. across restaurants so that the probability consumer i chooses j takes the logit form. The μ term represents the importance of this idiosyncratic match. Given the assumption on the distribution of e_{ij}, the probability consumer i chooses j is:

$$P_{ij} = \frac{exp[(\nu(p_j) - t^g d^g_{ij} - t^m d^m_{ij})/\mu]}{\sum_{k=1}^{n} exp[(\nu(p_k) - t^g d^g_{ik} - t^m d^m_{ik})/\mu]} \tag{3}$$

When μ is quite small relative to the transportation cost parameters, then competition is entirely local and firms only compete with their closest neighbors. The definition of close depends on the relative sizes of t^g and t^m. If t^g is much larger than t^m, then firms mostly compete with their closest geographic neighbors; if t^m is much larger than t^g, then competition is with restaurants that have the most similar cuisine. As μ increases some consumers will choose restaurants beyond the minimum distance to their geographic location or ideal menu, and thus restaurants will compete with more distant firms. When μ is large relative to transportation costs, then the geographic distance or menu similarity between firms becomes irrelevant and all firms compete with each other in global competition. When there are many firms this is monopolistic competition: an individual firm becomes negligible and each firm ignores the actions of other firms (Hart 1985, Wolinsky 1986). In fact, as Anderson and de Palma show, with specific assumptions about the form of $\nu(p)$, the model collapses to the canonical CES form of Dixit-Stiglitz (Dixit and Stiglitz 1977) where firms choose a constant mark-up over marginal cost[4].

If firms compete locally by setting prices, then equation 3 implies that the price of firm i should be a function of the prices of other nearby firms. This observation informs the empirical strategy of Pinkse, Slade and Brett (2002), who use a sophisticated econometric model and cross-sectional data to estimate the best response function of gasoline wholesalers to competitors at different distances, concluding that competition in the wholesale gasoline market is highly localized. By contrast, in this paper we seek to take advantage of rich longitudinal data on restaurants to use simple estimation methods without structural assumptions, and to allow responses to competition along both price and non-price margins.

To illustrate the basic strategy of our empirical work, consider a market that has two restaurants, A and B, separated by a significant geographical distance from the perspective of consumers (d^g_{AB} is large). For simplicity, we start by assuming $t^m = 0$, so that spatial competition is confined to geography. Now a third restaurant, C, enters the market close to A and far from B ($d^g_{AC} < d^g_{AB}$ and $d^g_{AC} < d^g_{BC}$). If transportation costs are important, meaning t^g/μ is large, then restaurant A now faces significant competition for consumers located between A and the new entrant C, and therefore has an incentive to respond. However, restaurant B should not change behavior since it is unaffected by this new entrant, having never received business from

[4]Setting $t^g = t^m = 0$ and assuming that $\nu(p) = ln(p)$ yields CES demand, see p440 of (Anderson and de Palma 2000).

the distant consumers near A. On the other hand, if competition is global (t/μ is small), then the distance doesn't matter and both A and B will be affected equally by C. Therefore we can test for the presence of local competition by comparing the response of restaurants facing a new nearby competitor to the post-entry behavior of restaurants without new competition.

If we now allow $t^m > 0$, then the above scenario becomes somewhat more complicated. First, the definition of a nearby entrant becomes unclear since the relevant distance could be measured in geographic space, menu space, or some combination of the two. For this reason, and as discussed in depth in section 3.1, we test different specifications of distance. Second, incumbent restaurants may now respond to entrants by updating their menu, which changes the distances d^m between consumers and the restaurant. Depending on the distribution of consumer preferences, the incumbent restaurant could change their menu to increase differentiation with the entrant or actually make their menu more similar to that of the entrant[5]. Therefore we take a flexible approach and examine a range of price and product responses. While these considerations add some complexity to our empirical analysis, the basic design remains the same: if competition is local then restaurants which experience a local competitive shock will change their behavior more than restaurants without new local competition.

1.2 Evidence on competition in differentiated markets

Much of the empirical work on competition in differentiated markets focuses on how market size affects average firm outcomes (mark-ups, capacity, output), rather than examining specific responses to new competition. Syverson (2004) uses a spatial competition model to argue that larger markets will have more efficient firms and then finds evidence of this pattern in the market for ready-mixed concrete. Campbell and Hopenhayn (2005) use an aspatial monopolistic competition model to show that the effect of market size on firm output and price mark-ups depends on whether the entry of additional firms increases the average substitutability of each firm's product, thus increasing competition, or if new entry is always symmetrically differentiated from existing firms. They test this prediction using cross-sectional data from the 1992 *Census of Retail Trade* on a number of industries, including restaurants, and find that restaurants in larger markets have greater average size (sales, employment) and a greater dispersion of sizes. In a follow-up paper, Campbell (2011) finds that restaurants in larger cities have lower prices, greater seating capacity, and lower exit rates. The author concludes that these results are evidence of the importance of strategic interaction in the restaurant industry, namely that markups decrease with market size, requiring firms to have greater volume to break even. This conclusion is in contrast to our findings showing no local strategic interaction in New York restaurants. However, the two sets of results are not inconsistent: more recent monopolistic competition models allow for market size effects on markups without any local strategic interaction[6]. Lastly, Hottman (2016) examines markups in the retail industry across US counties using a nested CES model where retailers differ in quality and therefore size. Higher quality firms face less elastic demand and make decisions taking into account their effect on the overall price index. This feature of the model allows firms to act strategically (the author analyzes both Cournot and Bertrand cases), but there is still no local interaction in the sense of

[5] In many spatial competition models firms seek to differentiate their products in order to mitigate direct price competition (see Tirole(Tirole 1988), Chapter 7 for an overview of relevant models). For tractability these models often assume uniformly distributed demand, but it's quite possible that New York City restaurant demand is "lumpy" with concentrations of demand for different cuisines.

[6] Quite a few papers have modified the original CES framework and shown that these changes could lead to market size effects on mark-ups, see discussion in Parenti, Ushchev and Thisse (2017) and the survey of monopolistic competition models in Thisse and Ushchev (2018). Further, several authors have developed more general variable elasticity of substitution (VES) models that encompass the CES framework as a special case, including Behrens and Murata (2007), Zhelobodko, Kokovin, Parenti and Thisse (2012), Dhingra and Morrow (forthcoming),Bertoletti and Etro (2016), and Parenti et al. (2017).

competition with a specific rival. Using retail scanner data the author finds that markups are significantly lower in larger US counties, and that interestingly for our study, markups in New York City are "close to the undistorted monopolistically competitive limit."

There is less empirical work on local competitive responses in differentiated industries. Netz and Taylor (2002) examine patterns of location for gasoline stations in Los Angeles and conclude that increased competition leads to increase spatial differentiation, defined as the geographic distance between stations. They also look at the relationship between spatial differentiation and characteristics differentiation, which they measure using attributes of the stations, such as gasoline brand or repair services available. They find a positive relationship between these two types of differentiation. Kalnins (2003) reports that hamburger prices at proximate restaurants of different chains are uncorrelated while hamburger prices at proximate restaurants of the same chain are correlated, suggesting price competition exists among similar restaurants. However, chain restaurants may have very different incentives in their price decisions than non-chain restaurants (Lafontaine 1995). There are also a number of papers examining entry of large retailers or grocers on incumbent firms[7]. Our empirical approach has some similarity in that we also use panel data to estimate the effect of entry on incumbent firms, but our context is quite different and lacks the large asymmetries in firm size central to these other papers.

Pinkse and Slade (2004) estimate cross-price elasticities of competing British beers and then use the estimates in a structural model to simulate the effects of mergers among brewers. They find that brands of the same beer type (lager, ale, or stout) have the strongest cross-price effects, with significant but weaker cross-price effects for brands with similar alcohol content (one of their measures of distance in product space). In our context, we might expect to find that incumbent menu responses are larger to entrants of the same cuisine. Chisholm, McMillan and Norman (2010) investigate competition between thirteen first-run movie theaters in Boston. They find that theaters closer in geographic space are more distant in product space, as measured by film-programming choices over a one year period. Sweeting (2010) studies mergers between radio stations in the same listening format and geographic market to study the effect of common ownership on product differentiation. He finds that after two stations come under common ownership, the new owner increases differences between the music playlists of the two stations and repositions at least one of the stations closer to other competing stations. He also looks at whether the merger increases implicit listener prices, measured as commercials per hour, but finds no statistically significant result. Busso and Galiani (2019) undertake a randomized control trial of changing the competitive environment for grocery stores in the Dominican Republic. They find that incumbent stores lower their prices but do not change the quality of their products or services.

The markets we study and the data we use share some features with earlier studies, but differ in several important ways. First, most studies of differentiated markets with large numbers of firms quantify competitive effects through market level outcomes, such as average mark-ups or dispersion, but do not analyze how individual firms respond to competition. The studies that focus on individual firms tend to do so in markets with relatively few firms. Second, the majority of papers examine equilibrium outcomes with cross-sectional data or product changes in markets with little entry or exit. In contrast, our work is focused on dynamic responses to new competition in markets with substantial entry and exit, which helps us to more easily control for firm heterogeneity[8]. Third, while some previous work has quantified the similarity of two firms' product offerings in a differentiated market (radio, movies), our dataset of restaurant menus not only

[7] This is a well developed literature. Two notable examples include Basker (2005) on Walmart and Atkin, Faber and Gonzalez-Navarro (2018) on the entry of international retailers into Mexico.

[8] Sweeting (2010) also uses a panel to look at dynamics. However, both his focus on mergers, rather than entry, and the substantial differences between the radio industry and the restaurant industry (geography, number of firms, business model) make it difficult to extrapolate his results to our context.

provide extensive detail on product differentiation, but also give itemized prices, allowing for a richer study of price competition across firm attributes.

2 Overview of data

We collected data on New York City restaurants from the Grubhub website, which lists restaurant menus in a standardized text format. Grubhub is the largest food delivery platform in the United States with 16.4 million active users and 95,000 restaurants as of late 2018 (Grubhub 2018). Restaurants are highly dependent on the service; in reference to Grubhub one New York restaurateur told a local media outlet "If I stop using them, tomorrow I close the door" (Torkells 2016). An important feature for our study is that customers order and pay for food from a restaurant directly through the website, which implies that the prices and items listed on the menu are current. As Cavallo (2016) notes, these high-frequency directly-measured prices avoid some of the potential limitations associated with scanner data sets and the observations used in CPI calculations.

We collected data on every available restaurant weekly from the week of November 27, 2016 through the week of March 11, 2018 for a total of 68 periods. We observe restaurants joining the website and leaving the website, giving us an unbalanced panel of menus from roughly 11,700 unique restaurants (550,000 restaurant periods). The top panel of Figure 1 shows the count of restaurants in every week, along with the stock of restaurants observed in the first period that are present in each subsequent period. The bottom panel shows the count of new restaurants appearing on the website (site entrants) and the count of restaurants that have left the website (site exits) each period. As of Februrary 2017, the New York City Department of Health listed approximately 24,000 active restaurants, which implies that over one-third of the city's restaurants appear in our data each period. Our data likely features some selection on restaurant characteristics; for example, extremely expensive restaurants may not offer delivery. Nonetheless, we believe the size of this dataset is sufficient to allow us to make general statements about restaurant competition.

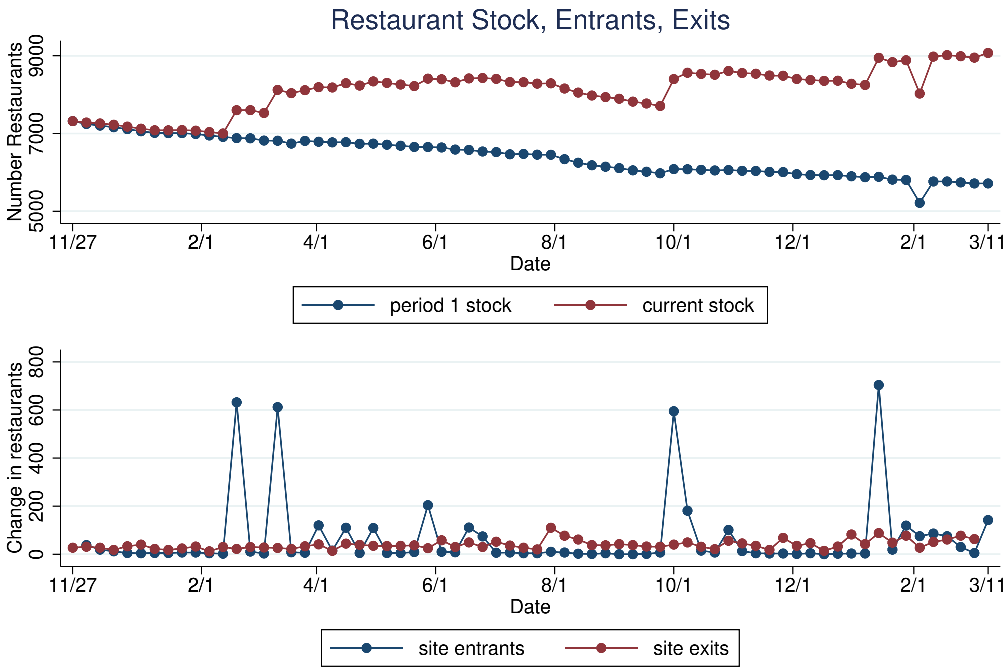
2.1 Sources of noise

While our dataset contains a high level of detail on restaurant prices and products, it also has a fair amount of noise. This measurement error is found in our outcome variables and therefore is unlikely to bias coefficient estimates. However, a legitimate concern is that the noise could obscure measurement of competitive responses. In this section we describe the issues and sources of the noise; later in our empirical analysis we show that our results are robust to various specifications addressing the noise.

There are three sources of noise in our data which we refer to as 1) "outliers" 2) "missing data" and 3) "time-of-day effects." We use outliers to describe menus that show very unusual values, such as extremely high or extremely low prices or item counts. Many of these reflect idiosyncratic situations, such as when a restaurant lists a catering package for 100 people, priced at $2000, as an item on the menu. We classify these cases as outliers using a set of conservative rules and drop them from all of the analysis, decreasing our sample by 2.4% (roughly 13,500 restaurant periods)[9].

The second source of noise comes from data collection difficulties caused by website changes, which resulted in some missing data. For four consecutive periods starting the week of April 23 we are missing the prices for all menu items, and thus we do not use these periods in most of our analysis. Additionally, we are missing item names for five consecutive periods starting the week of September 24th. Item names in every period are not necessary for our estimation work, but we do need them in order to accurately drop duplicate

[9] Specifically, we drop restaurant periods where the item count is less than 10 or greater than 500, where the median item price is less than $2.5 or greater than $25, and where the mean item price is greater than $50.



Data is 68 wks, 11/27/2016-3/11/2018; entrants not defined in first period, exits not defined in last period.

Figure 1: Stock and flow of restaurants on website.

items, affecting our measurement of item counts and prices[10]. Therefore we also drop these periods from most of our analysis. For a couple periods we did not collect review data (count of reviews, stars, measures of quality), but we do not use these variables much in our analysis.

Our third source of noise comes from a unique feature of the website, in which the menus shown to the user can change depending upon the time of day the page is viewed. Some restaurants offer different menus for different meals, such as a breakfast, lunch, dinner, or late night menu. Additionally, when a restaurant is closed users have the option to pre-order, but the items shown may be only those core items that the restaurant always serves (many restaurants still show a full menu). When the restaurant is open the menu may be longer and include daily specials and other items not part of the core set. Since we collect data at different times of day throughout our panel, we sometimes observe just a core menu or short lunch menu, while at other times we see the full menu for that day. This can generate what looks like large period to period changes in the menu, but instead simply reflects the time of day viewed. In these cases the number of items observed in a period may oscillate between two fixed item counts—such as a closed menu and an open menu—providing us a way to identify this situation. We address this source of noise in three ways. First, we define "oscillating periods" as a set of three consecutive periods in which the first to second period absolute change in the log item count is larger than 0.15 log points, and the second to third period change is also larger than 0.15 log points, but the change is in the opposite direction[11]. An absolute change of 0.15 log points is a large change–about the 90th percentile of all period to period changes in log item count–and

[10] Restaurants may list the exact same item, with the same price, multiple times in different sections of the menu, often in a promotional or "popular items" section. For these five periods our item count would be inflated and quantiles of the price distribution would be inaccurate since some items are multiply counted.

[11] In notation, we define oscillating periods as three consecutive periods, $\{t-1, t, t+1\}$, where $abs(ln(itemct_t) - ln(itemct_{t-1})) \geq 0.15$ and $abs(ln(itemct_{t+1}) - ln(itemct_t)) \geq 0.15$ and $(ln(itemct_t) - ln(itemct_{t-1})) \times (ln(itemct_{t+1}) - ln(itemct_t)) < 0$.

thus two consecutive large swings in menu length of opposite directions is quite unlikely to be a permanent change to the menu. There are about 50,000 oscillating periods in our data (not already tagged as outliers), about 9% of our sample, and we drop these periods from much of our analysis. Second, for most weeks in our sample we know the exact time the menu was downloaded, as well as the listed hours of the restaurant. Therefore in our main specification we include fixed effects for the hour of day and whether the restaurant was open when the menu was collected. Lastly, we also run our analysis at the restaurant-item level by examining price changes over time for a constant set of restaurant menu items, which ensures that missing items do not affect our estimates.

It is worth emphasizing that all three sources of noise are completely unrelated to entry and thus our definition of treatment. Further, this noise does not lead to problems of precision in our estimates. Even after dropping observations that could increase measurement error, we still have a large sample and can estimate coefficients with small standard errors.

2.2 Descriptive statistics

In Table 1 we show characteristics of the restaurants, averaged across restaurant-periods. On average, each menu has 124 items, and therefore we calculate price statistics for each menu and then examine these menu-level statistics across all restaurant periods. For example, the variable "median item price" represents the median price across all items on a restaurant's menu in a single period; the median item price averaged across all restaurant-periods is \$8.62 and the median is \$8. The average price of the most expensive item on the menu, "max item price," is about \$32.5 and for the average restaurant the mean item price (\$9.40) is above the median. In addition to menus, the website also lists restaurant level characteristics, such as the number of cuisines, count of user reviews, and measures of user ratings.

Table 2 examines changes in menus for item counts and price variables. For each variable, we define a unique menu as consecutive periods of a menu with no change in the variable. For example, if a restaurant keeps the same number of items on its menu for four consecutive periods before changing in the fifth period, then we define the first four periods as one menu and the menu in the fifth period as another. With this method we can calculate statistics on menu durations, as well as the size of changes, for different variables. The first row of Table 2 shows that the mean duration (column 3) for a menu with the same item count is 3.9 periods (weeks) while the median duration (column 4) is just one period. These statistics are calculated from $141,666$ unique constant item count menus (column 5). When the item count changes the average change is 8.91 items (column 1) while the median change is 3 items. All change statistics are calculated as absolute changes, $|x_t - x_{t-1}|$, so that positive and negative changes don't nullify each other. Note that columns 1 and 2 are calculated from changes whereas column 5 shows the count of unique menus. The average duration for a menu with the same median item price is 7.67 periods and the average change to this price is \$0.84. On the other hand, the average duration for a constant *mean* item price is only 3.69 weeks but with a smaller change of \$0.28. Interestingly, different quantiles of the item price distribution change with different frequencies, with the ends of the distribution (min, max) changing the least frequently.

Lastly, in Table 3 we look at changes over time within a restaurant by running regressions of the form:

$$Y_{rt} = \beta * weeks_{rt} + \eta_r + \varepsilon_{rt} \tag{4}$$

The η_r term is a restaurant fixed effect and the "weeks" variable measures the number of weeks (periods) since we first observed the restaurant. We cluster standard errors by restaurant. From columns 1-4 we can see that restaurants slowly increase their median item prices at roughly \$0.007 per week, with much larger changes for the most expensive menu item. Menus increase in length by about 0.09 items per week and the average restaurant receives about 5.3 new reviews each week. The decrease in the user rating of food quality

Table 1: Descriptive statistics on restaurant characteristics.

	mean	median	sd	min	p1	p99	max	N
item count	124.44	100.00	88.66	10.00	15.0	399.0	500	419782
median item price	8.62	8.00	3.35	2.50	3.0	18.5	25	419782
mean item price	9.40	8.82	3.88	2.28	3.9	22.9	49	419782
min item price	1.59	1.25	1.42	0.00	0.0	8.0	25	419782
max item price	32.52	22.50	49.29	2.99	7.5	190.0	2199	419782
cuisines	4.05	4.00	3.11	0.00	0.0	14.0	35	423214
reviews	380.63	206.00	509.99	1.00	4.0	2326.0	10064	370764
stars	3.72	4.00	1.19	1.00	1.0	5.0	5	395984
food rating	85.30	88.00	9.62	0.00	50.0	100.0	100	406096
order rating	89.61	92.00	9.01	0.00	56.0	100.0	100	406093
delivery rating	86.09	89.00	11.09	0.00	46.0	100.0	100	406079

Statistics averaged across all restaurant-periods.
Sample excludes outliers, oscillators, missing item name periods, and missing price periods.
Review information not collected for all periods.

Table 2: Descriptive statistics on menu changes and durations.

	mean	median	mean dur	med dur	N
item count	8.91	3.00	3.90	1	141666
median price	0.84	0.50	7.67	2	72001
mean price	0.28	0.09	3.69	1	149781
min price	0.96	0.50	30.16	23	18307
p25 price	0.54	0.26	7.54	2	73193
p75 price	0.98	0.50	7.85	2	70363
max price	14.07	3.05	20.86	10	26471

Stats calculated for unique changes specific to each var.
Mean and median use absolute changes.
Duration is number continuous periods with no var change.
N indicates count of unique menus across all restaurants.
Exclude outliers, oscillators, missing item/price periods.

is statistically significant, but with an average food rating of 85.5, this change is not meaningful. Overall, Tables 2 and 3 show that while restaurant menus are generally quite stable, there is still a fair amount of change, both across restaurants and within restaurants, with which we might measure competitive responses.

2.3 Measuring entry

Unfortunately, the appearance of a new restaurant menu on the delivery website does not imply that the restaurant has just entered the market. In order to determine entry we combine data from two additional sources: restaurant inspections from the City of New York and restaurant reviews from Yelp.com. According to the New York City government website, all restaurants in the city must have a "Food Establishment Permit" and a pre-permit inspection is required before the restaurant can open (NYC Department of Consumer Affairs 2019). This suggests that pre-permit inspection dates should capture market entry. However, although the inspection data begins in August 2011, there are many restaurants whose first inspection date is in 2014 or later without a recorded pre-permit inspection. This implies that the sample may include entrants

Table 3: Regression results for within-restaurant menu changes.

	(1) item ct	(2) p50 item prc	(3) mean item prc	(4) min item prc	(5) max item prc	(6) reviews ct	(7) food rtng
weeks observed	0.0886***	0.0068***	0.0088***	0.0001	0.1142***	5.2740***	-0.0113***
	(0.0048)	(0.0002)	(0.0004)	(0.0001)	(0.0139)	(0.0835)	(0.0009)
Observations	456153	456153	456153	456153	456153	404211	441055
Clusters	11302	11302	11302	11302	11302	10403	10576

All specifications include restaurant fixed effects.
Sample excludes outliers, oscillators, missing item/price periods.
Standard errors clustered by restaurant, * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

without pre-permit inspections[12]. Further, for some restaurants whose initial inspection occurs during our sample period, the first reviews on Yelp far precede this initial inspection date. To ensure we have accurate dates for entry we use the following procedure. For each restaurant which first appears in the inspection data during our sample period, we find the date of the first Yelp review for the restaurant. If the first Yelp review is less than 90 days days before the first inspection or less than 35 days after the first inspection, we assume that this is a newly opened restaurant[13]. We define the entry date as the earlier of the first inspection date and the first Yelp review date. In Figure 2 we show two and half years of entry, from November 1st, 2015 to March 17, 2018. The area to the right of the vertical line shows entry over our main analysis period, or the period for which we have menu data, November 27, 2016 to March 17, 2018. The area to the left we refer to as the "pre-period" and only use in an extension to our main analysis in Section 5.

3 Empirical approach

Our identification strategy compares the behaviour of restaurants which have experienced a change in their competitive environment with restaurants which have not. We use a two-stage matching process to control for heterogeneity. Specifically, we seek restaurants which have both similar location characteristics and menu characteristics. As described in further detail below, the empirical approach proceeds as follows:

1. Assign "treated" status to restaurant-periods which have a new entrant open within a specified distance and "control" status to restaurant-periods with no entrants within this distance.

2. Pair each treated restaurant with a control restaurant, over the exact same periods, in a two-stage process that matches first on locational attributes and then on menu text.

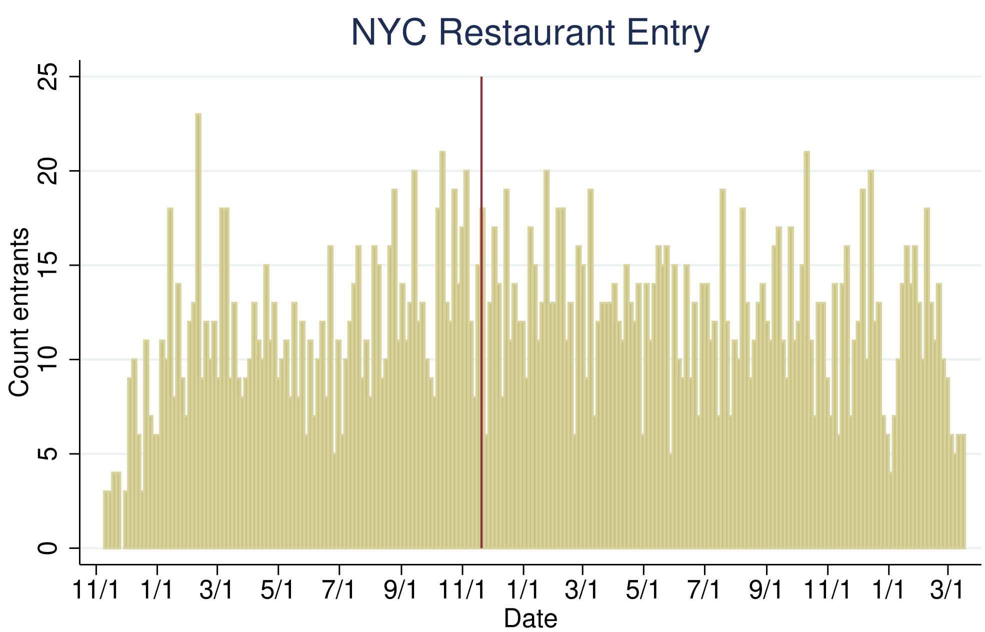
3. Run regressions on the matched sample of treated and control pairs to measure the causal response to the new entrant.

Given the complexity of the data set, we provide explicit notation in Table 4. Throughout, we index restaurants in our sample by $r \in R$, entrants by $e \in E$, and periods by $t \in T$.

[12] A call to the New York City Department of Health and Mental Hygiene, which oversees inspections, confirmed that while all restaurants should request an inspection before opening, this does not always happen.

[13] To choose this duration we randomly selected 300 restaurants whose first inspection was within 100 days of their first Yelp review. Next we read all the reviews for these restaurants in order to determine which were likely to be new, looking for phrases such as "newly opened," "a welcome addition to the neighborhood," "this could become my new favorite [cuisine] spot," "I've been waiting for this place to open," and "went on the grand opening date." We labeled restaurants as new only if it was quite obvious from the reviews. Finally we looked at a histogram of the difference in days between the review and inspection dates for these new restaurants and defined our threshold using the 5% and 95% percentiles, a symmetric range that covered 90% of new restaurants.

Figure 2: Entrants identified from inspection and Yelp data.

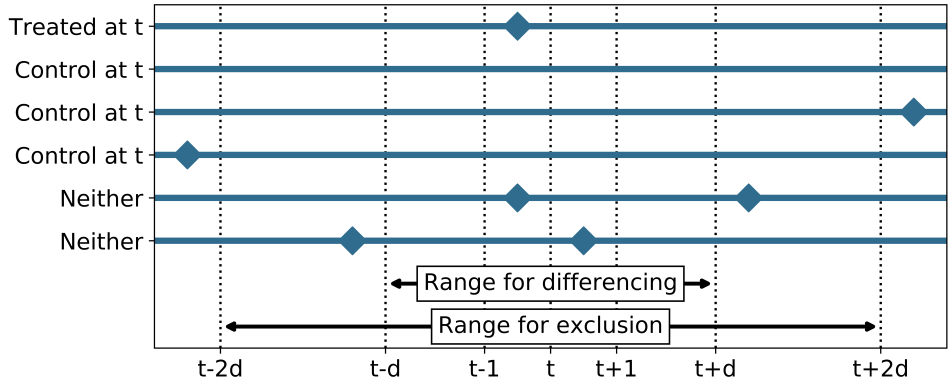


Entry date calculated as earliest of inspection and Yelp date, 11/1/2015-3/17/2018.
Bin width is 4 days; there are 2,585 entrants over the entire period.
Menu data period, 11/27/2016-3/17/2018, right of vertical line.

Table 4: Notation used to describe menu data. Refer to the text for further description.

L_r	Location of restaurant r
τ_r^o	First date in sample for restaurant r
M_r	Menu text for restaurant r
Y_r	Other attributes for restaurant r (e.g. hours)
D_{rt}	Indicator for treated status of restaurant r in period t
e_{rt}	Entrant near treated restaurant r in period t
c_{rt}	Control matched to treated restaurant r in period t
k_r	First treatment period for restaurant r
$X(L)$	Locational attributes of location L
$P(L)$	Observed entrant intensity at location L
$\hat{P}(X(L))$	Predicted entrant intensity at location L
$\rho(L,L')$	Spatial distance from L to L'
$\omega(M,M')$	Cosine distance from M to M'
ρ_T	Inner radius for treatment assignment
ρ_C	Outer radius for treatment assignment
d	Duration of treatment window

Figure 3: Schematic of the timing for treatment and control assignment.



3.1 Treatment and control

We define treatment as the opening of a new entrant nearby. We do not know *a priori* the spatial range over which restaurants compete, nor the timescale with which they may change their menus in response to the entrant. Further, for incumbent restaurants facing multiple entrants, it could be difficult to identify which entrant the incumbent is responding to. Therefore we choose to focus on cases where an entrant is most likely to represent a change in competition and where the response to a specific entrant can be isolated.

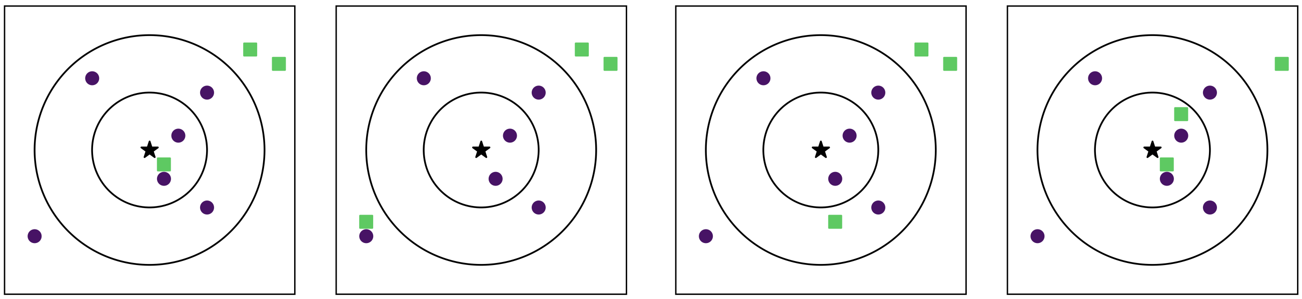
To implement this, we specify a tuple (d, ρ_T, ρ_C) where d is a duration (measured in weeks), ρ_T is an inner radius, and ρ_C is an outer radius (i.e. $\rho_C > \rho_T$). In our main analysis we measure ρ_C and ρ_T in meters (physical space) but in Section 4.3 we use a measure of the distance between menus (characteristics space). A restaurant is deemed treated at time period t if and only if exactly one entrant within radius ρ_T first operates in period t and no other restaurants open from period $t-2d$ through period $t+2d$ anywhere within the larger radius ρ_C. A restaurant is deemed to be a control if and only if no restaurants open anywhere within radius ρ_C from period $t-2d$ through period $t+2d$[14]. Note that many restaurant-periods will be neither treated nor control. Figure 3 provides a schematic of the timing of treatment and control definitions. Figure 4 provides a visual representation of the spatial aspects of treatment and control definitions.

These definitions yield conservative samples of treatment and control restaurants. The separate radii ρ_T and ρ_C enforce a "buffer" between situations where the change in competitive environment from the nearby entrant is salient and situations where any new entry is too far away to have a substantial effect. Only including restaurants with exactly one entrant over $2d$ periods ensures that we are including restaurants which have experienced a comparable change in local competitive intensity. In our regression analysis we use a subset of this window, analyzing changes in a restaurant's menu from period $t-d$ to period $t+d$. Therefore the long $t \pm 2d$ window serves a similar function to the distance buffer by helping us to exclude lagged effects and thus isolate effects only due to the observed new entrant. An important aspect of this definition is that treatment is determined by geography *and* timing. Over our entire sample period two incumbent restaurants r and r' may receive the same number of entrants within distance ρ_T, but for a given

[14] Formally, we define the sets of treated and control restaurants $R_t^T(d, \rho_T, \rho_C)$ and $R_t^C(d, \rho_T, \rho_C)$ at period t as follows:

$$\begin{aligned} R_t^T(d, \rho_T, \rho_C) = \{ r \in R : & |\{e \in E : \tau_e^o = t \wedge \rho(L_r, L_e) < \rho_T\}| = 1 \wedge \\ & |\{e \in E : \tau_e^o \in [t-2d, t+2d] \wedge \rho(L_r, L_e) < \rho_T\}| = 1\} \\ R_t^C(d, \rho_T, \rho_C) = \{ r \in R : & |\{e \in E : \tau_e^o \in [t-2d, t+2d] \wedge \rho(L_r, L_e) < \rho_C\}| = 0\} \end{aligned}$$

Figure 4: Examples of treatment and control assignment



(a) Treatment (b) Control (c) Neither (d) Neither

The caption for each example indicates the assignment for the restaurant at the centre of the diagram (indicated by a star). Blue circles represent incumbent restaurants and green squares represent entrants. The two concentric circles represent the radii ρ_T and ρ_C.

period t it may be that r is treated, $r \in R_t^T(d, \rho_T, \rho_C)$, while r' is a control, $r' \in R_t^C(d, \rho_T, \rho_C)$. In this way our approach is somewhat similar to identification strategies that compare treated agents with agents that will be treated in the future.

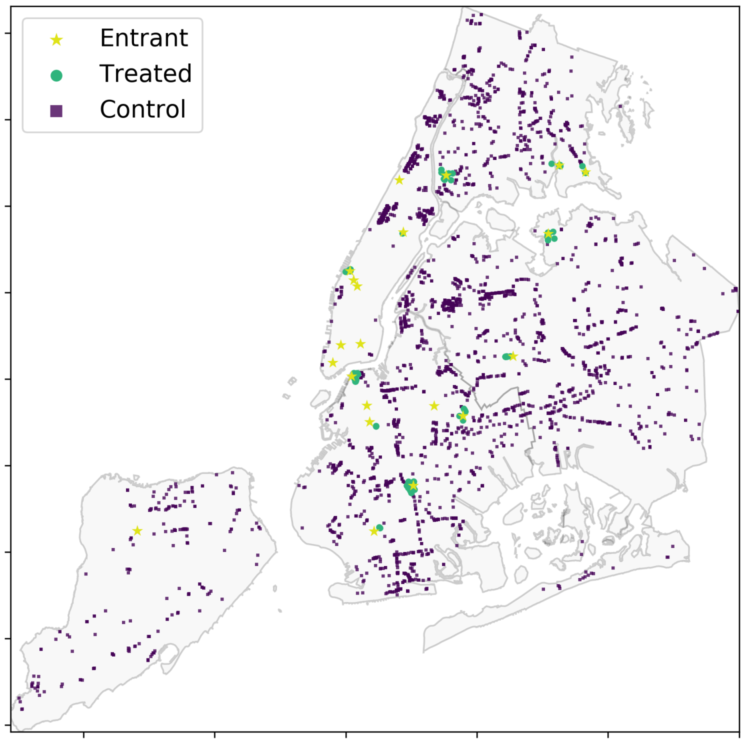
In our analysis we use an inner radius of $\rho_T = 500$m and an outer radius of $\rho_C = 600$m. These radii capture the spatial scale regarded as a reasonable walking distance in the urban planning literature. In the 1995 Nationwide Personal Transportation Survey the median length of a daily walking trip is a quarter mile (Boer, Zheng, Overton, Ridgeway and Cohen 2007). Krizek (2003) describe this as "a scale sensitive to walking behavior". Our scale corresponds to approximately two long "avenue" blocks or six short "street" blocks in Manhattan (Pollak 2006). Figure 5 shows an example of treatment and control for February 6, 2017, using these radii and a duration of four weeks. The blank regions in lower Manhattan—an area with many restaurants—shows that the parsimonious specification of treatment and control excludes many restaurants for being near to several simultaneous openings.

We examine three durations in our regression specifications: four, six, and eight weeks ($d \in 4,6,8$). In choosing these durations we face a tradeoff between the response window and the sample size. If incumbent restaurants are slow to adapt to new competition, then a longer duration may better capture any potential responses. On the other hand, a longer duration d requires that a treated restaurant has only one new competitor within $4 * d$ weeks, and a control restaurant has no competitors over this time period. New York City has frequent entry and therefore the number of restaurants satisfying this requirement drops quickly as the duration increases. At long durations, the remaining restaurants may be less representative of the market. Further, with fewer control restaurants it becomes more difficult to find a good match for the treated restaurant. Given these issues, and the high frequency of menu changes shown in Table 2, we chose three durations that we thought could capture important competitive responses while still yielding a sufficient sample size. In Section 4.2 we examine the robustness of our results to extended durations.

3.2 Endogeneity and identification

In this section we discuss potential endogeneity concerns and our identification strategy; in Appendix A.1 we formalize these ideas with notation from the potential outcomes framework. Let Y_{rt} be a restaurant level outcome (e.g. median price or item count) for incumbent restaurant r at location L_r at time t. Denote the period when a new competitor enters near restaurant r as k_r, which is the first treatment period; $k_r = \emptyset$ if r is never treated. Let D_{rt} indicate whether at time t a new competitor (entrant) has entered within radius

Figure 5: Treatment and control assignment for the week of February 26, 2017 under the $d = 4$ specification.



ρ_t of restaurant r, so that $D_{rt} = \mathbb{I}\{t \geq k_r\}$. Our reduced form model for restaurant outcome Y_{rt} for $t \in [k_r - d, k_r + d]$ is:

$$Y_{rt} = \beta * D_{rt} + u_r + u_{L_r} + \xi_{rt} + \xi_{L_r t} + \varepsilon_{rt} \tag{5}$$

Our objective is to estimate β, but there may be a variety of restaurant and neighborhood level effects, both time-varying and invariant, that affect restaurant r's outcomes. The time-invariant restaurant effect u_r could represent a restaurant's tendency to generally have high prices or a long menu in every period while the location effect u_{L_r} could capture the average income level or house price for a neighborhood over time. The ξ_{rt} variable represents restaurant-specific time-varying shocks, such as the hiring of a new chef or a price increase in some ingredient important for that restaurant. There could also be location specific shocks, represented by $\xi_{L_r t}$, such as gentrification in a neighborhood or new road construction that deters customers. Lastly, ε_{rt} represents i.i.d. shocks affecting restaurant r at time t.

As discussed in Appendix A.1, the entry process may also be a function of characteristics of incumbent restaurant r and location L_r, both time-varying and invariant. If any of the factors affecting entry are also correlated with the restaurant outcome variables in equation 5, then the coefficient β estimated from a simple regression of Y_{rt} on the treatment indicator D_{rt} would be biased due to selection. In fact, in Appendix Table A2 we show that treated restaurants are in higher income locations, have higher menu prices, and differ in a number of other ways. Many realistic processes could generate selection and lead to such differences. For example, certain types of restaurants (e.g., coffee shops) may always have low prices and attract additional entry, a correlation between fixed factors. Alternately, unobserved changes to a neighborhood (such as gentrification or a neighborhood becoming "trendy") could affect both existing restaurants and entry probabilities. Relatedly, unobservable restaurant-level shocks could also change outcomes and spur entry. If incumbent restaurant r is struggling because their cuisine has suddenly become less popular then the restaurant may try to lower prices to attract consumers while, at the same time, a new entrant may locate nearby because they expect little competition from an unpopular cuisine type.

We address these concerns with a difference-in-difference matching strategy (see Heckman, Ichimura, Smith and Todd (1998) and Smith and Todd (2005)). Essentially, we first difference the outcomes to remove the time-invariant effects and then use matching to try and control for the time-varying components that may cause selection bias. We match treated restaurants with control restaurants using both characteristics of the incumbent restaurant's location $X(L_r)$ and the restaurant's menu text M_r. We use a two-stage matching process as follows:

1. We calculate the predicted intensity of entry for each location L_r using locational variables $X(L_r)$. For each treated restaurant, this yields a subset of control restaurants with a similar likelihood of facing a new entrant.

2. We then choose the control restaurant within this subset that has a menu closest to the treated restaurant's menu.

We use the predicted entrants in essentially the same way as a propensity score. However, as discussed in detail below, this count variable is better suited to our context than a propensity score based on a simple binary entry variable. Let $\hat{P}(X(L_r))$ denote the predicted intensity of entrants at location L_r — i.e., the predicted count of new entrants near location L_r during our sample period. Further, denote the symmetric difference in a variable X from $t-d$ to $t+d$ as $\Delta X_{rt} = X_{r,t+d} - X_{r,t-d}$. Lastly, let ΔY^0_{rk} represent the differenced outcome around the treatment period k_r when there is no treatment (no entry). Then, our key identifying assumption is conditional mean independence (see Smith and Todd (2005)):

$$E[\Delta Y^0_{rk} | \hat{P}(X(L)), M_r, \Delta D_{rk} = 1] = E[\Delta Y^0_{rk} | \hat{P}(X(L)), M_r, \Delta D_{rk} = 0] \tag{6}$$

In our context, Equation 6 implies that conditional on the predicted entrants and menu text, competition within this time period is essentially randomly assigned. This allows us to use the observed outcomes of restaurants that do *not* have new competition over a specific duration as a replacement for the counterfactual outcomes of the treated restaurants, had they not received new competition.

Qualitatively, this approach relies on the fact that matched treated and control restaurants will be located in similar neighborhoods and sell similar food. Therefore, they will be subject to similar location and restaurant-level shocks. For example, city-wide trends in tastes (e.g. a fad for cupcakes or kale) may have a similar effect on the demand for restaurants selling these foods; this is captured in their menu text. On the supply side, increases in the cost of an input specific to certain types of restaurants (e.g., sushi grade tuna or the wage of sushi chefs) will impact restaurants with that cuisine on the menu. We can make an analogous argument for location. If neighborhood trends are correlated with underlying demographic and economic characteristics then by matching on these characteristics we choose control observations that experience the same trends. For example, neighborhoods with relatively low rent but well educated residents might become hip neighborhoods with many new restaurants and changes in incumbent restaurants.

Lastly, when we select a control restaurant using menu-text we are essentially using an outcome variable in the pre-treatment period to improve the match. Chabe-Ferret (2014) argues that matching with pre-treatment outcomes when selection is due to both a fixed effect and transitory shocks can lead to improperly matched observations or misalignment. The author suggests instead matching on covariates that do not vary over time. For this reason we use the earliest period menu for each restaurant, which we believe will capture the general cuisine of the restaurant but is far enough (often months) from the new competitor entry date that the menu is unlikely to include pre-treatment trends.

3.3 Two-stage matching process implementation

We base our approach on Rubin and Thomas (2000), who (in a different context) use a large set of covariates to get an initial propensity score and then match on a few highly-important covariates within narrow propensity score callipers. In our case, we match each treated restaurant with a group of control restaurants that have a predicted entrant count within a narrow band of the predicted entrant count of the treated restaurant, and then select the control restaurant with the closest menu to the treated restaurant.

3.3.1 Entrant intensity

As noted earlier, treatment assignment depends on timing and thus a given restaurant may be treated, control, or neither, for different time periods. For this reason time-invariant characteristics of a location cannot accurately predict treatment assignment and thus we do not use a propensity score for matching. However, as we show in this section, some locations have much more entry than others over our sample period and the total number of entrants is correlated with time-invariant location characteristics. Therefore, although exact treatment timing cannot be predicted by fixed location characteristics, we can use the likelihood of entry to ensure that we are comparing treated restaurants to control restaurants in similar areas. We model the total number of entrants over our entire sample period in each location using a Poisson model and then use the predicted number of entrants to balance the location covariates. Since every location has the same number of observed periods, the predicted number of entrants corresponds to the predicted intensity of nearby entry.

For each incumbent restaurant ever observed in our sample, we count the number of total entrants $P(L_r)$ observed over the sample period within $\rho_T = 500$ meters of r's location. Note that this count of entrants is a characteristic of the location and does not depend on how many periods we observed restaurant r or when it entered our sample. We then model the count of entrants as a Poisson process where the expected count depends on the characteristics of the area L_r around restaurant r, $X(L_r)$:

$$ln(E[P(L_r)|X(L_r)]) = X(L_r)'\theta \tag{7}$$

As candidates for $X(L_r)$, we assembled a large number of census tract variables from the 2009-2014 American Community Survey, "fair market rent" at the zipcode level from the department of Housing and Urban Development (HUD), and the distance to the nearest subway station. We also included the count of competitor restaurants within several different radii, calculated with the first period of data to ensure this measure wasn't correlated with our dependent variables. We then use a penalized poisson model (LASSO) to select the variables and estimate the coefficients. We describe the details of this process and show the coefficients estimates in Appendix A.7.

For each restaurant r we can now calculate the number of predicted entrants $\hat{P}(L_r)$ using our model. To form a control group for each treated restaurant, we will choose a subset of all control restaurants that have a predicted entrant count within a narrow bandwidth ("callipers") of the treated restaurant. Choosing the callipers necessarily entails a tradeoff. A narrow bandwidth will ensure close matches in the predicted entrant count, but few restaurants will have a close cosine match within their callipers. As discussed in Appendix A.3, we choose a bandwidth of 0.25 standard deviations of the logarithm of predicted entrant count. We do not estimate any treatment effects during this process and our choice of bandwidth is based on balancing covariates and uninfluenced by outcome variables. Lastly, we trim the distribution of predicted entrant counts to exclude observations with very high or very low predicted counts. Appendix A.4 describes this trimming process in further detail.

3.3.2 Cosine distance

The second stage of our matching process requires matching restaurants with similar menus. Our menu data is literally the text of a restaurant menu, with no additional structure, classification, or standardization. Each restaurant usually divides their menu into item sections (e.g., “Vegetables” or “Noodles”) and then lists each item in the section with a price. Restaurants may also include an item description (e.g., “Thin noodles. Spicy.”). For the purposes of economic research, it would be ideal if restaurants classified every one of their dishes into standardized item codes so that menus could be easily compared. Any attempts to create our own item standardization would require a myriad of arbitrary decisions, such as whether a meatball hero sandwich is the same as a meatball submarine sandwich. Instead, we follow the text processing literature in computer science to calculate a measure of the similarity between the overall text of two restaurant menus. Specifically, we use the “cosine similarity” method in Damashek (1995), which breaks the text of a document into a set of strings of consecutive characters, called “ngrams,” and then compares two documents based on the counts of their component ngrams. We describe this method in detail in Appendix A.2, but also give a brief overview below.

An ngram of size n is a text string of n consecutive characters. The phrase “with fries” has seven 4-grams including the space between words: “with”, “ith_”, “th_f”, “h_fr”, “_fri”, “frie”, and “ries”. We decompose the text of any restaurant menu into ngrams of size 3 and then count the number of occurrences of every specific ngram. For example, if we looked at the 3-gram decomposition of a barbecue restaurant menu there might be a large number of “bar” or “bbq” 3-grams. Dividing the count of any specific ngram by the total count of ngrams in the menu gives us the proportion of the menu represented by that particular ngram. For a given ngram i on menu M we denote this proportion, or weight, as x_{Mi}. Then a menu with J unique ngrams can be represented as a J-dimensional vector of the ngrams' weights $x_{Mj} \forall j \in J$, with each weight representing the relative frequency of the ngram. Once two restaurant menus have been converted into vectors in ngram space, we can then measure the difference between their menus as the angle between their ngram vectors. Damashek notes that for some applications this method can be improved if the vectors are first centered by subtracting a common vector, $\mu_j \forall j \in J$, with the ngram distribution over all documents (menus). This yields what is essentially a correlation coefficient ranging from 1, when two menus are identical, to -1, when the ngram shares of two menus are perfectly negatively correlated. Damashek describes this measure as “centered cosine similarity,” which we denote as $S^c(M,M')$ for menus M and M'. Finally, in order to make our product space metric consistent with geographic distance we subtract S^c from 1 and call the resulting measure “cosine distance,” $\omega(M,M')$, which ranges from 0, when there is no distance between products, to 2, indicating the maximum distance between products:

$$\omega\left(M,M'\right) = 1 - \frac{\sum_{j=1}^{J}(x_{Mj}-\mu_j)(x_{M'j}-\mu_j)}{\left(\sum_{j=1}^{J}(x_{Mj}-\mu_j)^2\sum_{j=1}^{J}(x_{M'j}-\mu_j)^2\right)^{1/2}} = 1 - S^c\left(M,M'\right) \tag{8}$$

Several previous papers have used similar measures for pairwise comparisons of differentiated products. Jaffe (1986) defines the technological position of a firm as a vector of the distribution of its patents over 49 classes and then uses the angle between two of these vectors to measure changes in technological position. Similarly, Sweeting (2010) measures differentiation between radio stations as the angle between vectors of airplay for music artists and Chisholm et al. (2010) measure differentation between first-run theaters as the angle between vectors of movie screenings. Most similar to our application is a recent paper by Hoberg and Phillips (2016) that measures product differentiation for large firms using the angle between vectors of

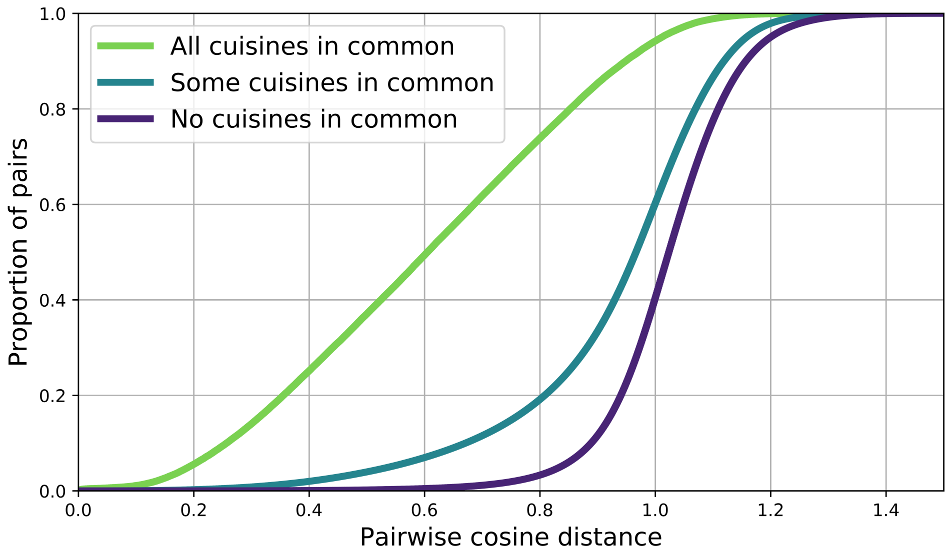


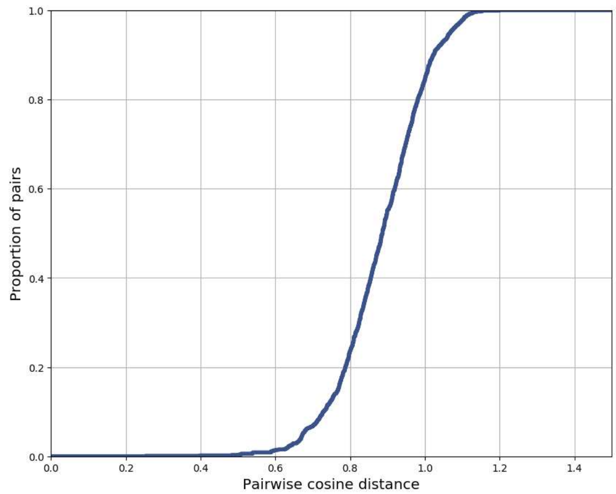
Figure 6: Cumulative distribution function of cosine distance between pairs of restaurants that share all cuisines, some (but not all) cuisines, and no cuisines.

certain key nouns in 10-K forms filed with the SEC. While this previous work demonstrates the effectiveness of cosine similarity in other contexts, it is not clear that ngrams are a good representation of products, nor that the angle between two restaurants' centered vectors of potentially thousands of ngrams provides any information about the similarity of their menus. Therefore we now present some results validating this measure and then at the end of this section describe how we use cosine distance in matching treated and control restaurants.

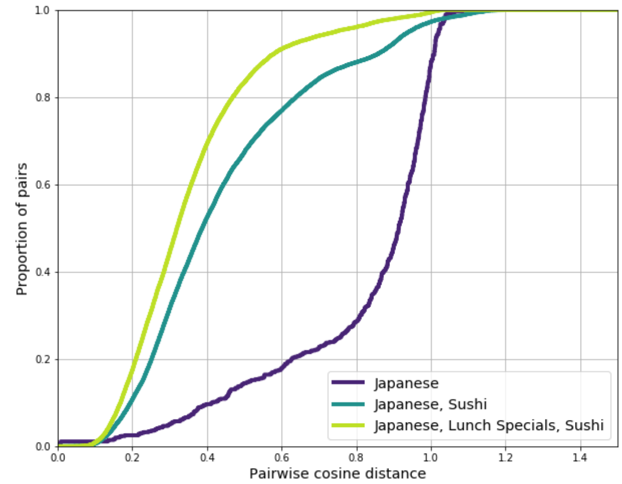
In our data the site assigns one or more cuisine categories to each restaurant in the sample; if cosine distance is a salient measure of cuisine then two restaurants with similar cuisines should have a closer cosine distance. As shown in Figure 6, the distribution of pairwise cosine distances between restaurants with identical cuisine sets first-order stochastic dominates the distribution of restaurants that share at least one, but not all, cuisines. Moreover, the distribution of pairwise cosine distance between restaurants that share at least one cuisine first-order stochastic dominates the distribution of pairwise cosine distances between restaurants that share no cuisines. Pairs of restaurants with a small cosine distance are particularly likely to share all cuisine categories. For example, the plot shows that roughly 75% of all restaurant pairs with the same cuisines have a cosine distance less than 0.8, compared to 20% of pairs sharing some cuisines, and only about 5% of pairs with no cuisines in common.

The cosine metric can also provide additional information beyond the cuisine categories of the online delivery service. Many of the cuisine categories are very broad and two restaurants with the same sole listed cuisine may not have particularly similar menus. For example, Bella Pizza (which serves items including "10 Piece Chicken Buffalo Chicken Wings"), Genuine (which serves items including "Fries with Turkey Chili and Queso Fresco"), and WINE 34 (which serves items including "Acorn Squash Ravioli") are all listed with "American" as their sole cuisine category. Figure 7a shows the distribution of cosine distances between pairs of restaurants with "American" as their sole listed cuisine. As shown, many of these pairs of restaurants have large cosine distances between their menus.

Conversely, Figure 7b shows the distribution of cosine distances between pairs of restaurants with successively more narrowly defined cuisine combinations: "Japanese", "Japanese" and "Sushi", and "Japanese",



(a) Cumulative distribution function of cosine distance between restaurants with the cuisine "American".



(b) Cumulative distribution function of cosine distance between pairs of restaurants of three cuisine combinations: "Japanese", "Japanese" and "Sushi", and "Japanese", "Sushi", and "Lunch Specials".

Figure 7: Cumulative distribution functions for restaurantsin selected cuisines.

"Sushi", and "Lunch Specials"[15]. As the set of cuisines becomes more specific and the restaurants with the set of cuisines become more similar, the cosine distance between pairs of restaurants within the cuisine set decreases.

Lastly, to obtain our matched regression sample, we match each restaurant r treated at period t with the control restaurant c_{rt} with the minimal nonzero cosine distance. We consider only potential control restaurants within the predicted entrant intensity callipers described above. We trim the sample to include only treated restaurants with reasonably close control matches; specifically, we only include matched pairs of treated and control restaurants in our regression sample if the cosine distance $\omega(M_r, M_{c_{rt}})$ is within the lowest 5% of pairwise cosine distances between all restaurants in the sample.

3.4 Testing match quality

In our results section we will present spatial competition results for three durations ($d = 4, 6, 8$) using two different dimensions to define space (physical and characteristics), as well as results on exit likelihood. Rather than showing separate balance tables for all of these analyses (7 tables), we instead present more general results showing the sample balance for matched restaurants across the distribution of the count of nearby entrants during the sample period[16]. These results demonstrate that treated and control restaurants (which by construction have different entrant counts over the defined duration) are balanced on observables for different durations. To do so, we group restaurants into quintiles of observed entrant count. Then, we compare the covariates for observations in a specific quintile to observations in all other quintiles before and after matching. Since we use a two-stage matching process, we first show the balance improvement from matching on entrant intensity and then show the additional effect of using cosine distance relative to matching on entrant intensity alone.

[15] All of these restaurants are also in the "Asian" cuisine category; this is suppressed in Figure 7b for legibility.

[16] All of our tests on balance and our matched sample regression results use an inner radius of $\rho_T = 500$ meters to define entry near a given restaurant as discussed in Section 3.1. Separate post-match balance tables for any particular analysis and duration are available upon request.

3.4.1 Testing entrant intensity balance

We follow the general procedure of Hirano and Imbens (2004) by grouping restaurants into quintiles of observed entrant counts; for example, the first quintile consists of locations that have two or fewer nearby entrants. We wish to compare the average value of each location covariate for locations with up to two entrants (quintile 1) to locations with more than two entrants (quintiles 2-5). As recommended in Imbens (2015), we compare covariates using normalized differences. Our approach proceeds as follows:

1. Divide restaurants into quintiles according to the number of nearby entrants over the sample period. Let R_q be the set of restaurants in quintile q and let R_{-q} be the set of restaurants not in R_q.

2. For each quintile q for each restaurant $r \in R_q$ define a candidate set $C(r)$. This is the intersection of R_{-q} and the set of observations lying within the propensity calliper of r — i.e., the observations with a log predicted entrant count within 0.25 standard deviations of the log predicted entry count for r.

3. For each quintile q for each observation $r \in R_q$ randomly sample (with replacement) one thousand observations from $C(r)$. Index these bootstrap draws by b. For each r denote the corresponding bootstrap observation by $s^b(r)$.

4. For each bootstrap iteration b for each locational variable $X_j(L) \in X(L)$ calculate the following absolute normalized difference across all restaurants r and their randomly-selected matches $s^b(r)$:

$$v_{qj}^b = \frac{\left|\text{mean}_{r\in R_q}\left(X_j(L_r)\right) - \text{mean}_{r\in R_q}\left(X_j(L_{s^b(r)})\right)\right|}{\frac{1}{2}\sqrt{\text{var}_{r\in R_q}\left(X_j(L_r)\right) + \text{var}_{r\in R_q}\left(X_j(L_{s^b(r)})\right)}} \tag{9}$$

5. Take the average over values of v_{qj}^b across all bootstrap iterations b.

Table A7 compares the resulting normalized differences to the normalized differences obtained without callipers — that is, by randomly sampling from R_{-q} rather than $C(r)$ in step 3. Imbens (2015) suggests 0.2 as a reasonable threshold for the normalized difference. With the callipers nearly all covariates fall below this level. Although the normalized distances are generally lower than in the pre-callipers sample, some age brackets and housing characteristics still differ across quintiles.

3.4.2 Testing cosine distance balance

In the second stage of the matching process, we match each treated restaurant to the within-calliper control restaurant with a menu at the smallest cosine distance. As discussed in Section 3.2, this is intended to produce matched pairs of treated and control observations which would have a similar response to competition.

In order to measure the similarity of the matched pairs, we compare the normalized differences between menu attributes of the treated and control matched pairs with the normalized differences between menu attributes of a counterfactual set of treated and control pairs. We generate this counterfactual set by randomly selecting a control restaurant within the propensity callipers for each treated restaurant. The comparison isolates the improvement in menu similarity using the nearest-neighbor cosine match from that already achieved by matching on predicted entrants. Table 5 compares the menu similarity of the set of matched pairs with the menu similarity of the counterfactual set for each quintile of the nearby entrant count. We report the normalized differences for several menu attributes, as well as three measures of similarity in cuisine

Table 5: Balance of menu and restaurant characteristics

Variable		Q1	Q2	Q3	Q4	Q5
Median price	Before matching	0.24	0.17	0.13	0.17	0.14
	After matching	0.02	0.32	0.17	0.04	0.09
95th perc price	Before matching	0.24	0.16	0.17	0.19	0.17
	After matching	0.11	0.18	0.01	0.13	0.05
Item count	Before matching	0.17	0.16	0.18	0.24	0.19
	After matching	0.15	0.18	0.27	0.37	0.30
Quality	Before matching	0.22	0.14	0.19	0.14	0.18
	After matching	0.03	0.12	0.17	0.11	0.18
Timeliness	Before matching	0.20	0.16	0.20	0.17	0.18
	After matching	0.13	0.11	0.07	0.01	0.04
Accuracy	Before matching	0.18	0.13	0.21	0.16	0.16
	After matching	0.00	0.09	0.17	0.17	0.13
Cuisines Jaccard	Before matching	0.91	0.92	0.92	0.93	0.93
	After matching	0.62	0.58	0.63	0.66	0.72
Cuisines equal	Before matching	0.01	0.00	0.01	0.01	0.01
	After matching	0.05	0.14	0.10	0.07	0.07
Cuisines subset	Before matching	0.10	0.07	0.06	0.05	0.06
	After matching	0.49	0.52	0.41	0.32	0.28

Normalized differences for randomly-selected within-calliper control matches compared to matched treated and control pairs. Unmatched values are the average over one hundred repetitions of random selections.

categories: the Jaccard distance[17], an indicator for whether cuisine sets are identical, and an indicator for whether one cuisine set is a subset of the other.

As shown, cosine matching yields improved pairs compared to randomly selected restaurants within the propensity callipers of the treated restaurants. For most menu and restaurant attributes ("median price" through "accuracy"), the normalized differences are significantly smaller for the matched set. An exception is item count; for this variable our matching does not decrease differences across quintiles and in some quintiles the differences are slightly larger after matching. However, general menu lengths tend to be a fixed characteristic of a restaurant—for example, delis tend to have very large item counts—and therefore we expect that much of this difference will be absorbed by restaurant fixed effects in our analyses (see the item count event study in Figure 8 for an example). The last three rows of Table 5 show that the cuisines of matched restaurants are much closer; the Jaccard distance is smaller and a greater proportion have identical cuisines or some overlapping cuisines.

4 Results

We present a series of results on the response to competition by incumbent restaurants. We focus on four dependent variables to understand the price and variety response to competition: the median item price, the 95th percentile item price, the number of menu items, and mean price change at the item level (described in detail below). We start with our main results showing the response to competition from an entrant locating within 500 meters of an existing restaurant. We then run a number of robustness checks examining different outcomes, durations, and heterogeneity. Next, in an extension we show results for the response to competition in *characteristics* space by defining treatment as an entrant whose menu is within a maximum cosine distance to the menu of an incumbent restaurant. In a further extension we examine the location choices of

[17] The Jaccard distance between two sets A and B is defined as $1 - \frac{|A \cap B|}{|A \cup B|}$.

entrants using a Monte-Carlo simulation. Finally, in Section 5 we investigate the effect of competition on the likelihood an incumbent restaurant exits the market.

4.1 Main Results: Spatial Competition in Physical Space

We use primarily two fixed effect specifications to examine the response to competition: a restaurant-level specification and an item-level specification. In the restaurant level specification we compare matched treated and control restaurants over the exact same periods, before and after treatment:

$$Y_{r,t} = \beta_1 * post_{rt} + \beta_2 * (post_{rt} \times D_{rt}) + \beta_3 * open_{rt} + \eta_h + \eta_r + \varepsilon_{r,t} \tag{10}$$

In the above specification, Y_{rt} is an outcome for restaurant r in period t, $post_{rt}$ is a post-treatment period indicator, and the $post_{rt} \times D_{rt}$ captures the post-treatment effect for treated restaurants, our main variable of interest. The treated-control pairs are matched exactly across pre and post-treatment periods so that for any pre-post window $(-w, w)$ there are four observations: the treated restaurant w periods before and after treatment, and the matched control restaurant w periods before and after treatment. For this reason, we do not include time period fixed effects. However, in order to deal with the potential noise created by time of day effects (see earlier discussion in section 2.1), we also include an indicator for open status, $open_{rt}$, and hour fixed effects for the hour of the day we observed the menu, η_h. The η_r term is a restaurant fixed effect[18]. Following the framework of Abadie, Athey, Imbens and Wooldridge (2017), we note that treatment status is assigned to a cluster of restaurants based on a common entrant, and therefore calculate standard errors clustered at the level of the entrant generating the treated status, throughout our results section.

While we believe the fixed effects in the above specification capture much of the time-of-day noise, we also run an item-level specification that, for each restaurant, compares the prices of the same set of menu items, before and after treatment. For each item, the comparison is again symmetric: we only include the item w periods before treatment if we also observe it w periods after treatment. The specification is similar to the restaurant-level equation above but without the time-of-day fixed effects:

$$ItemPrice_{i,r,t} = \delta_1 * post_{rt} + \delta_2 * (post_{rt} \times D_{rt}) + \eta_r + \varepsilon_{r,t} \tag{11}$$

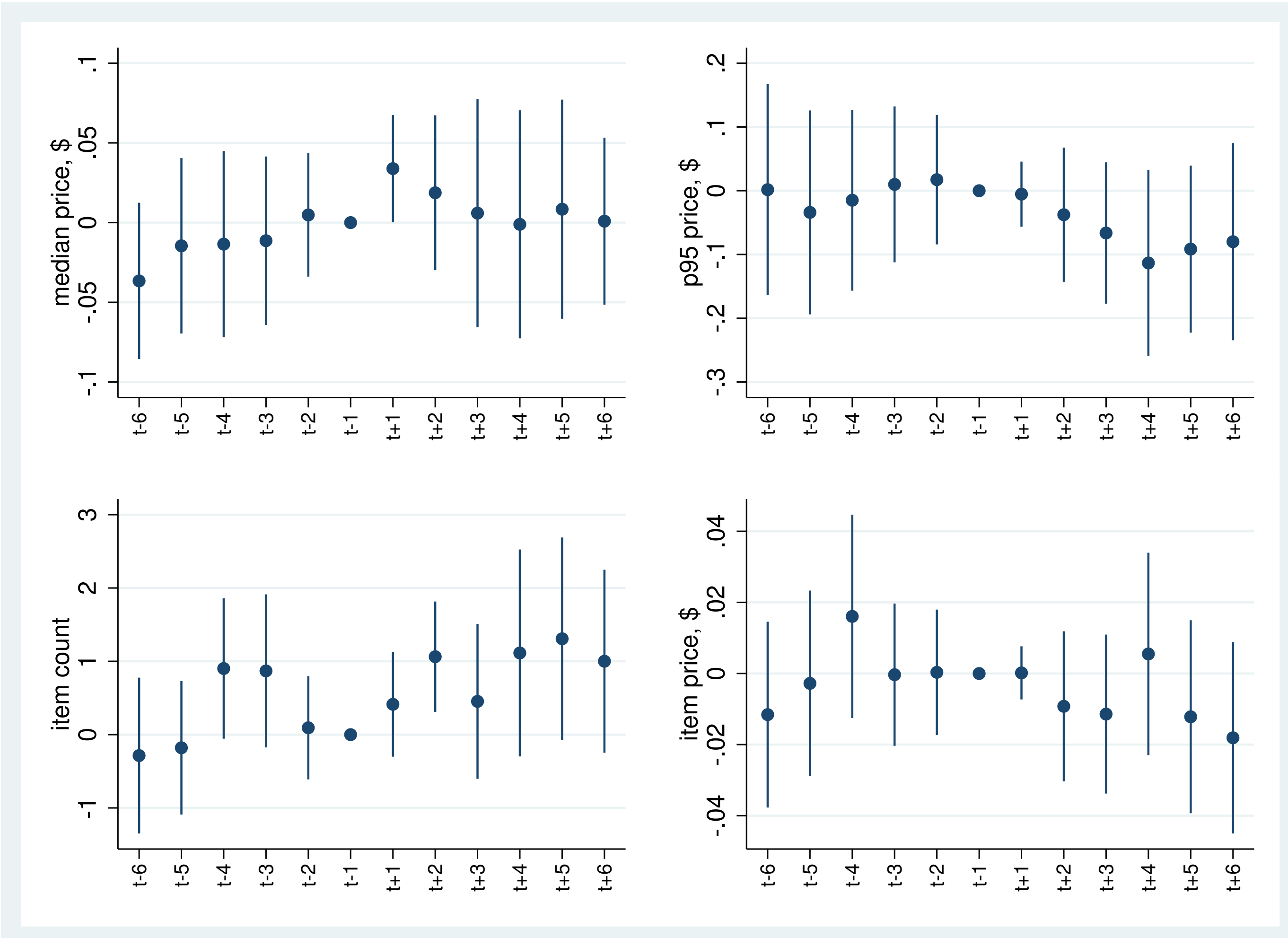
Importantly, while restaurants are still matched as in the restaurant-level specification, restaurant *items* are not matched across treated and control restaurants. Since restaurants vary widely in item counts, we weight specification 11 by the inverse of the item count so that δ_2 can be interpreted as the change in the average item price, for the average restaurant. The advantage of this specification over the restaurant-level specification is that price changes are computed from a constant set of items, and thus unaffected by item availability that differs by time of day. However, this makes δ_2 an estimate of the intensive margin change only, while the restaurant-level estimate, β_2, reflects changes in both the intensive and extensive margins (items added or deleted).

We first present "event study" plots of the two specifications for the 6 week duration, which provides a balance between sample size and time range[19]. Figure 8 shows the estimated coefficients for our three

[18] We refer to matched treated and control restaurants over the comparison period, $[-d, d]$, as a "comparison pair." Each one of these restaurants could be treated or control over a different period, and the control restaurant could serve as a control for a different treated restaurant in the same time period. To ensure that our fixed effects are unique to each restaurant in each comparison pair, the restaurant fixed effect is actually an indicator for a restaurant X comparison pair. If the restaurant is only used in one comparison pair then this fixed effect reduces to a simple restaurant fixed effect, and so we use the term "restaurant fixed effect" for simplicity.

[19] Letting k_r indicate the treatment period for restaurant r, the restaurant-level event study specification is: $Y_{r,t} = \sum_{j=-d}^{-1} \beta_j * \mathbf{1}(j =$

Figure 8: Event study plots for six week duration sample



main restaurant-level variables and the item-level price specification, in the lower right-hand corner. These plots show little evidence of pre-trends or a post-treatment response, with the possible exception of a small decrease in the 95 percentile price. As we will emphasize throughout the paper, the point estimates and confidence intervals are quite small. The $t+6$ point estimate for the 95th percentile price variable ($\beta_6 = -0.1$) is a 0.5% change for the average restaurant and the corresponding item count estimate ($\beta_6 = 1$) is a 0.7% change.

In Table 6 we present the regression results from our two specifications for the three durations. Across the twelve regressions, the post-treatment effect for the treated is small and statistically insignificant, with the exception of item count for the four period sample, which is significant at the 10% level. Further, the magnitude of the treatment effects are small even compared to the "post" coefficients, which capture the average change in the outcome for all restaurants over d periods. For example, the 95% confidence interval for the treatment effect on median item price in the d=6 sample is $[-0.022, 0.071]$. Given that the average increase in median price is 0.046, the bounds of the treatment effect are only about 1.5 times the magnitude of normal price inflation. In the fourth column of each subtable we present the results from the item-level

$t - k_r) + \sum_{j=1}^{d} \beta_j * \mathbf{1}(j = t - k_r) + \eta_t + \eta_r + \varepsilon_{r,t}$. We normalize β_{-1} to zero. We include period fixed effects since the periods are unbalanced across specific treatment lags and forwards. The item-level specification is the same, except we again weight by inverse item count.

specification, and find very small treatment effects with tight 95% confidence intervals, while the average changes ("post" coefficients) are quite similar to the median price estimates in column 1. In column two we show the treatment effects for the 95th percentile item price and also find no evidence that restaurants are changing prices at the upper end of their menus. The treatment effects for item count, column three of each table, are all positive but also quite small, with no point estimate larger than 0.5% of the average item count. The open status coefficients are positive and significant, illustrating that menus are about 1.7 items longer when restaurants are open. Lastly, comparing the dependent variable means across the different subtables provides evidence of heterogeneity across the samples. This heterogeneity is not surprising. As discussed earlier, restaurant characteristics differ across areas with higher or lower entry frequencies. Since a restaurant in the d=8 sample must have no entry nearby over 32 weeks, the entry frequency rates are different for this sample in comparison with the shorter duration samples. Of course, within each sample, treated and control restaurants are matched and have similar characteristics.

4.2 Robustness

In this section we explore other ways in which restaurants could be responding to competition that might not be apparent in the specifications tested in the previous section. We first examine a set of other outcomes, next explore different response durations, and then examine response heterogeneity.

In Appendix table A3 we run our main restaurant-level specification on the following set of non-menu variables that Grubhub provides to consumers describing each restaurant: quality ratings, hours of operation, listed cuisines, and count of reviews[20]. We find a statistically significant decrease in the quality of order fulfillment (column 3) for the four period duration, but the magnitude is tiny—0.09 from a mean of 90.8—and unlikely to be economically meaningful. We also find no change in the weekly hours of operation nor in the number of cuisines the restaurants lists. Lastly, in column 6 we look at the count of reviews, which increases each week and might be interpreted as a very noisy proxy for sales. Interestingly, we find a statistically significant decrease of 3.6 in the growth of reviews for treated restaurants for the four period duration. If we just compare the change in review counts from four periods before treatment to four periods after treatment (a "long difference"), then control restaurants have 53.5 additional reviews and treated restaurants have 46 additional reviews, about a 14% decline. However, we find no evidence of a change in review count growth in the other two durations and therefore it's rather unclear whether this single coefficient indicates a decrease in sales volume resulting from new competition.

An obvious concern with our analysis thus far is that incumbent restaurants may only respond to new competition after longer periods than we have tested. To assess this concern we first run a long difference version of our specification comparing the change in outcomes from $t-d$ to $t+d$ only (just two periods). This range removes the effect of early post-treatment periods and is also more robust to any anticipatory reactions to new competition, although the pre-treatment coefficients shown in Figure 8 provide no evidence of this. The results from this analysis are similar to those presented in Table 6 and so we omit them for brevity (available upon request). Next, we try re-running our analysis shifting the definition of pre-treatment and post-treatment periods forward by d periods, so that the pre period is $[0, d-1]$ and the post period is $[d+1, 2d]$ (actual entry still occurs between periods -1 and 0). The idea behind this analysis is that if we are not finding any effects in Table 6 because restaurants do not respond in the first d post-entry periods—for example, incumbent restaurants may conduct business as usual while waiting to see how successful is

[20]Other studies find that retail firms in other industries respond to competitive intensity by improving service quality. Auto dealerships carry more inventory (Olivares and Cachon 2009) and supermarkets reduce their inventory shortfalls (Matsa 2011) when competition increases. Longer hours also constitute a form of improved service quality for retail businesses including gas stations (Kügler and Weiss 2016) and outlets of fast-food restaurant chains (Xie 2018) where other forms of differentiation may be infeasible.

Table 6: Fixed effect results for physical distance treatment. The fourth column shows results from an item-level regression. All specifications include restaurant fixed effects, standard errors clustered by entrant are shown in parentheses. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Itm Prc
treated X post	0.006	-0.025	0.448*	-0.007
	(0.016)	(0.060)	(0.264)	(0.005)
post	0.027**	0.074	-0.004	0.030***
	(0.011)	(0.056)	(0.135)	(0.004)
open	-0.030***	-0.012	1.660***	
	(0.010)	(0.019)	(0.199)	
Observations	19016	19016	19016	3383522
Clusters	285	285	285	311
Treated	1668	1668	1668	1811
DepVarMean	8.40	17.95	148.57	8.69

(a) Four period duration

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Itm Prc
treated X post	0.025	-0.065	0.703	-0.009
	(0.024)	(0.081)	(0.491)	(0.007)
post	0.046***	0.140*	0.196	0.048***
	(0.016)	(0.073)	(0.261)	(0.006)
open	-0.031**	-0.014	1.824***	
	(0.015)	(0.028)	(0.317)	
Observations	12815	12815	12815	2328974
Clusters	222	222	222	224
Treated	922	922	922	933
DepVarMean	8.19	17.80	154.06	8.58

(b) Six period duration

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Itm Prc
treated X post	0.018	-0.038	0.560	0.000
	(0.023)	(0.098)	(0.578)	(0.010)
post	0.047***	0.095	0.057	0.048***
	(0.016)	(0.075)	(0.287)	(0.008)
open	-0.017	0.044	1.780***	
	(0.015)	(0.048)	(0.424)	
Observations	8116	8116	8116	1462892
Clusters	148	148	148	150
Treated	498	498	498	502
DepVarMean	8.11	17.19	158.69	8.53

(c) Eight period duration

the new entrant—then those first d post-entry periods are actually valid control periods. Further, since our definition of treated and control requires no entry in the $[0,2d]$ periods, we can use the $[d+1,2d]$ range as post-treatment periods without worrying about the effect of additional entrants.

We present the results of this shifted analysis in Table 7. Overall the results are fairly close to those using the original duration in Table 6 and the similarity of the coefficients on "post" suggest that we are capturing consistent changes restaurants make to their menus in the absence of any competitive effects. However, we again find a statistically significant change at the 10 percent level for item count in the four period duration, with a similar magnitude to before. We also now find a statistically significant post-treatment coefficient for the item-level specification in the eight period sample. The coefficient is quite small: a three cent increase on an average item price of \$8.6, and only a little larger than the general increase in item prices of \$0.025. A positive treatment effect is counter to expectations from a spatial competition model, which would suggest that restaurants charging prices above marginal cost would have to cut prices in response to new competition. However, since we only find this effect when looking at more than eight periods after treatment, it's unclear whether this finding results from using a sufficiently long duration or whether it's due to something idiosyncratic in the eight period sample, which has the fewest number of unique restaurants.

To explore this question we compare the six period sample and the eight period sample using overlapping durations. Specifically, we define the pre-treatment period as $[4,7]$ and the post-treatment period as $[9,12]$, which is the maximum symmetric overlap between the two duration samples. In columns 1 and 2 of Appendix Table A4 we show the results for this range for the six period and eight period samples, respectively. The post treatment coefficient for the six period sample is small and statistically insignificant while the eight period sample is similar to the results shown in Table 7. These results suggest that the earlier significant eight period treatment coefficient was not due to the extended periods tested, but rather might be something specific to that particular sample.

As noted earlier, there is significant heterogeneity across the duration samples due to the different required lengths in which there can be no entry. Perhaps one of the most important ways in which these samples differ is the number of other incumbent competitors around each restaurant: within 500 meters there are on average 27.4 other competitors around each restaurant in the four period sample, 20.5 in the six periods sample, and 13.6 in the eight period sample. It's possible that the response to competition depends on the number of existing competitors and that this heterogeneity could partly explain some of the inconsistencies across different samples, such as the item count effect for the four period sample. As a last robustness check, we add an interaction between the number of incumbent competitors within 500 meters (observed in the earliest period in our data for each restaurant) and the treated X post indicator. We run this new specification on the four period sample, which includes all the treated restaurants from the longer durations, and has the most heterogeneity in the number of nearby competitors. We show the results in the final four columns of Appendix Table A4. The new interaction term is small and statistically insignificant across all dependent variables, thus providing no evidence of heterogeneity by competitive environment[21].

4.3 Extension: Spatial Competition in Characteristics Space

The previous section suggests that restaurants do not react when confronted with a nearby entrant. While this provides evidence against the spatial competition model, a natural concern is that restaurants may only compete with competitors selling similar products, and thus the relevant dimension for spatial competition is not physical distance but rather distance in characteristics space. In this extension we re-run our main analysis using menu distance to define treatment. For a given incumbent restaurant, we define treatment

[21]The coefficient on the treated X post variable is statistically significant in column 6 but has no interpretation given the insignificance of the interaction term.

Table 7: Fixed effect results using extended durations. The fourth column shows results from an item-level regression. All specifications include restaurant fixed effects, standard errors clustered by entrant are shown in parentheses. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Itm Prc
treated X post	-0.002	-0.108	0.523*	0.008
	(0.013)	(0.100)	(0.302)	(0.007)
post	0.022***	0.171*	0.141	0.021***
	(0.008)	(0.091)	(0.180)	(0.003)
open	-0.004	0.012	2.000***	
	(0.011)	(0.029)	(0.264)	
Observations	14931	14931	14931	2596554
Clusters	263	263	263	278
Treated	1472	1472	1472	1600
DepVarMean	8.38	17.87	148.87	8.68

(a) Four period duration: pre [0,3], post [5,8]

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Itm Prc
treated X post	-0.016	0.057	0.285	0.012
	(0.019)	(0.068)	(0.349)	(0.019)
post	0.048***	0.079***	0.150	0.033***
	(0.012)	(0.020)	(0.234)	(0.006)
open	-0.045***	-0.049	1.543***	
	(0.013)	(0.053)	(0.331)	
Observations	9208	9208	9208	1648274
Clusters	193	193	193	211
Treated	739	739	739	861
DepVarMean	8.23	17.49	156.90	8.58

(b) Six period duration: pre [0,5], post [7,12]

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Itm Prc
treated X post	0.018	0.169	0.426	0.031**
	(0.030)	(0.183)	(0.441)	(0.012)
post	0.028***	0.023	0.653**	0.025***
	(0.009)	(0.025)	(0.259)	(0.004)
open	-0.026**	-0.044	1.607***	
	(0.012)	(0.040)	(0.363)	
Observations	7743	7743	7743	1341446
Clusters	140	140	140	142
Treated	470	470	470	488
DepVarMean	8.16	17.30	159.98	8.59

(c) Eight period duration: pre [0,7], post [9,16] as a new entrant on Grubhub within 1.5km, where the menu distance between the incumbent and entrant is less than the 2nd percentile of all pairwise menu distances observed in our data. These are restaurants with very similar menus and often all of the same cuisines. We use entry on Grubhub, rather than actual entry into the New York City market as before, for both conceptual and practical reasons. If competition is in characteristics space, then consumers are choosing among restaurants with similar cuisines over physical distances that are likely significantly larger than the 500m tested earlier. When a restaurant joins Grubhub, it will then be competing with similar restaurants that deliver to the same locations, which we approximate as within 1.5km[22]. Thus, even if a restaurant has already been in the market for a while, when that restaurant joins Grubhub it represents new competition to restaurants already on the platform. From a practical standpoint, we are only able to match about 40% of our main entrant sample (that shown in Figure 2) to Grubhub menus. Therefore if we only used this data source to define treatment by menu distance, we might misclassify treated and control restaurants since we cannot calculate entrant-incumbent menu distances for 60% of entrants.

We define treated and control restaurants for a given duration using our existing scheme (see Figure 3). Analogous to the distance buffer of 600m, in this analysis we use a menu distance buffer equal to the 5th percentile of all pairwise menu distances. Thus, a treated restaurant has exactly one entrant within the 2nd menu distance percentile and no other entrants within the 5th menu distance percentile over $2 * d$ weeks; a control restaurant has no entrants within the 5th menu distance percentile over the same $2 * d$ weeks. Lastly, we ignore Grubhub entrants whose menu distance to incumbents is less than the 0.1th percentile, as these are usually different branches of the same local franchise.

Since this analysis examines the importance of menu distance, we reverse the two steps of the matching procedure by first defining calipers in menu distance and then choosing the control with the most similar count of predicted entrants. We use the 2nd percentile of menu distances as the caliper size and then require that matched treated control pairs have a predicted entrant count within the same bandwidth as before (0.25 standard deviations of the logarithm of predicted entrant count). Thus treated and control pairs have very close menus and similar demographic characteristics.

We present the results of this analysis in Table 8, using the same format as earlier. In comparison with the physical space treatment in Table 6, there are more entrants (shown in "Clusters" row) but fewer treated restaurants per entrant. The precision of the estimates is roughly comparable in both tables (standard error size), as are the coefficients on the "post" terms, again showing a consistent estimate of the general changes all restaurants make to their menus. Across all twelve specifications we only find a significant post-treatment effect, at the 10% level, for median price in the eight period duration. Our estimate implies that treated restaurants raise their median item price by 11.4 cents while control restaurants raises prices by 4.6 cents over the same period; the mean value for this variable is $8.6. However, this effect does not show up in the item-level specification for the same sample. Further, with the large number of specifications we have tested it's quite possible to find a statistically significant coefficient due to noise alone. Therefore, while we cannot rule out that this small and weakly significant coefficient represents a real, if counter-intuitive, competitive response, we think it is more likely to be the result of noise.

[22]The website actually allows each restaurant to choose different delivery zones, and even charge different delivery fees based on the customer's location, see discussion from Grubhub programmers on Quora (https://www.quora.com/How-does-Grubhub-limit-the-delivery-area-of-a-restaurant-By-zipcode-radius-or-polygon-system) and on the Grubhub site page for restaurants(https://learn.grubhub.com/archives/basics/updating-delivery-boundary). We noticed that most restaurants were willing to deliver to locations within one mile, and thus chose 1.5 kilometers as a conservative distance within which all delivery restaurants should compete.

Table 8: Fixed effect results for cosine distance treatment. The fourth column shows results from an item-level regression. All specifications include restaurant fixed effects, standard errors clustered by entrant are shown in parentheses. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

	(1)	(2)	(3)	(4)
	Med Prc	p95 Prc	Itm Ct	Itm Prc
treated X post	0.003	-0.174	-0.003	0.017
	(0.019)	(0.167)	(0.413)	(0.012)
post	0.036***	0.010	0.031	0.024***
	(0.013)	(0.046)	(0.320)	(0.005)
open	0.039**	-0.318*	2.692***	
	(0.017)	(0.189)	(0.479)	
Observations	8446	8446	8446	1663222
Clusters	343	343	343	444
Treated	700	700	700	910
DepVarMean	8.76	19.22	162.16	9.10

(a) Four period duration (menu distance treatment)

	(1)	(2)	(3)	(4)
	Med Prc	p95 Prc	Itm Ct	Itm Prc
treated X post	0.033	0.078	-0.098	0.024
	(0.024)	(0.157)	(0.433)	(0.016)
post	0.044***	0.168	0.386	0.030***
	(0.016)	(0.126)	(0.283)	(0.005)
open	0.009	-0.055	2.965***	
	(0.017)	(0.052)	(0.576)	
Observations	8485	8485	8485	1759042
Clusters	347	347	347	348
Treated	679	679	679	682
DepVarMean	8.52	18.83	166.95	9.01

(b) Six period duration (menu distance treatment)

	(1)	(2)	(3)	(4)
	Med Prc	p95 Prc	Itm Ct	Itm Prc
treated X post	0.068*	-0.083	0.463	0.018
	(0.039)	(0.263)	(0.627)	(0.013)
post	0.046**	0.344	0.300	0.043***
	(0.023)	(0.234)	(0.507)	(0.006)
open	0.007	0.006	2.657***	
	(0.025)	(0.096)	(0.495)	
Observations	7405	7405	7405	1639322
Clusters	237	237	237	237
Treated	419	419	419	419
DepVarMean	8.59	18.81	180.54	9.03

(c) Eight period duration (menu distance treatment)

4.4 Extension: Location choice analysis

One possible explanation for our finding of no competitive response is that new entrants strategically choose locations to limit potential competition. As documented by Mazzeo (2002), Freedman and Kosová (2012), and others, firms in many industries enter the market with a product differentiated from their spatially proximate competitors in order to lessen competitive intensity. However, in the context of restaurants in New York City it may be difficult for new entrants to choose locations so precisely. Location options are limited; 2017 retail vacancy rates for the five boroughs range from 2.9% to 4.1% (Marcus & Millichap 2017). Moreover, the high density of restaurants would pose difficulties to an entrant trying to avoid nearby competition; the median entrant has 28 incumbent competitors within 500 meters. It's also possible that entrants may actually prefer to locate near similar incumbents to facilitate shoppers' desire to shop among similar businesses (Fischer and Harrington Jr 1996, Konishi 2005), because the presence of similar incumbents indicates existing demand (Toivanen and Waterson 2005), or because consumers prefer access to several nearby firms with similar product offerings when making consumption decisions (Cosman 2017).

To better understand the entrant location decision, we use a Monte Carlo exercise to compare the similarity between entrants' menus and those of nearby restaurants with the similarity from a set of counterfactual location choices. Specifically, we compare the observed distribution of menu cosine distance between entrant restaurants and incumbent neighbors (within 500 meters) to a counterfactual distribution generated by repeatedly reshuffling entrants in the $d = 8$ regression sample between observed entrant locations. That is, on each iteration, we randomly reassign entrants among the set of observed entry locations according to a uniform distribution and without replacement. If entrants were strategically locating to soften local competitive intensity, the observed distribution would feature fewer incumbent neighbors at small menu cosine distance than the counterfactual distribution. Restaurant location choices are constrained by many factors (zoning laws, vacancies, availability of suitable space) and therefore limiting the random reassignment to the set of observed entrant locations helps to generate plausible counterfactuals.

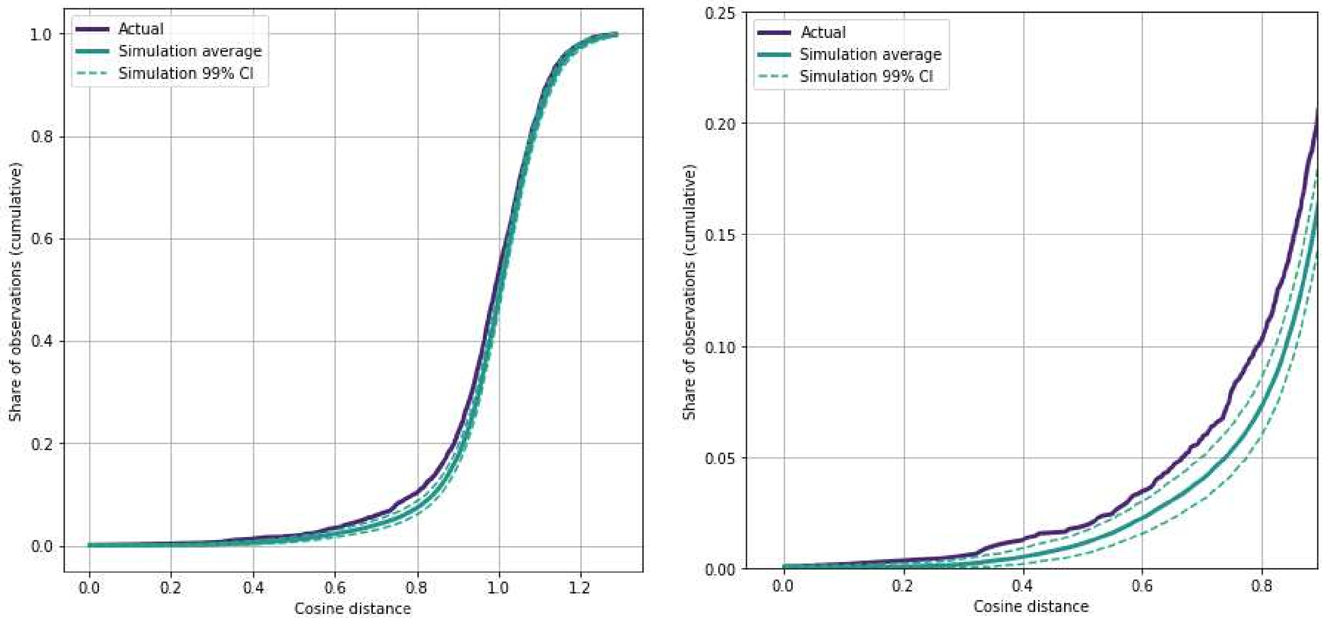
Figure 9 shows results generated by randomly reshuffling entrants between the observed entrant locations ten thousand times. As shown, the observed distribution of menu cosine distance between entrants and incumbent neighbors is actually concentrated at closer cosine distances. The tenth percentile of cosine distances in the observed distribution is 0.794 whereas the 99% confidence interval across the bootstrap repetitions is $[0.823, 0.858]$. A Kolmogorov-Smirnov test strongly rejects the null hypothesis of identical distribution. This suggests that contrary to the hypothesis of choosing locations to soften competitive intensity, similar restaurants are more likely to co-locate.

5 Effect of Entry on Incumbent Exit

Although restaurants may not change their menus in response to competition, this does not imply that there is no effect of competition. We now examine whether a nearby entrant affects the likelihood of an incumbent restaurant exiting the market. We cannot infer a market exit date using New York City inspections and Yelp reviews because inspections are infrequent (often annual) once a restaurant has opened. However, we do observe if a restaurant leaves the online delivery site, which is likely correlated with market exit. We define the exit date of a restaurant as the first week in which a restaurant is absent from our data and never reappears.

In the previous sections we defined treated and control using specific durations. A feature of this definition is that the same restaurant could be both treated and control over different time periods, allowing us to identify the short-run response to specific entrants using this timing. This definition of treatment is no longer appropriate for examining market exit because a restaurant can only exit the market once and thus, unlike changing a menu, is unlikely to exit within a short post-treatment duration. Relatedly, it seems more

Figure 9: Location Choice Analysis



Plots shows cumulative distribution function of menu distance between entrants and incumbent neighbours compared with counterfactual cumulative distribution under random reshuffling. The left panel shows the full distribution. The right panel shows the bottom quintile to emphasize the higher incidence of similar menus in the observed distribution.

likely that the decision to exit is the result of cumulative effects of competition, which cannot be identified with a timing-based treatment definition. For example, if restaurant r receives a single nearby entrant, followed by a long duration without entry, and then exits the market, does that suggest the single new entrant increased or decreased the likelihood of exit? However, identifying the effect of cumulative entry is also quite difficult because the cumulative number of entrants received likely increases with time in the market. If the likelihood of exit tends to increase over time, independent of the number of new competitors, then this would lead to a spurious correlation between cumulative entrants and exit. On the other hand, if the ability to withstand competition from new entrants is sufficiently heterogeneous across incumbent restaurants, then it could lead to a survivor bias where the longest surviving restaurants are also those who have received the largest number of cumulative entrants.

Given these issues, we instead ask a simpler question: do restaurants in areas with high entrant intensity exit the market at higher rates? Restaurant exit could itself lead to entry–there may be persistent demand in the location or a new restaurant may simply want to use the existing food preparation facilities of a failed restaurant–and so to avoid this reverse causality issue we measure entrant intensity using only entrants from before the start of our menu data. Specifically, we define entrant intensity as the total count of entrants from November 7, 2015 to November 20, 2016, within 500m of every restaurant's (eventual) location, where entry is again inferred from inspections and Yelp (see section 2.3). We then estimate the effect of this entrant count on the hazard of exit for restaurants in our dataset from November 27, 2016 onwards.

5.1 Exit analysis methodology

While using fixed pre-period entrant intensity avoids some of the timing issues discussed above, this measure of entrant intensity is likely still strongly correlated with other location specific characteristics which could affect exit. Again, the direction of this bias is not clear. It could be that locations with many entrants also have fickle consumers or more volatile commercial rents, and thus restaurants exit at higher rates independent of entrant competition. It could also be that locations with very few entrants also have little restaurant demand, and thus the few restaurants that open in such locations often fail. In order to address these concerns we use a strategy that balances location characteristics by comparing restaurants with the same number of predicted pre-period entrants. In this analysis our treatment variable (the count of pre-period entrants) is a count variable and therefore we control for a generalized propensity score (GPS) to estimate the effect of different entrant counts on exit. This effect of different treatment levels is referred to as the "dose-response function" in Hirano and Imbens (2004) and we follow their estimating procedure[23]. The general idea is to first estimate the effect of the treatment on an outcome, conditioning on the probability of observing that treatment level using the GPS. One then calculates the effect of a specific treatment level on the outcome by predicting the outcome for each observation at the chosen treatment level (which includes the GPS evaluated at that treatment level) and then averaging the predicted outcome over all observations in the sample.

We first re-estimate our Poisson entry model, equation 7, using only entrants from the 54 weeks of the pre-period. We then derive the GPS directly from the predicted number of pre-period entrants using this model. Let λ_r be the predicted number of pre-period entrants within 500 m of restaurant r. This λ_r is an arrival rate (per 54 weeks) for new entrants in the area around restaurant r. We then define the GPS at entrant count n as the Poisson likelihood of n events with rate parameter λ_r:

$$GPS_r(n) = Pr(n|\lambda_r) = \frac{\lambda_r^n e^{-\lambda_r}}{n!} \tag{12}$$

In Equation 12, $GPS_r(n)$ is a function specific to every restaurant r. It measures the probability that a location with entry rate λ_r receives n entrants over 54 weeks.

We model the hazard of exiting in any one week using a Cox proportional hazard model with a common baseline hazard, $\phi_0(t)$. For restaurant r in a location that received n_r entrants over the 54 periods, the hazard of exiting after t weeks is:

$$\phi_r(t|n_r) = \phi_0(t) * exp(\gamma * n_r) \tag{13}$$

We then estimate the conditional expectation of the outcome given the treatment and the GPS. Note that in the conditional expectation equation below we evaluate the GPS for restaurant r at the actual number of entrants observed in that location in the per-period, n_r.

$$\phi_r(t|n_r) = \phi_0(t) * exp(\gamma_1 * n_r + \gamma_2 * GPS_r(n_r)) \tag{14}$$

Our interest is in the relative hazard (the exponentiated term) which shows how the hazard of exit increases or decreases with entry. Therefore we calculate the dose response function as the relative hazard of exit at a "dose" of n entrants. To do so we take the coefficients from Equation 14, predict the relative hazard at n entrants with the GPS evaluated at n, and then average this predicted relative hazard over all R restaurants:

$$E[\phi_r(t|n)/\phi_0(t)] = \frac{1}{R}\sum_r (exp(\hat{\gamma}_1 * n + \hat{\gamma}_2 * GPS_r(n))) \tag{15}$$

We use bootstrapping to calculate confidence intervals for Equation 15 using 1000 bootsamples for each dose level[24]. This estimated dose-response function shows the effect of being in a location with a given

[23]Our estimation is also informed by the discussion of the GPS in Flores, Flores-Lagunes, Gonzalez and Neumann (2012) and in Austin (2018), who discusses using the GPS for survival modeling.

[24]We draw with replacement from our estimation sample, re-estimate equation 14, and then calculate equation 15 with the estimates. We repeat this 1000 times and then report the 25th and 975th largest estimates for each dose level as the 95% confidence interval.

(pre-period) entry rate on the likelihood of later exit, and thus allows us to test whether greater competition (more entry) increases exit.

5.2 Exit analysis results

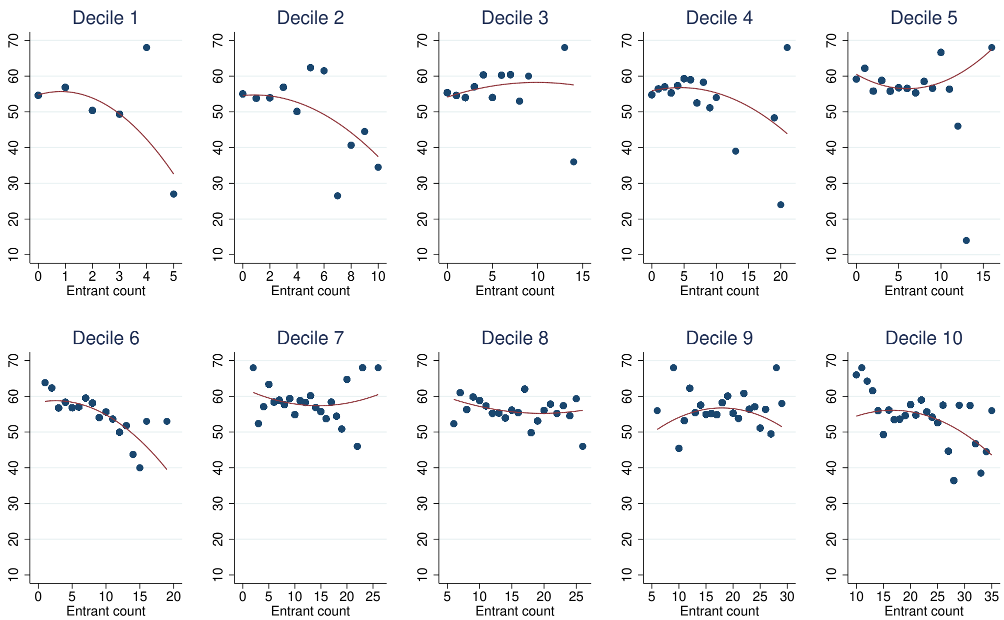
We start our analysis with 11,024 unique restaurants for which we have matching demographic characteristics and can predict pre-period entrant counts, and then apply two filters. First, we drop all restaurants that we observe for fewer than ten weeks. This primarily affects restaurants observed in the first week of our data which exit shortly after, and restaurants that enter our data (join the site) towards the end of our sample period. This requirement also drops restaurants which enter and exit our sample in fewer than ten weeks, behavior that is more likely to reflect exit from the delivery site than exit from the market. This filter drops 1,679 restaurants. Secondly, we drop restaurants whose GPS values are outside of a common support, removing an additional 35 observations[25].

To provide some intuition for our general methodology, we group restaurants into deciles by predicted pre-period entrants, so that within each decile the location characteristics should be fairly similar. We then plot survival time in weeks against the observed pre-period entrant count. In Figure 10 each point represents the mean survival time across restaurants that have the same count of observed pre-period entrants. The fit lines are based on a quadratic specification; while the number of restaurants in each entrant count bin can vary substantially, the fit line is weighted by restaurant count. The higher deciles have higher predicted entrants and therefore the range of observed entrants (horizontal axes) generally shifts rightward with each decile. Across most of the deciles, the survival time decreases noticeably as entrant count increases. However, for a given entrant count the mean survival time can be quite different across deciles: restaurants that had ten pre-period entrants in low deciles have much shorter survival times than restaurants with the same number of entrants in the upper deciles. We also show the heterogeneity of entrant count by location with two simple OLS regressions. In Appendix Table A5 we regress survival time on entrant count (column 1) and then run the same specification adding predicted entrants as a control (column 2). In the first specification we find that pre-period entrants have a small and insignificant negative effect on survival time but when controlling for predicted entrants the magnitude of this negative effect becomes ten times larger and statistically significant. These patterns again illustrate the heterogeneity of location characteristics by entrant intensity and motivates our use of the GPS for balancing.

Next we run a series of Cox proportional hazard models, as specified by Equation 13, and report the results in Appendix Table A5. When we include observed pre-period entrants (entrant intensity) without any controls (column 3) we find a coefficient of 0.0029, indicating that each additional entrant increases the hazard of exiting relative to the baseline by 0.29 percentage points. This implies that a restaurant in a location with an entrant rate of ten entrants in 54 weeks is about 2.9% more likely to exit in a given week than a restaurant in a location with no entrants; however, this coefficient is statistically insignificant. In column 4 we add the GPS and find a much larger positive coefficient on entrant intensity. This coefficient is also now statistically significant but, as emphasized by Hirano and Imbens, has no causal interpretation. Hirano and Imbens suggest using a flexible form for estimating the conditional expectation, and so in columns five and six we add an interaction term and quadratic terms. However, in the most flexible specification (column 6) all of the coefficients are imprecisely estimated and in column 5 the interaction term is insignificant with similar

[25] We apply the common support trimming method used in Flores et al. (2012), which drops restaurants with extreme GPS values. Specifically, we group the restaurants into entrant count quintiles and calculate five GPS values for each restaurant, one for each quintile using the median entrant count of that quintile (GPS_q). We then drop restaurants in quintile q if their GPS_q is out of the range of GPS_q values for restaurants *not* in the quintile. Due to the wide range of GPS_q both in and out of quintile q, this trimming only drops 35 restaurants.

Figure 10: Survival time against pre-period entrant count, by predicted entrant count decile.



Survival time in weeks graphed against pre-period entrant count, by predicted entrant decile.
Each point represents mean survival time for restaurants with the same entrant count.
Lines show quadratic fit with entrant count bins weighted by number of restaurants.
Sample restricted to restaurants surviving at least 10 weeks and in common support.

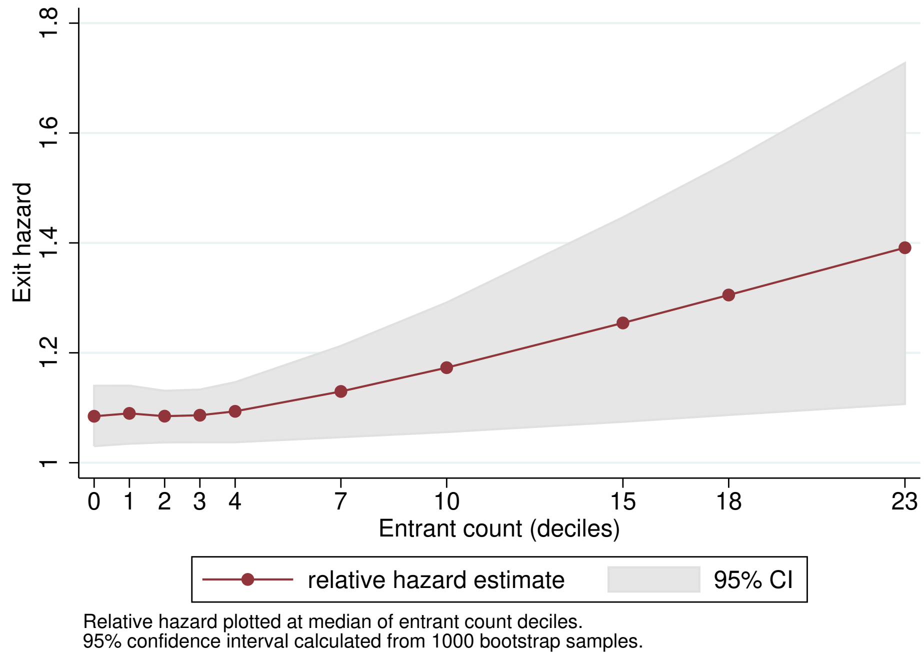
coefficients for entrant intensity and the GPS to those in column 4. Further, a likelihood ratio test comparing the goodness of fit for the simplest specification in column 4 to the more flexible forms in columns 5 and 6 cannot reject that the fit is equal. Therefore we choose the coefficients from the specification in column four to calculate the dose response function[26].We calculate this dose response at the median value for each entrant count decile and plot the results with bootstrapped 95% confidence intervals in Figure 11.

Figure 11 shows the relative hazard (exponentiated coefficients), with estimates at every decile significantly different from one (the value indicating no change in the hazard) at the 5% level. However, the relative hazard is the increase in the likelihood of exit compared to a location with both zero observed entrants and zero predicted entrants, and thus the more important implication of Figure 11 is that the magnitude of the relative hazards increases steeply and nearly monotonically over each decile. The hazard in the top decile (23 median entrants) is 31 percentage points larger than the hazard in the first decile (zero median entrants). We can calculate the predicted survival fraction after t weeks for a given decile using the baseline survival function and the relative hazard for that decile[27]. After 365 days, 80.5% of restaurants in the first decile are predicted to survive but only 75.6% in the highest entry decile, implying the probability of survival after a year is five percentage points lower in the high entry locations. These results suggest that competition from new entrants substantially increases the likelihood of exit, but only in areas with lots of entry. Of course, it is important to emphasize that these results are based on our measure of exit—leaving the website—and we do not know how well this measure approximates actual exit from the New York City restaurant market.

[26] In Austin (2018) the author also uses just the treatment variable and GPS.

[27] Denote the baseline survival function as $S_0(t)$ and the relative hazard for quantile q as rh_q. Then the predicted survival fraction at time t is $S_0(t)^{rh_q}$.

Figure 11: Effect of entrant intensity on exit hazard.



6 Conclusion

In this paper we estimated the response to entry in the restaurant industry in New York City using a panel of menus. We documented that the demographics of areas with high entry intensity, and the menu characteristics of restaurants in those areas, differ from those of areas with fewer entrants. This pattern can lead to bias in studies of the response to entry. We addressed this potential endogeneity problem using a matching strategy that balanced location characteristics using an entry model and restaurant characteristics using a pairwise measure of menu similarity. This two-stage matching technique has potential for applications in other environments, especially in markets where the attributes of differentiated products are conveyed via text (e.g., real estate listings, investment prospectuses, political candidates).

Our findings suggest that incumbent restaurants do not change their menus in response to competition from new entrants. We observe restaurants updating their menus on a regular basis and we find that, across all restaurants, there are statistically significant changes to prices over the durations we study. However, we do not find that restaurants are making these adjustments differentially in response to changes in the competitive environment. The size of our panel and the high entry rates in the industry allow us to estimate fairly precise confidence intervals, and we do not believe our results stem from insufficient statistical power. Further, we do not find any evidence that entrants strategically select locations to mitigate competition, and in fact, we observe entrants locating somewhat closer to incumbent restaurants with similar products than would be found from random location choice. However, we do find that restaurants in areas with many entrants are likely to exit the market sooner.

These results are broadly consistent with canonical monopolistic competition models. In the context of large markets, assuming away local competition may be an empirically plausible simplification. Nonetheless, our finding of higher exit rates does raise the question why restaurants don't respond if entry is affecting profits. One possibility is that restaurants are quite constrained in their ability to change their product after opening, as suggested by the "putty-clay" model of Aaronson, French, Sorkin and To (2017). It is also possible that firms may be constrained in their ability to adjust prices and product offerings by incentives internal to the firm (Kaplan and Henderson 2005, Gibbons and Henderson 2012) or by firm "identity" that precludes certain changes in product offerings even if those changes would improve profitability (Bénabou and Tirole 2011, Henderson and Van den Steen 2015). Empirical studies on endogenous product differentiation in monopolistically competitive markets may help us better understand these constraints.

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A Appendix: For Online Publication

A.1 Selection model and identification strategy

We start with the following reduced form model of restaurant outcomes, analogous to Section 3.2:

$$Y_{rt} = \beta_r * D_{rt} + u_r + u_{L_r} + \xi_{rt} + \xi_{L_r t} + \varepsilon_{rt} \tag{A1}$$

Following the potential outcomes framework, let Y^1_{rt} be the outcome of a restaurant at time t when there is entry (treatment) and Y^0_{rt} represent the outcome when there is not entry (control). From Equation A1, these terms and the switching equation may be expressed as follows:

$$\begin{aligned} Y^0_{rt} &= u_r + u_{L_r} + \xi_{rt} + \xi_{L_r t} + \varepsilon_{rt} \\ Y^1_{rt} &= \beta_r \mathbb{I}\{t \geq k_r\} + Y^0_{rt} \\ Y_{rt} &= D_{rt} * Y^1_{rt} + (1 - D_{rt}) * Y^0_{rt} \end{aligned} \tag{A2}$$

We want to estimate the effect of new competition on incumbent restaurants, the average treatment effect on the treated (ATT), β:

$$ATT = E[Y^1_{rt} - Y^0_{rt} | D_{rt} = 1] = E[\beta_r | D_{rt} = 1] = \beta \tag{A3}$$

We do not observe what restaurants that faced new competition would have counterfactually done in the absence of this competition ($Y^0_{rt} | D_{rt} = 1$). Further, it is highly likely that factors determining restaurant outcomes also affect entry. To model entry we assume that a new competitor enters near restaurant r at time t if expected profit (modeled as a latent variable) is positive[28].

$$D_{rt} = \mathbb{I}\{\theta_r + \theta_{L_r} + \psi_{rt} + \psi_{L_r t} \geq 0\} \tag{A4}$$

Equation A4 shows that the entry process may also be a function of characteristics of incumbent restaurant r and location L_r, both time-varying ($\psi_{rt}, \psi_{L_r t}$) and invariant (θ_r, θ_{L_r}). As discussed in Section 3.2, we address the endogeneity of entry with a difference-in-difference matching strategy. Given potential entry in period k, define the difference in an outcome d periods before entry and d periods after as $\Delta Y_{rk} = Y_{r,k+d} - Y_{r,k-d}$. Then we can estimate β from this difference:

$$ATT = E[\Delta Y^1_{rk} - \Delta Y^0_{rk} | \Delta D_{rk} = 1] = E[\beta_r | \Delta D_{rk} = 1] = \beta \tag{A5}$$

This differencing removes any correlation between the time-invariant terms in the outcome equation and the selection equation[29]. Entry and outcomes could still both be influenced by the time-varying terms ξ and ψ and therefore we use matching to mitigate this form of selection bias. Our identifying assumption is conditional mean independence:

$$E[\Delta Y^0_{rk} | \hat{P}(X(L)), M_r, \Delta D_{rk} = 1] = E[\Delta Y^0_{rk} | \hat{P}(X(L)), M_r, \Delta D_{rk} = 0]$$

[28] In equation A4 we are treating entry as a process independent of the characteristics of the entrant. We address entrant characteristics with the analysis in Table 8 and the entry location analysis in Section 4.4.

[29] In Equation A5 note that $\Delta Y^1_{rk} = Y^1_{r,k+d} - Y^1_{r,k-d} = \beta_r + Y^0_{r,k+d} - Y^0_{r,k-d} = \beta_r + \Delta Y^0_{kt}$

A.2 Cosine distance: details and implementation

We can compare menu M to menu M' by comparing their ngram weights on the set of J ngrams, where J is the superset of ngrams from both menus for some pre-chosen ngram size (we use a size of 3). If a menu has count m_i occurrences of ngram i then the weight x_i of this ngram is:

$$x_i = \frac{m_i}{\sum_{j=1}^{J} m_j} \tag{A6}$$

Damashek defines the "cosine similarity" $S_{M,M'}$ of two documents (menus) M and M' as the cosine of the angle between their ngram vectors (with elements denoted by x_{Mj} and $x_{M'j}$):

$$S\left(M,M'\right) = \frac{\sum_{j=1}^{J} x_{Mj} x_{M'j}}{\left(\sum_{j=1}^{J} x_{Mj}^2 \sum_{j=1}^{J} x_{M'j}^2\right)^{1/2}} \tag{A7}$$

In Damashek (1995) the author uses his method to assign documents to languages (e.g. "French") and topic areas for news articles in a given language (e.g. "mining"). He finds that Equation A7 performs well for language assignment but has worse performance for topic assignment. He suggests that this is because the ngram vectors of two articles written in the same language will have a great deal of similarity simply due to common and uninformative ngrams in the language or general group to which the documents belong. For example, in English the 3-gram "the" is common but uninformative about topic. To deal with this issue he suggests centering all ngram vectors by subtracting a common vector that captures the ngram distribution of some specific language or subject group. Letting μ represent this common vector of weights the "centered cosine similarity" is:

$$S^c\left(M,M'\right) = \frac{\sum_{j=1}^{J} (x_{Mj} - \mu_j)(x_{M'j} - \mu_j)}{\left(\sum_{j=1}^{J} (x_{Mj} - \mu_j)^2 \sum_{j=1}^{J} (x_{M'j} - \mu_j)^2\right)^{1/2}} \tag{A8}$$

In our context, we wish to subtract out the common distribution of restaurant menu ngrams and so we define the vector μ as simply the vector of ngram centroids across all restaurants $r \in R$. As described in Section 3.2, we want to capture a pre-treatment measure of the menu distance between two restaurants. Therefore we use the first observed menu for every restaurant. For the majority of restaurants this is the first period of our data but varies for later entrants[30]. If we weight each menu equally then the centroid for ngram j is:

$$\mu_j = \frac{1}{|R|} \sum_{r \in R} x_{M_r j} \tag{A9}$$

Note that when a menu M has no occurrences of ngram i that ngram receives zero weight, $x_{Mi} = 0$, but this weight of zero still enters the calculation of S^c. Finally, as mentioned earlier, we convert this measure to a

[30] As discussed below, this is a very large set of n-grams. Therefore, choosing different periods or combining periods is unlikely to have any qualitative effect on our measure. There are a few ngrams that show up in later menus which are missing from our μ vector. We assign these ngrams a μ value of zero.

Table A1: Most common n-grams in sample with frequency of occurrence.

_sa	_ch	chi	ed_	and
206624	197278	183113	176519	160072
ick	cke	en_	hic	ken
153950	148003	147005	145687	143927
_wi	th_	ith	wit	sal
123583	113200	111242	111117	105591
ala	nd_	_an	san	lad
96385	88437	83429	79267	78750
ich	_ro	che	_co	ice
76252	75512	73962	73711	73369

distance by subtracting it from 1, yielding our formula for cosine distance:

$$\omega\left(M,M'\right) = 1 - \frac{\sum_{j=1}^{J}(x_{Mj}-\mu_j)(x_{M'j}-\mu_j)}{\left(\sum_{j=1}^{J}(x_{Mj}-\mu_j)^2\sum_{j=1}^{J}(x_{M'j}-\mu_j)^2\right)^{1/2}} = 1 - S^c\left(M,M'\right) \tag{8}$$

In calculating this measure we use only the names of menu items and exclude the item descriptions (which are often missing) and the menu categories. We calculate the cosine distance between the initial menu of every restaurant in our sample, yielding a symmetric matrix of pairwise distances between all restaurants.

Our sample includes 23620 n-grams. Of these, 10454 appear in the sample at least ten times. Table A1 shows the most common n-grams; as shown, these include the n-grams comprising the words "chicken", "salad", and "sandwich".

A.3 Choice of predicted entrant bandwidth

The two-stage calliper matching process described in the text requires us to choose a bandwidth for the callipers. This bandwidth determines the range of predicted entrant counts in which we search for the closest control observation match by cosine distance. Bandwidth selection involves a tradeoff: a small bandwidth ensures a closer match on predicted entrant count in the first stage whereas a wider bandwidth improves the prospects of finding a close menu match in the second stage. Crucially, a wider bandwidth also increases the final sample size of matched treated and control pairs.

We explore possible bandwidths through a process that allows us to investigate this tradeoff:

1. We divide observations into quintiles of predicted entrant count $q \in \{1,2,3,4,5\}$.

2. For each observation i in quantile q we find the observation in quantile $-q \neq q$ with the smallest cosine distance to observation i. Then, we take the average across each quintile q. We denote the maximum of this average across all quantiles as the "maximum average cosine distance".

3. For each observation i in quantile q we select a random observation j from a quintile $-q \neq q$. For each covariate in the Poisson regressions we take the average of the standardized distance between the covariate value for observations i and j. We denote the maximum of this average across all quantiles as the "average maximum Poisson covariate distance".

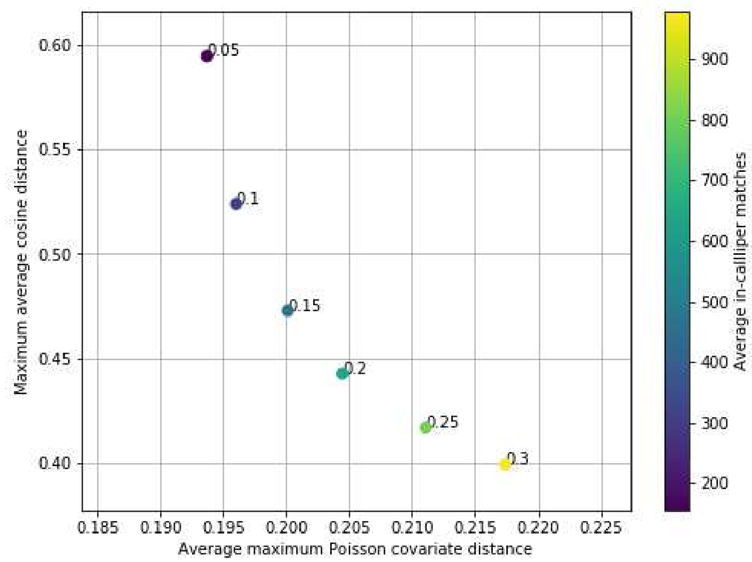

Figure A1: Comparison of cosine distance between treated and control pairs with standardized distance between Poisson regression variables for varying calliper sizes.

Figure A1 shows the resulting cosine distances and propensity covariate distances for a bandwidth of α standard deviations in the log of the predicted entrant count for $\alpha \in \{0.05, 0.1, 0.15, 0.2, 0.25, 0.3\}$. Based on these results, we select a bandwidth of 0.25 standard deviations of predicted entrant count for the two-stage calliper matching procedure.

A.4 Trimming the entrant count

When matching observations with similar predicted entrant counts, we trim observations with very high or very low predicted entrant counts. In a simpler model with a binary treatment variable Crump, Hotz, Imbens and Mitnik (2009) demonstrate that this approach improves the precision of the estimate by ensuring overlap in propensity covariate distributions. Specifically, we only include observations with a predicted entrant count in the *common support* of the quintiles of the observed entrant count. We calculate this common support as follows:

1. Divide the sample into five quintiles according to the observed entrant count at each observation.

2. Calculate the common support for each of the five quintile subsamples in a manner analogous to (Flores et al. 2012). Let q denote the set of observations in a given quintile subsample. Then, the common support CS_q for quintile subsample q is as follows:

$$CS_q = \left[\max\left\{\min_{i \in q}\left\{P(X_{j(i)})\right\}, \min_{i \notin q}\left\{P(X_{j(i)})\right\}\right\}, \min\left\{\max_{i \in q}\left\{P(X_{j(i)})\right\}, \max_{i \notin q}\left\{P(X_{j(i)})\right\}\right\}\right] \quad \text{(A10)}$$

3. Find the common support for the overall sample as the union of the common supports of the five quintile subsamples CS_q .

Figure A2 shows the range of predicted entrant counts for each quintile of the distribution of observed entrant counts. Qualitatively, the common support of the sample is the range of predicted entrant counts which lie in at least two quintiles of the observed entrant count. Trimming the sample to only include observations within

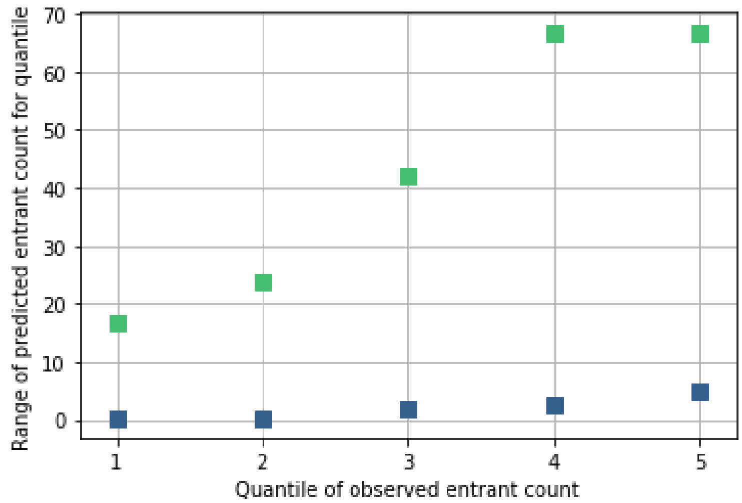

Figure A2: Range of predicted entrant counts for the five quintiles of observed entrant counts.

this common support ensures that we only match treated observations which could potentially be matched to a control observation in another quintile of the observed entrant distribution.

A.5 Pre-match differences between treated and control restaurants

As discussed in Section 3.2, a serious concern is that entry intensity may be correlated with location characteristics, making (unmatched) treated and control restaurants systematically different before the treatment period. The left panel of Table A2 uses the $d = 4$ sample to compare restaurant and menu characteristics for unmatched treated and control restaurants, four periods before treatment. The right panel uses the same sample and compares demographic characteristics of the restaurants' locations, showing the difference between the percent of the neighborhood with each characteristic and the count of other nearby restaurants. Treated restaurants have about 10 fewer items, higher prices at most points of the distribution (although these differences are not significant), 24 more reviews, and higher user ratings. Treated and control restaurants are also in quite different areas. Treated restaurants are located in neighborhoods with younger, less impoverished, and more highly educated residents, whereas control restaurants are found in neighborhoods with a larger black population share, a greater percentage of households married and in families, and a larger share of the single-family detached units in the housing stock. Moreover, a treated restaurant has about 11 more nearby restaurants than a control restaurant. Many of these differences stem from the fact that treated restaurants are in dense, high-income areas with frequent entry and many restaurants; a large percentage are located in lower Manhattan.

These differences highlight an identification challenge likely to be an issue for any study using entry to examine responses to competition. Specifically, locations with high entry intensity have both different demographic characteristics and different types of firms than locations with lower entry intensity. If a researcher only has cross-sectional data on post-entry outcomes then comparing firms near entrants to those further away could yield very misleading results. In our case we would conclude entry leads to shorter menus and higher prices. Further, if firms in areas with high intensity of entry also vary in the frequency with which they make changes, then longitudinal studies (including simple difference-in-difference methods) may also lead to biased conclusions. This motivates the use of the two-stage matching method in this study.

Table A2: Statistical tests for difference between treated and control restaurants. All values are measured four periods prior to treatment. Sample excludes outliers and missing price periods.

(a) Menu attributes

	Menu stats	
	t-tests	N
item count	-9.88***	126233
mean item price	0.16*	126233
median item price	0.18**	126233
p25 item price	0.10*	126233
p95 item price	0.14	126233
stars	0.04	121925
review count	24.56*	112771
order rating	0.54**	123590
food rating	0.35	123589
delivery rating	0.84***	123590

Tests difference between treated and control.
Calculated using values 4 periods before treatment.
Sample excludes outliers and missing price periods.

(b) Demographic attributes

	Demographics	
	t-tests	N
age.25.29	0.015***	126813
age.30.39	0.017***	126813
age.70.79	-0.001**	126813
race.white	0.063***	126813
race.black	-0.038***	126813
hh.family	-0.058***	126813
hh.married	-0.026***	126799
educ.degree	0.080***	126799
poverty	-0.015***	126799
income.100.150	0.005***	126799
income.150.200	0.005***	126799
unit.detached	-0.043***	125600
competitors 500m	10.694***	116750

Tests difference between treated and control.
All demographics calculated as percent of area.
Competitors calculated 4 periods pre-treatment.
Sample excludes outliers and missing price periods.

A.6 Additional regression results

Table A3: Fixed effect results for physical distance treatment. Dependent variables are quality ratings, weekly hours of operation, count of listed cuisines, and count of reviews. All specifications include restaurant fixed effects and period fixed effects. Standard errors clustered by entrant are shown in parentheses. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

	(1) Food Rtng	(2) Delivery Rtng	(3) Order Rtng	(4) Wkly Hrs	(5) Num Cuisines	(6) Review Ct.
treat_post	-0.056	-0.048	-0.093**	-0.643	0.023	-3.646**
	(0.045)	(0.046)	(0.042)	(0.433)	(0.039)	(1.615)
Observations	23486	23486	23486	23481	24020	21557
Clusters	310	310	310	311	311	308
Treated	1808	1808	1808	1812	1813	1802
DepVarMean	86.73	87.35	90.84	59.52	4.39	498.25

(a) Four period duration

	(1) Food Rtng	(2) Delivery Rtng	(3) Order Rtng	(4) Wkly Hrs	(5) Num Cuisines	(6) Review Ct.
treat_post	0.040	-0.034	-0.023	0.123	-0.041	0.188
	(0.070)	(0.065)	(0.050)	(0.652)	(0.050)	(2.888)
Observations	15488	15488	15488	15484	15768	14085
Clusters	223	223	223	224	224	222
Treated	934	934	934	935	935	927
DepVarMean	86.62	87.41	90.87	60.12	4.39	470.95

(b) Six period duration

	(1) Food Rtng	(2) Delivery Rtng	(3) Order Rtng	(4) Wkly Hrs	(5) Num Cuisines	(6) Review Ct.
treat_post	-0.134	-0.048	-0.097	0.218	-0.067	1.591
	(0.114)	(0.097)	(0.084)	(1.022)	(0.078)	(4.846)
Observations	9437	9437	9437	9429	9552	8486
Clusters	150	150	150	150	150	149
Treated	503	503	503	503	503	496
DepVarMean	86.51	87.31	90.37	56.98	4.40	429.27

(c) Eight period duration

A.7 Predicted entrant model

Table A4: Overlapping durations and heterogeneity. Fixed effect results for physical distance treatment. First two columns define the pre-periods as $[3,5]$ and post as $[10,12]$; last four columns include interaction between treated X post and number of nearby incumbent competitors. All specifications include restaurant fixed effects and period fixed effects; standard errors clustered by entrant are shown in parentheses. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

	(1) Itm Prc	(2) Itm Prc	(3) Med Prc	(4) p95 Prc	(5) Itm Ct	(6) Itm Prc
treated X post	0.015	0.050***	0.013	-0.028	0.093	-0.011**
	(0.014)	(0.017)	(0.021)	(0.060)	(0.463)	(0.006)
post	0.034***	0.016***	0.027**	0.074	-0.004	0.030***
	(0.007)	(0.004)	(0.011)	(0.056)	(0.135)	(0.004)
trtd X pst X comps			-0.002	0.001	0.124	0.002
			(0.004)	(0.009)	(0.091)	(0.002)
open			-0.030***	-0.012	1.663***	
			(0.010)	(0.019)	(0.199)	
Observations	801392	521566	19016	19016	19016	3383522
Clusters	158	104	285	285	285	311
Treated	668	378	1668	1668	1668	1811
Sample	d=6	d=8	d=4	d=4	d=4	d=4
DepVarMean	8.62	8.57	8.40	17.95	148.57	8.69

Table A5: Exit Analysis Specifications. First two specifications show OLS results for survival time (weeks), last four show results from proportional hazards models. For hazard models we show coefficients, not hazard ratios. There are 1760 observed exits in the sample. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

	(1) surv. time	(2) surv. time	(3) exit haz.	(4) exit haz.	(5) exit haz.	(6) exit haz.
observed entrants	-0.0198	-0.1952***	0.0029	0.0108***	0.0140***	-0.0243
	(0.0237)	(0.0640)	(0.0031)	(0.0039)	(0.0051)	(0.0212)
predicted entrants		0.2025***				
		(0.0686)				
GPS				0.6198***	0.6080***	-0.1364
				(0.1773)	(0.1768)	(0.7629)
obs. ents. X GPS					-0.0624	0.0464
					(0.0675)	(0.0961)
obs. ents.^{2}						0.0012*
						(0.0006)
GPS^2						0.6847
						(0.9114)
Observations	9310	9310	9310	9310	9310	9310
Likelihood	-39855.1	-39850.7	-15718.4	-15712.6	-15712.2	-15710.3

Table A6: Poisson regression coefficients for the number of nearby entrants during the sample period. The unit of observation is a restaurant. We used a LASSO penalty estimator with penalty parameter of 2.5 to select the regression variables.

	Coefficient	Std. err.		Coefficient	Std. err.
Competitors within 25 m	-0.010	0.003	Spanish and English	-0.076	0.012
Competitors within 50 m	-0.017	0.004	Other IE, limited English	-0.009	0.006
Competitors within 100 m	-0.015	0.005	Other IE, English	-0.008	0.004
Competitors within 250 m	0.031	0.008	AP, limited English	0.085	0.005
Competitors within 500 m	0.804	0.014	AP, English	0.030	0.005
Competitors within 1 km	0.345	0.016	Poverty	0.020	0.005
Competitors within 2.5 km	0.061	0.013	Income ¡10k	-0.013	0.005
Efficiency rent	0.075	0.013	Income 10k-20k	-0.024	0.006
One-bedroom rent	1.575	0.342	Income 20k-30k	-0.020	0.005
Two-bedroom rent	-2.180	0.543	Income 30k-40k	-0.021	0.005
Three-bedroom rent	-2.113	0.988	Income 40k-50k	-0.042	0.004
Four-bedroom rent	2.752	0.626	Income 50k-60k	-0.018	0.003
Age < 10	0.001	0.011	Income 60k-75k	-0.018	0.003
$10 \leq$ Age ≤ 17	0.066	0.012	Income 75k-100k	0.007	0.003
$18 \leq$ Age ≤ 24	0.044	0.011	Income 100k-150k	-0.045	0.004
$25 \leq$ Age ≤ 29	0.118	0.010	Income 150k-200k	-0.036	0.004
$30 \leq$ Age ≤ 39	0.079	0.011	Owner-occupied	0.086	0.006
$40 \leq$ Age ≤ 49	0.023	0.007	Detached house	0.075	0.010
$50 \leq$ Age ≤ 59	-0.001	0.006	3-9 unit structure	0.187	0.010
$60 \leq$ Age ≤ 64	0.049	0.005	10-49 unit structure	0.158	0.009
$65 \leq$ Age ≤ 69	0.034	0.005	> 50 unit structure	0.087	0.013
$70 \leq$ Age ≤ 79	-0.004	0.006	Built post-2010	-0.025	0.003
White	-0.037	0.015	Built 2000-2009	0.013	0.002
Black	-0.024	0.013	Built 1990-1999	0.041	0.004
Asian	0.023	0.011	Rent 1250-1499	0.039	0.005
Latino	-0.055	0.016	Rent 1500-1999	-0.163	0.004
Commute out of county	0.035	0.010	Rent 2000+	0.021	0.004
Commute out of state	-0.025	0.003	Rent-to-income 35-40%	-0.026	0.003
Family household	0.108	0.016	Rent-to-income 40-50%	0.013	0.003
Married household	-0.142	0.011	Rent-to-income 50%+	0.006	0.004
Roommate household	-0.028	0.005	House value 500k-750k	0.018	0.003
Enrolled in college	0.014	0.013	House value 750k-1m	0.052	0.003
Not enrolled in school	-0.012	0.017	House value 1m+	0.001	0.003
College graduate	-0.065	0.013	Distance to subway	0.013	0.010
Spanish, limited English	-0.005	0.008	Constant	2.463	0.004
Observations	11909				
Adj. Pseudo-R^2	0.755				

A.8 Entrant intensity covariate balance

Table A7: Entrant intensity covariate balance. Sample divided by quintile of entrant count. Only selected covariates shown. Additional covariates available upon request.

Variable		Q1	Q2	Q3	Q4	Q5
Competitors within 100 m	Without callipers	1.36	0.74	0.02	0.82	1.13
	With callipers	0.22	0.06	0.05	0.09	0.06
Competitors within 500 m	Without callipers	2.60	0.98	0.11	1.29	1.93
	With callipers	0.75	0.02	0.12	0.05	0.31
Competitors within 1 km	Without callipers	2.44	0.91	0.03	1.29	1.95
	With callipers	0.69	0.07	0.03	0.07	0.24
One-bedroom rent	Without callipers	1.39	1.02	0.11	1.23	1.20
	With callipers	0.07	0.07	0.11	0.08	0.20
Two-bedroom rent	Without callipers	1.38	1.01	0.11	1.23	1.19
	With callipers	0.07	0.06	0.11	0.08	0.20
White	Without callipers	0.80	0.64	0.20	0.91	0.54
	With callipers	0.01	0.05	0.08	0.14	0.37
Black	Without callipers	0.50	0.44	0.02	0.68	0.63
	With callipers	0.02	0.09	0.03	0.26	0.36
Asian	Without callipers	0.14	0.05	0.27	0.07	0.53
	With callipers	0.08	0.02	0.13	0.04	0.37
Latino	Without callipers	0.58	0.57	0.13	0.69	0.92
	With callipers	0.09	0.02	0.05	0.06	0.06
Family household	Without callipers	1.85	0.86	0.11	0.96	1.79
	With callipers	0.43	0.06	0.02	0.08	0.26
Married household	Without callipers	0.92	0.38	0.13	0.40	1.24
	With callipers	0.21	0.06	0.01	0.17	0.39
Enrolled in college	Without callipers	0.32	0.19	0.21	0.16	0.71
	With callipers	0.09	0.03	0.03	0.25	0.45
College graduate	Without callipers	1.71	0.90	0.04	1.19	1.30
	With callipers	0.28	0.01	0.02	0.03	0.15
Poverty	Without callipers	0.60	0.55	0.06	0.81	0.53
	With callipers	0.02	0.04	0.05	0.17	0.32
Income 75k-100k	Without callipers	0.24	0.08	0.01	0.16	0.18
	With callipers	0.10	0.02	0.04	0.04	0.09
Income 100k-150k	Without callipers	0.29	0.47	0.02	0.50	0.33
	With callipers	0.13	0.09	0.02	0.13	0.16
Income 150k-200k	Without callipers	0.66	0.64	0.09	0.55	0.81
	With callipers	0.10	0.02	0.02	0.05	0.04
Detached house	Without callipers	0.91	0.12	0.43	0.52	0.51
	With callipers	0.30	0.02	0.12	0.18	0.04
3-9 unit structure	Without callipers	0.13	0.23	0.59	0.18	0.77
	With callipers	0.06	0.10	0.15	0.06	0.20
$>$ 50 unit structure	Without callipers	0.80	0.36	0.28	0.61	0.71
	With callipers	0.32	0.14	0.17	0.03	0.14
Built post-2010	Without callipers	0.12	0.11	0.06	0.06	0.20
	With callipers	0.03	0.06	0.15	0.12	0.11
Rent 2000+	Without callipers	0.78	0.59	0.11	0.66	0.52
	With callipers	0.07	0.05	0.04	0.04	0.08
Rent-to-income 50%+	Without callipers	0.78	0.58	0.17	0.76	0.56
	With callipers	0.01	0.09	0.02	0.05	0.17
House value 500k-750k	Without callipers	0.28	0.07	0.06	0.08	0.20
	With callipers	0.15	0.06	0.07	0.06	0.02
House value 750k-1m	Without callipers	0.65	0.06	0.26	0.14	0.23
	With callipers	0.23	0.02	0.04	0.12	0.14
House value 1m+	Without callipers	1.04	0.34	0.16	0.49	0.47
	With callipers	0.29	0.04	0.02	0.06	0.01
Distance to subway	Without callipers	0.66	0.03	0.29	0.34	0.42
	With callipers	0.31	0.09	0.02	0.29	0.45