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Delivery in the city: Differentiated products competition among New York restaurants☆

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ABSTRACT

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 locations with an entrant count in the top decile—areas with many new competitors—are 22% more likely to exit after a year than restaurants in the lowest entry decile.

1. Introduction

Firms in many industries compete in markets with a large number of competitors and substantial product differentiation. To study these markets, many papers in trade, urban economics, and other fields use models of monopolistic competition, especially the Dixit–Stiglitz constant elasticity of substitution (CES) model Dixit and Stiglitz (1977). In these models competition is aspatial or global: each firm makes decisions only in response to market aggregates, there is no strategic competition between individual firms, and no two firms are closer in geographic or product space than any other pair. An alternative approach uses spatial competition models (e.g. Salop, 1979) to describe markets with differentiated products. In these models competition is spatial or local: firms compete strategically but 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?

In this paper we study the responses of incumbent restaurants to competition from new entrants. 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 similar product, or both. Unless researchers have 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. We use a novel panel of restaurant menus in New York City to measure responses to competition in both geographic space and product space. We collected menus from Grubhub, 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. We then used separate data from New York City restaurant inspections and Yelp reviews to identify 1500 entrants over this period. These datasets allows us to precisely define the distance between competitors 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

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as quality ratings and hours of operation, and to examine the effect of new firm entry on the likelihood of incumbent firm exit.

The restaurant industry—with many firms, substantial product differentiation, and low barriers to entry–is perhaps the canonical example of monopolistic competition.[2] This industry also provides a simple and intuitive 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? In addition to being a good test case for theoretical models of competition, the restaurant industry is economically important. As one of the largest employers of minimum wage labor, the competitiveness of this industry has direct implications for the effects of increases in the minimum wage.[3] Moreover, recent work suggests restaurants may be a critical amenity driving central city growth Couture and Handbury (2020).

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 independent of competition, menu changes may differ systematically across different types of restaurants. If entry frequency is correlated with restaurant characteristics—and we present evidence that it is—then this may also lead to bias. For instance, if high end sushi restaurants tend to locate in areas with lots of entry, then we might mistakenly attribute menu changes from an increase in the price of tuna to competition from new restaurants. 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.

Given the frequent entry and large number of firms in the New York City restaurant market, in most of our analysis we examine outcomes within very small sub-markets—our baseline analysis uses a radius of 500 m around an incumbent restaurant—and over short durations of 8, 12, and 16 weeks. At these levels of granularity in geography and time it is difficult to find cost shocks or other instruments for entry; in fact, little data of any kind is available at these scales. Therefore, to address the endogeneity issues above, we instead 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 similar menus and are sited in locations with equal entry frequency, but in that period faced no changes to the competitive environment. The intuition for our identification strategy is that restaurant location choice across these small sub-markets and within these limited durations is unlikely to be completely deterministic. Many factors affect restaurant site decisions including zoning, rent, square footage, the existence of previous restaurant infrastructure (cooking equipment, ventilation, plumbing, restrooms–all of which can affect startup costs), utility costs, and lease lengths, see discussion in Robson (2011). Further, location options are limited; 2017 retail vacancy rates for the five boroughs range from 2.9% to 4.1% Marcus & Millichap (2017). Therefore the central assumption for our matching approach is that for two similar restaurants in two sub-markets, which market receives entry in a given short duration is essentially random. Note that variation in entry times is a crucial part of this strategy: over different periods of our data the same restaurant can serve as both a treated and control observation.

A key 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 called "cosine similarity" to calculate a scalar measure of the similarity of two restaurant menus, 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 treated 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. We find no competitive response across different quantiles of restaurant prices, nor do we find a response at the item level, which controls for menu composition changes. Our results also show that restaurants are neither adding nor removing items in response to entry. Further, we find no evidence of restaurants responding along other margins, including food, order, or delivery quality, cuisine designations, or hours of operation. Our findings are consistent across a battery of specifications, including cases where the entrant's menu is very similar to those of the incumbents, and where there are relatively few incumbent restaurants, making an additional restaurant a larger competitive shock. However, we do find a causal relationship between high intensity of nearby entry and a higher rate of exit, which suggests that competition does affect firm profit. Our data is not sufficient to estimate consumer preferences and validate one particular model of competition, but our results provide support for the weak strategic interaction assumption of aspatial monopolistic competition models, and are relevant for a variety of related subjects, including retail competition, firm clustering, and location choice.

This surprising result of no competitive response is consistent with a recent theoretical paper by Gabaix et al. (2016) showing that mark-ups in random utility models of monopolistic competition are often minimally affected by additional competition in markets with many competitors. In a different context, Arcidiacono et al. (2020), find that Walmart's entry into a market has no effect on the prices of incumbent grocers, despite causing a 16% decline in revenue. Nonetheless, the restaurant industry is notoriously competitive and prices may be sticky ("menu costs"); it is natural to wonder if restaurants have the capacity to adjust menus in response to entry.[4] In our sample restaurants change their menus with high frequency: the median duration between price changes is two weeks. Therefore it is worth emphasizing that our results show frequent menu changes but no *differential* change in response to entry.

[2] 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).

[3] Aaronson and French (2007) show that if the market is competitive, or monopolistically competitive with a constant elasticity of substitution, 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 Draca et al. (2011).

[4] There is some evidence of price competition in the literature, with both Thomadsen (2005) and Kalnins (2003) studying local competition among fast food franchises. 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" (October 24, 2017, Gasparro, 2017). 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).

While our dataset allows us to observe detailed firm behavior, it also has several limitations. One limitation is that we only observe online menus and cannot observe changes to dine-in menus, if these differ from online menus, or changes to dine-in service (e.g., interior space improvements). A second limitation is that not all restaurants are on the Grubhub delivery platform and it is possible that those on Grubhub compete differently from those that are not, a selection issue. A third limitation is that we only observe restaurants in a single large city. We discuss the implications of these limitations in Section 7.

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, which we denote "menu distance." In Section 4 we present our main results on the causal response to entry in geographic space, evaluate the robustness of these findings, and examine potential heterogeneity. As an extension, we try an alternative identification strategy based on distance to the entrant, rather than matching. In Section 5 we estimate the response to entry in product (menu) space. Lastly, we estimate the effect of entry intensity on the likelihood of incumbent restaurant exit. We conclude with a summary and discussion of how our results fit into the theoretical literature on competition in differentiated markets.

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 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 geographic or product space, or do they simply compete indirectly for a share of a consumer's expenditure with all other restaurants in the market?[5]

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:

$$V_{ij} = v(p_j) + \epsilon_{ij} \tag{1}$$

The term $v(p_j)$ 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:

$$\epsilon_{ij} = -t^g d_{ij}^g - t^m d_{ij}^m + \mu e_{ij} \tag{2}$$

Eq. (2) allows the match value to depend on the geographic distance, d_{ij}^g, between consumer i and restaurant j (e.g., measured in km), and a distance in product space, d_{ij}^m, 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. 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:

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

When μ is 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 classical (Chamberlin) 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 $v(p)$, the model collapses to the canonical CES form of Dixit and Stiglitz (1977) where firms choose a constant mark-up over marginal cost.[6]

If firms compete locally by setting prices, then Eq. (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 et al. (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.

[5] The terminology describing spatial competition models can vary across authors. The title of the Salop (1979) paper is "Monopolistic Competition with Outside Goods." In Tirole's "The Theory of Industrial Organization" he describes Salop's model as "oligopolistic competition with free entry" Tirole (1988). Thisse and Ushchev describe these models as "spatial models of monopolistic competition" and classify them under an approach defining monopolistic competition as the limit of oligopolistic competition Thisse and Ushchev (2018). Anderson and de Palma (2000) describe the spatial model, the logit model, and the CES as models of "oligopolistic competition with differentiated products." Nonetheless, the distinction is always clear. In spatial models there is a distance component in consumer preferences that makes them asymmetric: if the distance between firms A and B is less than the distance between firms B and C, then A and B are closer substitutes. In aspatial monopolistic competition models there is no distance measure and consumer preferences are symmetric such that any pair of firms are equally close substitutes. In our paper we will use the terms spatial competition and local competition to describe models with a measure of distance, such as Salop (1979), and describe aspatial models with symmetric preferences as monopolistic competition or global competition, such as Dixit and Stiglitz (1977).

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

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 the distant consumers near A. On the other hand, if competition is global (t^g/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 may choose to make their menu more similar to that of the entrant.[7] 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 (markups, capacity, output). 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. Using cross-sectional data, they 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 also 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.[8] 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. Using retail scanner data the author finds that markups are significantly lower in larger US counties, and interestingly for our study, that markups in New York City are "close to the undistorted monopolistically competitive limit."

There is less empirical work on local competitive responses in differentiated markets with many firms. One approach investigates the cross-sectional relationship between geographic differentiation, product differences, and prices. Another approach analyzes prices and product positioning in the context of mergers. Examples of the first approach include Netz and Taylor (2002) in the retail gasoline market and Chisholm et al. (2010) in movie theaters; examples of the second approach include Sweeting (2010) in radio and Pinkse and Slade (2004) in the British beer market. Two papers study restaurants in particular, but focus on fast food chains, whereas our data is mostly non-chain restaurants. 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. Thomadsen (2005) uses data on Burger King and McDonald's outlets in a California county to estimate a supply and demand model, and then simulates different merger scenarios, concluding that mergers among geographically closer outlets of the same franchise increase prices more. As in Kalnins (2003), the author's results suggest spatial competition among similar restaurants. However, chain restaurants may have very different incentives in their price decisions than the non-chain restaurants we examine in this paper Lafontaine (1995). A third approach examines the response of incumbent firms to entry. This literature has mostly focused on how smaller incumbent firms react when a large and efficient retailer enters the market, such as Walmart.[9]

The markets we study and the data we use share some features with earlier studies, but differ in several important ways. First, the literature on large differentiated markets has mostly focused on market level outcomes, rather than on 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. 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 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

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

[8] 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 et al. (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 et al. (2012); Dhingra and Morrow (2019); Bertoletti and Etro (2016), and Parenti et al. (2017).

[9] This is a well developed literature. Notable examples include Basker (2005) and Arcidiacono et al. (2020) on Walmart and Atkin et al. (2018) on the entry of international retailers into Mexico. A unique paper by Busso and Galiani (2019) examines competition among smaller firms using a randomized control trial with grocery stores in the Dominican Republic, finding that incumbent stores lower their prices but do not change the quality of their products or services.

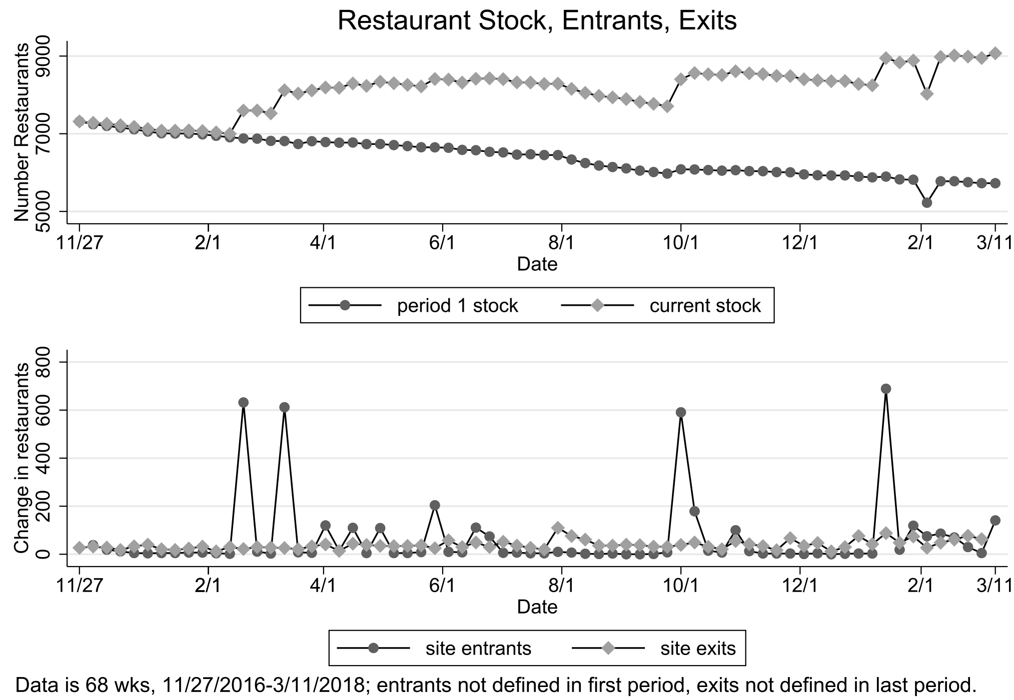


Fig. 1. Stock and flow of restaurants on Grubhub.

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 (2018) notes, these high-frequency directly-measured prices avoid some of the potential issues 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 Fig. 1 shows the simple count of restaurants in every week ("current stock"), along with the stock of restaurants observed in the first period that are present in each subsequent period ("period 1 stock"). These two stocks differ because new restaurants appear on the website ("site entrants") and existing restaurants leave the website ("site exits"), as shown in the bottom panel.[10]

In February 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. It is also unclear whether a restaurant has the same prices and products online and offline (dine-in).[11] We discuss how selection and other features of our dataset may limit our conclusions in Section 7. An additional issue is that while our dataset contains a high level of detail on restaurant prices and products, it also has a fair amount of noise. One important source of noise is that restaurants offer menus that vary with the time of day (e.g., breakfast, lunch, or dinner menus), and also often list shorter menus when they are closed (customers have the option to pre-order). Since we collect data at different times of the day throughout our panel, some of the week to week variation in a given restaurant's menu is the result of this time-of-day effect. The 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 Appendix section B.1 we provide more detailed discussion on the types and sources of noise in the data. In our empirical analysis we show that our results are robust to various specifications addressing the noise.

2.1. *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 of the 95th percentile item price on a menu, "p95 item price," is \$18.66 and for the average restaurant the mean item price (\$9.4) 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

[10] Initially, the period 1 stock is only slightly smaller than the current stock, but then the two series diverge significantly due to a spike of entrants in mid-February. These spikes in site entrants suggest the website may add new restaurants in waves. In most of the analysis we define market entry using other sources, see Section 2.2.

[11] A class-action lawsuit filed in April 2020 in the Southern District of New York against Grubhub and other delivery services alleges that these companies use a "No Price Competition Clause" that prevents restaurants from charging different online and dine-in prices. Zhou (2021) provides a helpful discussion of the lawsuit. The case is *Class Action Complaint, Davitashvili* et al.*v. Grubhub, Doordash, Postmates, Uber (S.D.N.Y. filed April 13, 2020) (No. 1:20-cv-03000).*

Table 1
Descriptive statistics on restaurant characteristics.

	mean	median	sd	min	p1	p99	max	N
item count	124.43	100.00	88.67	10.00	15.0	399.0	500	419,680
median item price	8.62	8.00	3.35	2.50	3.0	18.5	25	419,680
mean item price	9.40	8.82	3.88	2.28	3.9	22.9	49	419,680
p5 item price	2.68	2.25	1.62	0.00	0.5	9.0	25	419,680
p95 item price	18.66	16.00	13.05	2.99	6.5	70.4	228	419,680
cuisines	4.05	4.00	3.11	0.00	0.0	14.0	35	423,111
reviews	380.40	205.00	509.94	1.00	4.0	2326.0	10064	370,616
stars	3.72	4.00	1.19	1.00	1.0	5.0	5	395,882
food rating	85.30	88.00	9.62	0.00	50.0	100.0	100	405,994
order rating	89.61	92.00	9.01	0.00	56.0	100.0	100	405,991
delivery rating	86.09	89.00	11.09	0.00	46.0	100.0	100	405,977

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	141,582
mean price	0.28	0.09	3.69	1	149,696
p5 price	0.24	0.09	7.83	2	70,536
p25 price	0.54	0.26	7.55	2	73,140
p50 price	0.84	0.50	7.67	2	71,951
p75 price	0.98	0.50	7.85	2	70,289
p95 price	1.32	0.32	6.62	2	83,378

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.

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 141,582 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. Different quantiles/measures of the item price distribution change with different frequencies. 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.

Lastly, in Appendix section B.2 we look at changes within a restaurant over time by running panel regressions at the restaurant-week level. We find that restaurants increase prices roughly proportionally across different items on the menu, implying that prices increase by a larger (dollar) amount for more expensive items (see Figure A.3). Averaging across restaurants, we find that median item prices increase at $0.007 per week, menus increase in length by 0.09 items per week, and the average restaurant receives 5.3 new reviews each week (see Table A.5). Overall, the results from these tables and Figure A.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.

2.2. 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 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. The median difference between the inspection date and the first review date for the entrants in our sample is 25 days. In our main analysis, we examine outcomes over ranges of 8, 12, and 16 weeks, and in an extended analysis we examine outcomes up to 32 weeks after the entry date. Therefore while we believe our entry dates are quite accurate, our analysis allows for substantial measurement error. In Fig. 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

[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.

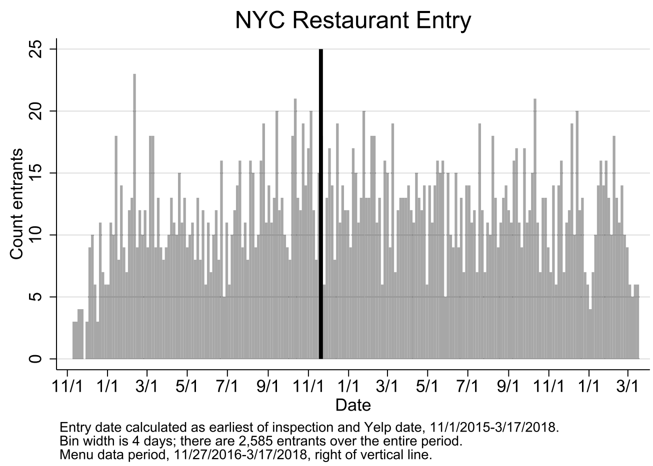


Fig. 2. Entrants identified from inspection and Yelp data.

17, 2018. The area to the left we refer to as the "pre-period" and only use in our analysis of market exit in Section 6.

3. Empirical approach

Our identification strategy compares the behavior of restaurants which have experienced a change in their competitive environment with restaurants which have not. We first assign "treated" status to restaurant-periods which have at least one new entrant opening within a specified distance and "control" status to restaurant-periods with no entrants within this distance. Next we 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. We run regressions on the matched sample of treated and control pairs to measure the causal response to the new entrant.

3.1. Treatment and control

We define treatment as the opening of one or more entrants nearby within a limited duration, thus treatment is a function of distance and time. 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 entrants. Further, if an incumbent faces entry over many successive periods, then it becomes difficult to identify which set of entrants triggered any competitive response. Therefore, we choose to focus on entrants at close distances to incumbents and within a short entry window. We try a range of distances and three different durations, representing cases where we think new competition is most likely to trigger a response that can be identified.

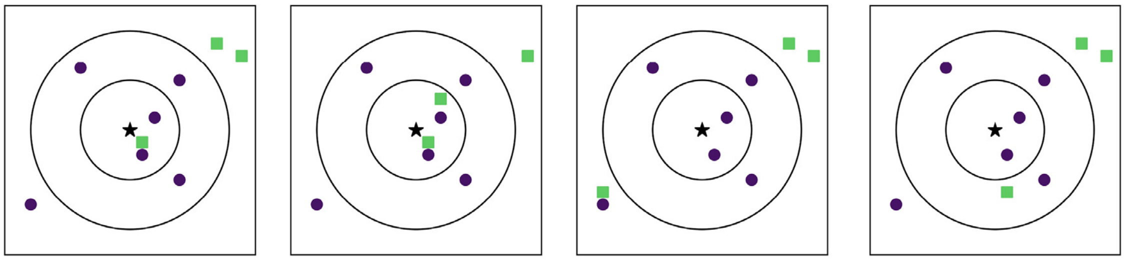
To implement this, we specify a tuple (d, ρ_T, ρ_C) where d is a duration (measured in weeks), ρ_T is an inner radius, and ρ_C is an outer radius, where $\rho_C > \rho_T$. We refer to the distance between ρ_T and ρ_C as a "spatial buffer." In our main analysis we measure ρ_C and ρ_T in meters (geographic space) but in Section 5 we use a measure of the distance between menus (product space). A restaurant is deemed treated at time period t if and only if there were one or more entrants within ρ_T over the entry window $(t - d/2, t]$, and no other entry from period $t - 2d$ to period $t + 2d$ within the larger radius ρ_C. Additionally, there must have been at least one entrant between $t - 1$ and t, which ensures that a sequence of entrants within $d/2$ consecutive periods only defines a single treated period and thus prevents double counting (see discussion of case h in Fig. 4 below). We define a restaurant as a control at time period t if there was no entry within the larger radius ρ_C over the entire $[t - 2d, t + 2d]$ period, or $4d$ consecutive periods. If there is any entry within $[t - 2d, t + 2d]$ that occurs outside of the entry window, $(t - d/2, t]$, or within the buffer zone between ρ_T and ρ_C, then a restaurant is neither treated nor control at t, which we denote as "neither" status. Fig. 3 provides a visual representation of the spatial aspects of treatment and control definitions. Fig. 4 shows a number of example time lines illustrating treatment timing, with filled circles representing entry within ρ_T. Cases a and b are both control restaurants at t because there is no entry over $[t - 2d, t + 2d]$. Note that case b would not be a valid control at $t - 1$ or $t + 1$ since in both cases there would be an entrant within the $4d$ period window. Cases c and d are both treated, with c showing the case of a single entrant and d showing multiple entry. In cases e, f, g, and i there is entry outside of the entry window $(t - d/2, t]$, thus these restaurants are neither treated nor control at t. Case h is also "neither" because there is no entry in the period immediately preceding t. This sequence of entrants would be valid for treatment in the following period, (treated at $t + 1$). Similarly, case i would be a valid treated restaurant for the preceding period (treated at $t - 1$).

These definitions yield conservative samples of treatment and control restaurants. As noted, we do not know the maximum distance at which restaurants compete and it is unlikely that a sharp spatial cutoff between competitor and non-competitor exists. We therefore use the spatial buffer—the difference between ρ_T and ρ_C—to ensure that all control observations are always significantly further away from the nearest entrant than any treated observations.[14] In our analysis, we will compare restaurant behavior in the pre-entry periods $[t - d, t - d/2]$ to behavior in the post entry periods, $[t + 1, t + d]$. We exclude the entry periods $(t - d/2, t]$ since these could possibly already reflect a competitive response. We also exclude the pre-entry periods $[t - 2d, t - d)$ and the post-entry periods $(t + d, t + 2d]$, which we refer to as buffer periods because they help to insulate our estimates from the potential response to entrants outside of the $[t - 2d, t + 2d]$ window. For example, it is possible that restaurant behavior in the pre-entry buffer period reflects the response to previous entry before $t - 2d$, or that behavior in the post-entry buffer period could include responses to entrants after $t + 2d$, if incumbents are able to anticipate entry. It is worth emphasizing that since treatment is defined by geography *and* timing, two incumbent restaurants may receive the same number of entrants within ρ_T over our sample period, but for a given period t one may be treated while the other is control. In this way our approach is somewhat similar to identification strategies that compare treated units with units that will be treated in the future.

In our analysis we test a range of radii, but use an inner radius of $\rho_T = 500$ m and an outer radius of $\rho_C = 600$ m as our baseline. 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 et al. (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). Across the different samples used in our estimation, the average number of incumbent competitors within 500 m ranges from 14 to 27 restaurants.

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 over $4 * d$ weeks a treated restaurant only receives new competition within the short entry window $(t - d/2, t]$, and

[14] For example, if ρ_T is 500 m, we would not want to compare a restaurant with a competitor at 495 m to a control restaurant with an entrant 505 m away. The buffer ensures that any restaurant with an entrant between 500 m and 600 m is neither treated nor control. This also allays concerns about measurement error in entrant distances, which could occur through geocoding issues. In practice, there are few of these cases on the border of the inner distance; the average treated restaurant has an entrant at about half of ρ_T and the nearest entrant to a control restaurant is usually over $2 * \rho_T$.



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

Fig. 3. Examples of treatment and control assignment. The caption for each example indicates the assignment for the restaurant at the center of the diagram (indicated by a star). The small solid circles represent incumbent restaurants and squares represent entrants. The two concentric circular lines represent the radii ρ_T and ρ_C.

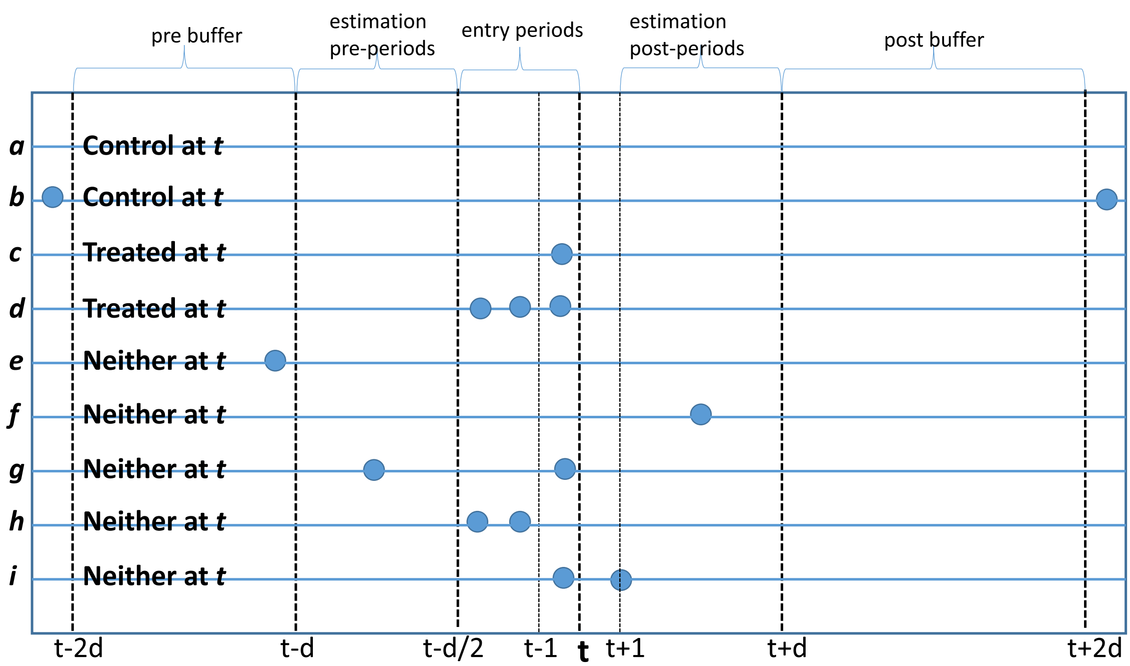


Fig. 4. Schematic of the timing for treatment and control assignment.

a control restaurant has no competitors over the entire $4 * d$ 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 ρ_T of restaurant r, so that $D_{rt} = \mathbb{1}\{t \geq k_r\}$. Our reduced form model for restaurant outcome Y_{rt} for $t \in [k_r - d, k_r + d]$ is:

$$Y_{rt} = \beta * D_{rt} + u_r + u_{L_r} + \xi_{rt} + \xi_{L_r t} + \epsilon_{rt} \tag{4}$$

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} and $\xi_{L_r t}$ represent restaurant specific and location specific time-varying shocks that could be correlated with treatment timing. 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 Eq. (4), 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 A6 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 et al., 1998; 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 use a two-stage process to 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. In the first stage, we calculate the predicted intensity of entry for each location L_r using locational variables $X(L_r)$. We then match each treated restaurant with a subset of control restaurants with a similar likelihood of facing a new entrant. In the second stage, we 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. Our key identifying assumption is that conditional on the predicted entrants and menu text, competition within this time period is essentially randomly assigned.[15] 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. Chabé-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.

[15] More formally, 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 identifying assumption is conditional mean independence (see Smith and Todd, 2005): $E[\Delta Y^0_{rk} | \hat{P}(X(L)), M_r, \Delta D_{rk} = 1] = E[\Delta Y^0_{rk} | \hat{P}(X(L)), M_r, \Delta D_{rk} = 0]$.

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 (with replacement) 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 $\rho_T = 500$ m 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, $X(L_r)$:

$$ln(E[P(L_r)|X(L_r)]) = X(L_r)'\theta \tag{5}$$

As candidates for $X(L_r)$, we assembled a large number of census tract variables from the 2011-2015 American Community Survey five year averages file, "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 show the coefficients estimates in Appendix Table A2.

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 menu distance 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. Using cosine similarity to measure menu 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. 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. Then a menu with J unique ngrams can be represented as a J-dimensional vector of these proportions or weights. 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 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. For ease of interpretation, we subtract this measure from 1 and call the resulting measure "menu distance," which ranges from 0, when there is no distance between products, to 2, indicating the maximum distance between products.

Somewhat similar measures have been used in other papers to compare differentiated products, but not using ngrams and not in the context of restaurants.[16] Therefore we now present some results validating this measure and then at the end of this section describe how we use menu 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 menu distance is a salient measure of cuisine then two restaurants with similar cuisines should have a closer menu distance. As shown in Fig. 5(a), the distribution of pairwise menu 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 menu distance between restaurants that share at least one cuisine first-order stochastic dominates the distribution of pairwise menu distances between restaurants that share no cuisines. Pairs of restaurants with a small menu 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 menu distance less than 0.8, compared to 20% of pairs sharing some cuisines, and only about 5% of pairs with no cuisines in common. Further, menu distance is a more precise measure than the cuisine categories of the online delivery service. Many of these categories are quite broad and two restaurants with the same sole listed cuisine may not have particularly similar menus. In fact, 50% of pairs of "American" cuisine restaurants have a menu distance greater than 0.9, implying their menus are only slightly more similar than a randomly drawn pair. Accordingly, Fig. 5(b) shows the distribution of menu distances between pairs of restaurants with successively more narrowly defined cuisine combinations: "Japanese", "Japanese" and "Sushi", and "Japanese", "Sushi", and "Lunch Specials." As the set of cuisines becomes more specific and the restaurants with the set of cuisines become more similar, the menu distance between pairs of restaurants within the cuisine set decreases.

To obtain our matched regression sample, we match each restaurant treated at period t with a control restaurant with the smallest menu distance.[17] We consider only potential control restaurants within the predicted entrant intensity callipers described above. We also 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 menu distance is within the lowest 5% of pairwise menu distances between all restaurants in the sample. In Appendix Section A.5 we run a series of exercises testing match quality. We first show that using predicted entrants helps to improve the balance of local area characteristics between treated restaurants and a set of matched possible control restaurants. We then show that within this set of control restaurants, using menu distance to pick the exact match further improves the balance of menu characteristics (prices, cuisine measures).

4. Competition in geographic space from market entrants

In this section 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 product response to competition: the median item price, the 95th percentile item price, the number of menu items, and the mean price change at the item level (described in detail below). In some analyses we also show the natural logarithm of median price to assess percentage changes, and additional quantiles of the item price distribution. We start with our main results showing the response to competition from entrants locating within different distances from an existing restaurant, using 500 m as a baseline. We then run a number of robustness checks examining different outcomes and durations, explore heterogeneity in the response to entry across restaurants and within a restaurant's menu, and examine the location choices of entrants. In an extension we use a different identification strategy that compares incumbent restaurants all within 1500 m of the same entrant, but which vary in distance to that entrant.

4.1. Main results: spatial competition in geographic space

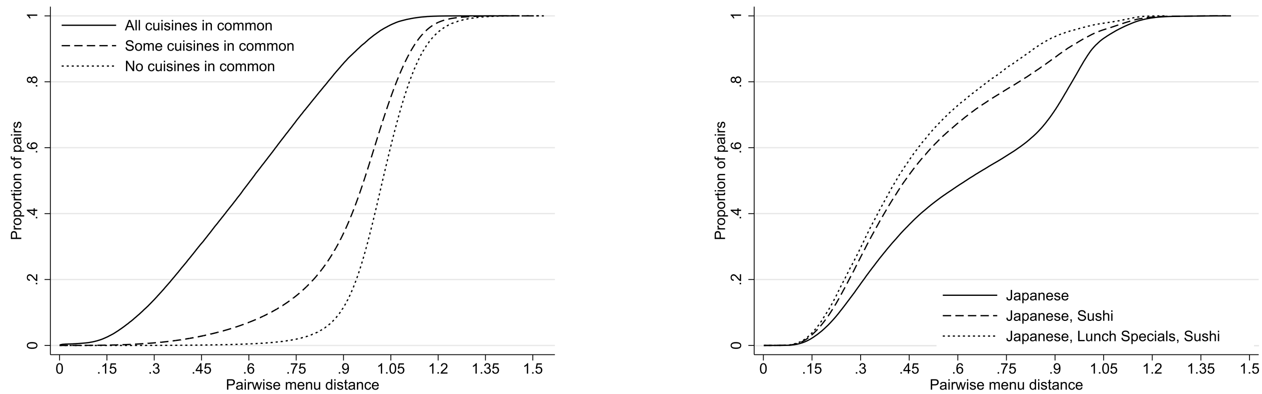
We use primarily two fixed effect specifications to examine the response to competition: a restaurant-level specification and an item-level specification. In the restaurant level specification we compare matched treated and control restaurants over the exact same periods, before and after treatment:

$$Y_{r,t} = \beta_1 * post_{rt} + \beta_2 * (post_{rt} \times D_{rt}) + \beta_3 * open_{rt} + \eta_h + \eta_r + \varepsilon_{r,t} \quad (6)$$

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 β_2 coefficient on the $post_{rt} \times D_{rt}$ captures the post-treatment effect for treated restaurants. The treated-control pairs are matched exactly across periods: for each treated restaurant in period t there must be a matched control observation in period t. We also require all restaurants to have at least one

[16] 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 certain key nouns in 10-K forms filed with the SEC.

[17] We exclude matches where the menu distance is zero; these are likely different branches from the same local chain of restaurants.



(a) Cumulative distribution function of menu distance between pairs of restaurants that share all cuisines, some (but not all) cuisines, and no cuisines.

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

Fig. 5. Cumulative distribution functions for restaurants in selected cuisines.

valid (non-missing and not an outlier) pre-treatment observation and one valid post-treatment observation. Since we match exactly by time, we do not include time period fixed effects; by construction, treatment status must be uncorrelated with the time period. However, in order to deal with the potential noise created by time of day effects, 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 et al. (2017), we note that treatment status is assigned to a cluster of restaurants based on common entrants, and therefore calculate standard errors clustered at the level of the entrant-group generating the treated status. We use entrant-group, rather than entrant, since when there are multiple entrants two incumbents may face a common entrant but have additional entrants that differ. In many tables we show 95% confidence intervals in order to emphasize that even the magnitudes of the upper and lower bounds of our estimates are small.

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. We include restaurant-item fixed effects so that the coefficient on the post-treatment indicator $post_{rt} \times D_{rt}$ is only estimated from changes to items that are observed in both the pre-entry and post-entry periods. The specification is similar to the restaurant-level equation, but drops the time-of-day fixed effects and includes the restaurant-item level fixed effects, η_{ir}:

$$ItemPrice_{i,r,t} = \delta_1 * post_{rt} + \delta_2 * (post_{rt} \times D_{rt}) + \eta_{ir} + \varepsilon_{r,t} \tag{7}$$

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 (7) 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).

As a first exhibit, we show event study plots of simple means in Appendix Figure A8. For each relative period around treatment, we calculate the mean of the treated restaurants and matched control restaurants.[19] In these plots there is no indication of a post treatment jump nor a pre-treatment trend for any variable across all durations. In fact, the trends look quite close to parallel, both before and after treatment. There is a significant difference in the average pre-treatment item count, with the menu of control restaurants about 10% longer than treated. This difference in levels is not a threat to identification in our regression specifications—we include restaurant fixed effects—but we still assess whether this could affect our results later in this section (see footnote [20]). We next present regression-based event study plots for our main variables and all three durations (4,6,8) in Fig. 6.[21] The plots in Fig. 6 show little evidence of pre-trends or sharp discontinuities after the entry window.

[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 a single control restaurant could be matched to multiple treated restaurants 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 × 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] We do not show plots of the item-level prices since a simple mean calculated across restaurants, which vary dramatically in item counts, has no easy interpretation. In Appendix section B.4 we also estimate a long difference version of specification (6) on the *unmatched* sample for completeness. These specifications show no evidence of a treatment effect except for a small decrease in the 95th percentile price for the 4 week duration.

[20] As noted earlier and shown in our balance table (Table A4) and raw means plots (Figure A8), there are post-match differences in item count between treated and control groups. While this difference appears constant over time, we still explored whether this difference could be obscuring treatment effects. To do so, we re-ran our matching procedure but added the additional constraint that the difference in item count, on the first observed menu, for treated and matched control restaurants could be no larger than 100 items. This filter reduced the average pre-treatment difference in item count across treated and control to around 6 items for all durations, or about 4% of average menu length. We then estimated the specifications shown in Table 3, but found very similar results for all variables and durations (results available upon request).

[21] Letting k_r indicate the treatment period for restaurant r, the restaurant-level event study specification is: $Y_{r,t} = \sum_{j=-d}^{-d/2} \beta_j * \mathbf{1}(j = t - k_r) + \sum_{j=1}^{d} \beta_j * \mathbf{1}(j = t - k_r) + \eta_t + \eta_r + \varepsilon_{r,t}$. Note that we do not estimate coefficients within the entry window, $(-d/2, 0]$, and we normalize $\beta_{-d/2}$ 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 weight by inverse item count.

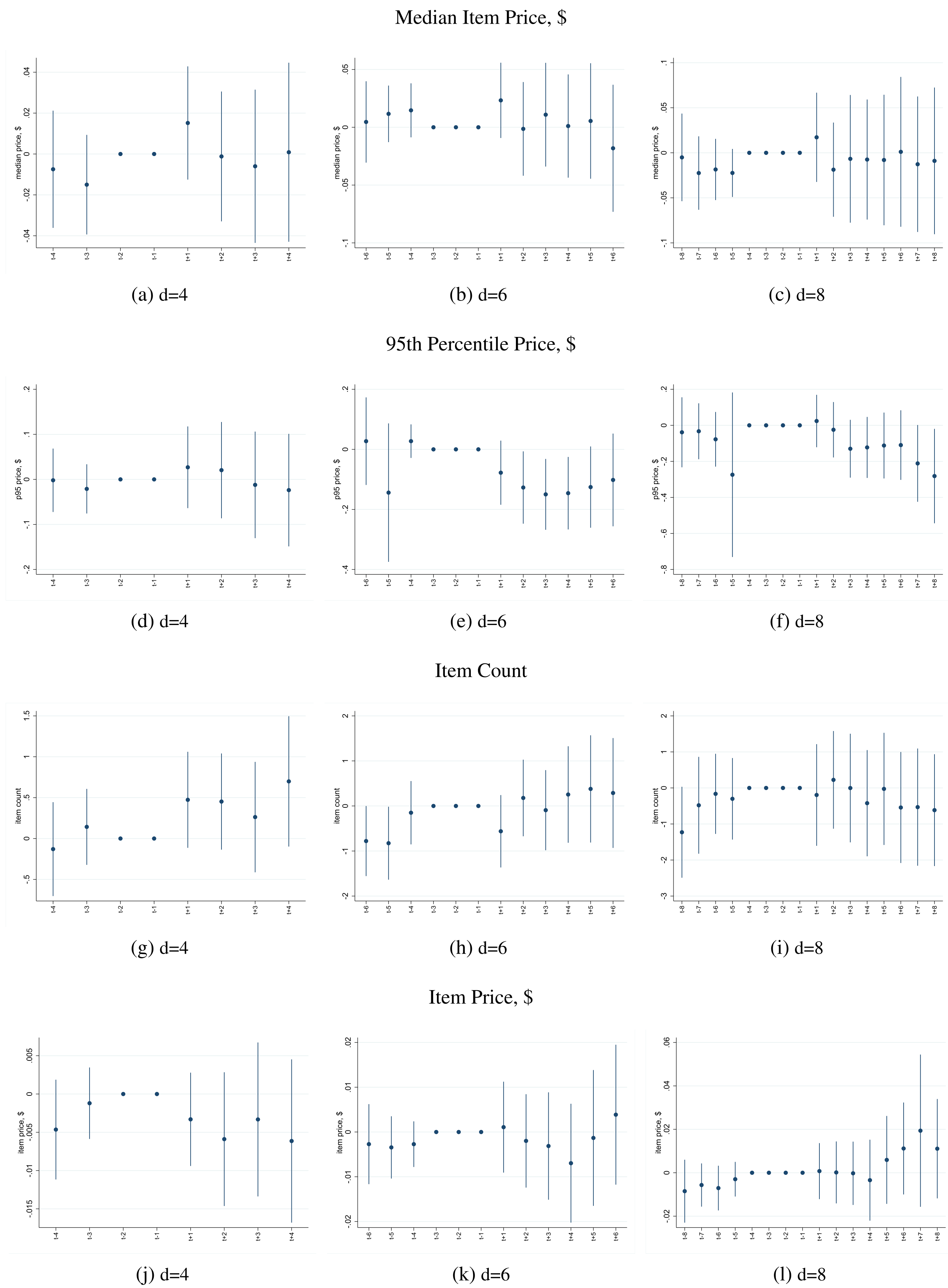


Fig. 6. Event study plots for competition in geographic space.

Table 3
Matching results for competition within 500 m.

	(1) Med Prc	(2) Ln Med Prc	(3) p5 Prc	(4) p95 Prc	(5) Itm Ct	(6) Itm Prc
			(a) Four period duration			
treated × post	0.009	0.001	−0.008	0.022	0.478*	−0.003
	[−0.020,0.038]	[−0.002,0.004]	[−0.025,0.010]	[−0.091,0.134]	[−0.091,1.047]	[−0.012,0.006]
post	0.024**	0.003***	0.014**	0.045	0.070	0.029***
	[0.004,0.045]	[0.001,0.006]	[0.003,0.025]	[−0.056,0.147]	[−0.276,0.416]	[0.023,0.036]
open	−0.023***	−0.002**	0.005	0.021	2.169***	
	[−0.040,−0.007]	[−0.004,−0.000]	[−0.005,0.014]	[−0.038,0.081]	[1.698,2.640]	
Observations	20,652	20,652	20,652	20,652	20,652	2,797,319
Clusters	371	371	371	371	371	371
Treated	1944	1944	1944	1944	1944	1943
DepVarMean	8.32	2.06	2.42	17.58	147.88	8.66
			(b) Six period duration			
treated × post	−0.009	−0.001	0.015*	−0.084	0.509	0.001
	[−0.051,0.034]	[−0.005,0.004]	[−0.001,0.030]	[−0.214,0.046]	[−0.416,1.434]	[−0.011,0.014]
post	0.069***	0.009***	0.006	0.159***	0.159	0.039***
	[0.034,0.104]	[0.005,0.012]	[−0.005,0.016]	[0.057,0.261]	[−0.355,0.672]	[0.030,0.047]
open	−0.025**	−0.004**	0.004	−0.034	1.837***	
	[−0.049,−0.001]	[−0.007,−0.000]	[−0.005,0.012]	[−0.093,0.024]	[1.273,2.402]	
Observations	16,944	16,944	16,944	16,944	16,944	2,373,665
Clusters	296	296	296	296	296	296
Treated	1246	1246	1246	1246	1246	1243
DepVarMean	8.12	2.04	2.31	17.36	153.44	8.50
			(c) Eight period duration			
treated × post	0.007	0.003	−0.004	−0.047	0.155	0.011
	[−0.061,0.076]	[−0.003,0.009]	[−0.027,0.018]	[−0.228,0.135]	[−0.972,1.282]	[−0.007,0.029]
post	0.076**	0.009***	0.018**	0.145**	0.417	0.042***
	[0.018,0.134]	[0.004,0.013]	[0.001,0.034]	[0.026,0.263]	[−0.264,1.098]	[0.031,0.054]
open	−0.010	−0.001	0.004	0.037	1.945***	
	[−0.040,0.021]	[−0.005,0.003]	[−0.009,0.017]	[−0.048,0.122]	[1.333,2.557]	
Observations	12,180	12,180	12,180	12,180	12,180	1,756,346
Clusters	211	211	211	211	211	211
Treated	703	703	703	703	703	702
DepVarMean	8.10	2.04	2.28	17.21	158.64	8.49

The fourth column shows results from an item-level regression. All specifications include restaurant fixed effects. In brackets we show 95% confidence intervals derived from standard errors clustered by entrant. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

A couple of the plots, such as the 95th percentile price plot for the 6 week duration (panel e) and item price plot for the 8 week duration (panel l), suggest a possible post-treatment response, although the confidence intervals mostly overlap zero. However, as we will emphasize throughout the paper, the point estimates and confidence intervals are quite small. For example, the $t+3$ point estimate for the six week duration 95th percentile price, one of the larger coefficients, is less than 1% of the mean restaurant's 95th percentile price and the maximum effect from the confidence interval (the lower bound) is less than 2%. In fact, the confidence intervals for every median price, item count, and item level price coefficient in Fig. 6 bound the point estimates to less than 1% of the mean restaurant's value.

In Table 3 we present the regression results from our two specifications for the three durations, adding the logarithm of median price and the 5th percentile price as dependent variables. Across the eighteen regressions, the post-treatment effect for the treated is small and statistically insignificant at the 5% level. The coefficients on item count for the four week duration and the 5th percentile price for the six week duration are significant at the 10% level, but the magnitudes are both less than 1% of the mean restaurant's value (dependent variable means are listed at the bottom of each column). In fact, the magnitude of the treatment effects across all variables 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 $d=4$ sample is $[-\$0.020, \$0.038]$. Taking the post coefficient of $0.024 as the average increase for all restaurants, the upper bound of the treatment effect is only about one and half times the magnitude of normal price inflation, and less than one half percent of the average restaurant's median price of $8.32. In the sixth column of each subtable we present the results from the item-level specification, and find very small treatment effects with tight 95% confidence intervals, while the average changes ("post" coefficients) are somewhat similar to the median price estimates in column 1. In columns three and four we show treatment effects for the 5th percentile and 95th percentile items prices, finding no evidence that restaurants are changing prices at the lower and 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 0.3% of the average item count. The open status coefficients are positive and significant, illustrating that menus are about 2 items longer when restaurants are open. Lastly, comparing the dependent variable means across the different subtables provides evidence of moderate 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 control 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.[20]

Thus far, all of our results have been based on entry within 500 m. We now re-estimate our models using a range of inner radii from 500 m to 1500 m, keeping a spatial buffer of 100 m. Each radius defines a unique set of treated and control restaurants, which we then re-match based on predicted entrants and menu distance.[22] In Fig. 7 we plot the *treated* × *post* coefficients for each radius, dependent variable, and dura-

[22] For tractability, we use predicted entrants within 500 m to do the matching for all radii.

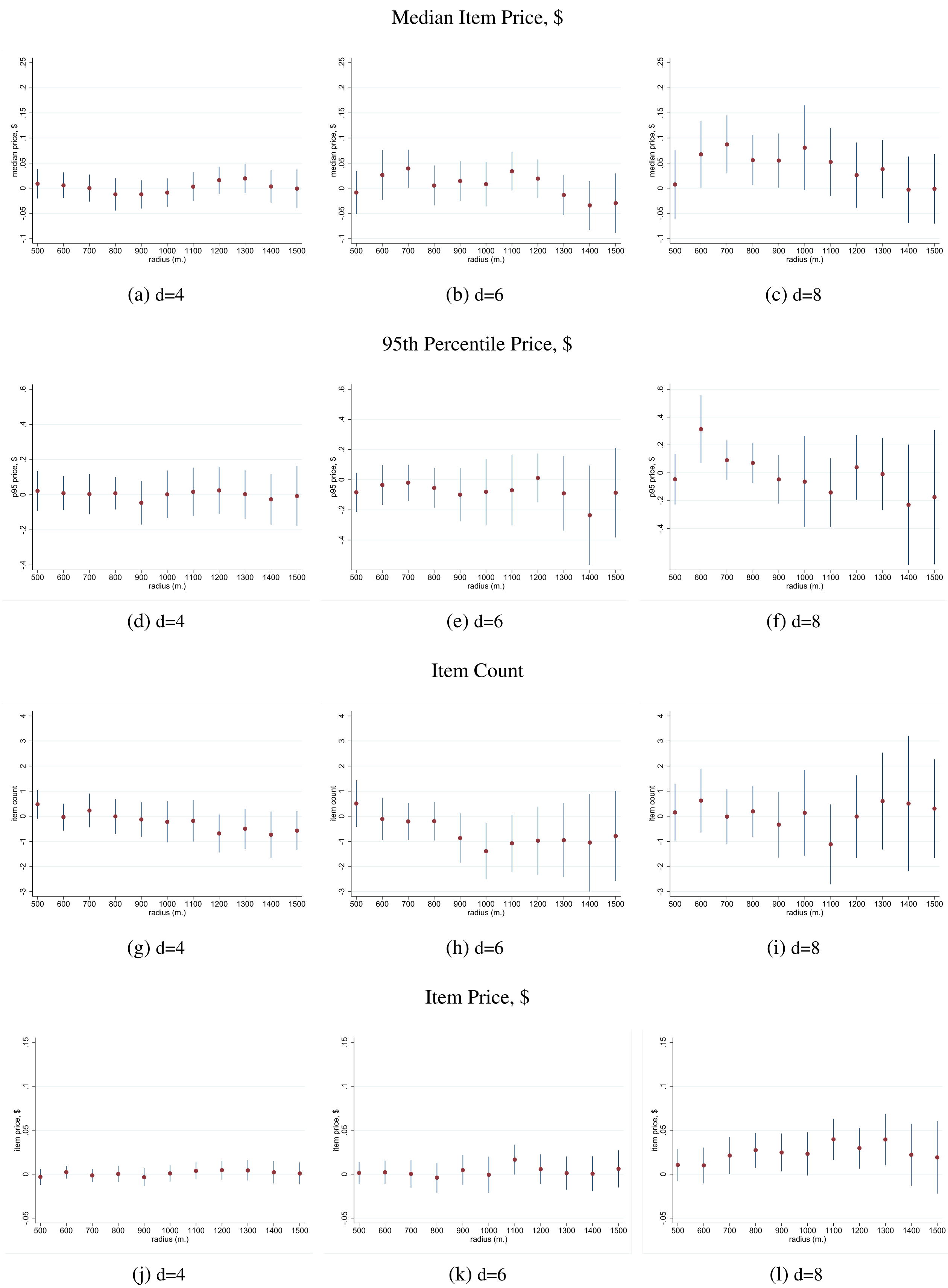


Fig. 7. Treatment effects at different spatial distances.

tion, along with 95% confidence intervals. Again, nearly all treatment effects are close to zero. For median price and the item level price in the 8 week duration (panels c and l), we do find a number of positive estimates statistically different from zero, although these confidence intervals do not take into account the large number of hypotheses tested. However, the magnitude of these estimates is still quite small—less than one percent of the average restaurant's median price—suggesting that even if this sample of restaurants is indeed raising prices in response to entry, the effect is not economically meaningful.

4.2. Robustness, heterogeneity, and complementary evidence

In Appendix section C 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, such as quality ratings and hours of service, and find no evidence that restaurants are responding through these channels. Next we run a long differences specification, with results quite similar to Table 3, and a "shifted" specification that allows for a response over a longer post-entry period. The results from the shifted specification are also similar to Table 3, but we do find two statistically significant treatment coefficients out of the 18 regressions we run. However, these are both quite small in magnitude—less than half a percent of the mean value of the outcome variable—and may simply be the result of sampling variation. We then examine response heterogeneity by restaurant characteristics, such as price or item count, to test if different types of restaurants vary in their response to competition. We also examine response heterogeneity within a restaurant's menu since a given restaurant may change prices for some items in different ways from others. Neither of these analyses suggest that heterogeneity is masking a competitive response to entry. In Section 5.2 we also compare the effect of single versus multiple entrants, showing results for both competition in geographic space and menu space.

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, as noted in the introduction, it may be difficult for new entrants to choose locations so precisely. 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 m. 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 (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). In Appendix Section D.1 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. Contrary to the hypothesis of choosing locations to soften competitive intensity, we find that similar restaurants are more likely to co-locate.

4.3. Extension: continuous measures of competition

This section uses an alternative identification strategy to estimate competitive responses. We first find sets of incumbents who are located within 1500 m of a *single* entrant within a specific duration. We use the same timing rules as outlined in Fig. 4, which implies that the single entrant appeared between $t-1$ and t, and there was no other entry within 1500 m over $[t-2d, t+2d]$. Within a set of incumbents all facing the same entrant, we then estimate whether incumbents closer to the entrant change their menus after entry differently than those further from the entrant. The identification assumption is that within 1500 m of the entrant, the distance to the entrant would be uncorrelated with menu change behavior in the counter-factual with no entry. In the specification below, η_h and η_r are hour fixed effects and restaurant fixed effects, and $open_{r,t}$ is an indicator for open status, as earlier:

$$Y_{r,t} = \beta_1 * \left(post_{r,t} \times dist_r\right) + \beta_2 * open_{r,t} + \eta_h + \eta_r + \left(\eta_{e(r)} \times post_{r,t}\right) + \epsilon_{r,t} \tag{8}$$

The key term is $\left(\eta_{e(r)} \times post_{r,t}\right)$, which is a fixed effect for incumbents within 1500 m of entrant e in the post-entry period. This term captures an average post-entry value for outcome Y for restaurants facing entrant e. Given this term, β_1 then estimates any post-entry difference in Y that varies linearly with distance to the entrant. We cluster standard errors by entrant. Since distance is a continuous variable, the equation is a form of a continuous difference-in-difference specification; if we discretized distance into just two categories (near, far) it would be a conventional DiD specification.

A possible benefit of this method over our matching method is that it may better control for unobserved local area trends, which are captured by the entrant-by-post fixed effect. On the other hand, if within the 1500 m area entrants are choosing locations based on distance to specific incumbents, or incumbent behavior varies by distance to the entrant location—say the entrant is located on top of a subway station or other point of interest—then the matching strategy would be better identified since it explicitly matches treated restaurants with similar control restaurants. That said, we did not find evidence of entrants choosing locations to maximize menu distance in the previous section and it is unclear why entrants would systematically choose points of interest more than incumbents. Another difference is that the matching strategy can identify any shared competitive response while the continuous DiD only captures a competitive response that varies by distance. For example, if all restaurants in an entrant area lower prices by the same amount—consistent with some models of monopolistic competition—then we would observe a negative coefficient in our matching specification but a zero coefficient on $post_{r,t} \times dist_r$ in Eq. (8) above. Therefore the continuous DiD is better suited to detecting spatial competition, while the matching strategy can detect a more general change in response to entry, but cannot distinguish between spatial and pro-competitive monopolistic competition (without adding interaction terms and additional assumptions).

To illustrate this method, in Appendix Figure A6 we plot symmetric differences in median price against distance to the entrant, for both geographic distance and menu distance. Both plots are flat with statistically insignificant slopes. In Table 4 we estimate Eq. (8) using geographic distance to define $dist_r$ in the first three columns and standardized menu distance in the second three columns. We require restaurants to have symmetric observations around entry: we only include a restaurant's observation in $t-w$ if we also have an observation in $t+w$. To save space, we limit the outcomes to median price, the 95th percentile price, and item count; tables with the full set of six outcomes used earlier are available upon request. Across all durations and specifications, the coefficient on $dist. \times post$ is small and statistically insignificant at the 5% level (this is also true for the outcomes not shown). For geographic distance, one of the largest (still insignificant) point estimates is for median price in the 8 week duration (column 1 of panel C) and implies that a one kilometer increase in distance from the entrant leads to a \$0.097 decrease, which is about 1% of the average restaurant's median price. In column 4 of panel A the treatment coefficient is significant at the 10% level but still small: a one standard deviation increase in menu distance implies a \$0.03 decrease in the change in price after entry. Thus the difference between price changes for restaurants that are three standard deviations away from each other, which implies very different menus, is just \$0.09, or slightly larger than 1% of the average restaurant's median price.

Table 4
Continuous DiD using spatial and menu distance.

	(1) Med Prc	(2) p95 Prc	(3) Itm Ct	(4) Med Prc	(5) p95 Prc	(6) Itm Ct
			(a) Four period duration			
dist. × post	−0.031	−0.036	−0.012	−0.030*	−0.036	−0.152
	[−0.094,0.032]	[−0.202,0.129]	[−2.106,2.082]	[−0.064,0.004]	[−0.112,0.040]	[−0.807,0.502]
open	−0.010	−0.008	2.392***	0.022	−0.042	2.043***
	[−0.047,0.027]	[−0.073,0.057]	[1.664,3.120]	[−0.029,0.073]	[−0.101,0.018]	[1.051,3.036]
Observations	7002	7002	7002	3290	3290	3290
Clusters	154	154	154	56	56	56
Treated	1391	1391	1391	596	596	596
DepVarMean	8.36	18.86	159.93	8.42	19.23	159.03
Dist. measure	km	km	km	m. dist	m. dist	m. dist
			(b) Six period duration			
dist. × post	−0.054	−0.017	0.064	−0.024	−0.028	−0.475
	[−0.172,0.064]	[−0.261,0.228]	[−5.742,5.871]	[−0.071,0.022]	[−0.069,0.013]	[−1.244,0.294]
open	−0.004	−0.044	1.803**	0.022	−0.070	1.328
	[−0.040,0.031]	[−0.139,0.050]	[0.403,3.204]	[−0.030,0.074]	[−0.169,0.030]	[−0.630,3.285]
Observations	4146	4146	4146	1956	1956	1956
Clusters	114	114	114	46	46	46
Treated	722	722	722	319	319	319
DepVarMean	8.24	18.30	165.09	8.00	18.19	167.38
Dist. measure	km	km	km	m. dist	m. dist	m. dist
			(c) Eight period duration			
dist. × post	−0.097	−0.191	0.583	−0.024	−0.027	−0.370
	[−0.253,0.059]	[−0.524,0.142]	[−5.782,6.947]	[−0.073,0.024]	[−0.068,0.015]	[−1.091,0.350]
open	−0.010	−0.092*	1.962**	0.008	−0.158**	1.623
	[−0.068,0.048]	[−0.191,0.008]	[0.303,3.622]	[−0.076,0.091]	[−0.292,−0.025]	[−0.793,4.039]
Observations	3434	3434	3434	1748	1748	1748
Clusters	81	81	81	32	32	32
Treated	457	457	457	229	229	229
DepVarMean	8.16	17.95	169.03	8.00	17.91	166.95
Dist. measure	km	km	km	m. dist	m. dist	m. dist

All specifications include restaurant and entry-by-post fixed effects, standard errors clustered by entrant are shown in parentheses. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

5. Competition in product space from site entrants

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 geographic distance but rather distance in product space. In this section we define treatment as entry cases where the menu distance between entrants and an incumbent is within a specified low threshold. We now define an entrant as a new restaurant on Grubhub (the "site entrants" in the lower panel of Fig. 1), rather than a new restaurant opening a storefront on a street in New York, as shown in Fig. 2. Therefore the results in this section measure competition on the delivery platform, which may differ from competition in the physical market.

5.1. Results: competition in product space

For a given incumbent restaurant, we define treatment as a new entrant on Grubhub within a specified *menu distance percentile*, and within 1.5km in geographic space. We calculate the menu distance percentile from all pairwise menu distances observed in our data and use this to define ρ_T. Our baseline analysis uses the 2nd percentile, meaning that we define competition as a new entrant whose menu is closer to the incumbent's menu than 98% of all pairwise menus. 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 product space, then consumers are choosing among restaurants with similar cuisines over geographic distances that are likely significantly larger than the 500 m baseline 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 1.5 km.[23] 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 Fig. 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 Fig. 4). Analogous to the outer radius of 600m, in this analysis we use an outer radius equal to the 5th percentile of all pairwise menu distances, or a menu distance buffer of three percentiles. Thus, a treated restaurant faces entrants within the 2nd menu distance percentile only during the entry window and no other entrants within the 5th menu distance percentile over $4d$ weeks. A control restaurant has no entrants within the 5th menu distance percentile over the same $4d$ 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

[23] 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 km as a conservative distance within which all delivery restaurants should compete.

Table 5
Matching results for competition within 2nd percentile of menu distance.

	(1) Med Prc	(2) Ln Med Prc	(3) p5 Prc	(4) p95 Prc	(5) Itm Ct	(6) Itm Prc
	(a) Four period duration (menu distance treatment)					
treated × post	−0.023	−0.002	−0.006	−0.392**	−0.330	−0.002
	[−0.069,0.024]	[−0.007,0.003]	[−0.028,0.016]	[−0.766,−0.019]	[−1.374,0.713]	[−0.030,0.026]
post	0.059***	0.006***	0.012	0.248***	0.305	0.046***
	[0.027,0.091]	[0.003,0.009]	[−0.003,0.027]	[0.085,0.410]	[−0.539,1.150]	[0.031,0.061]
open	0.037*	0.005**	0.012	−0.320	3.334***	
	[−0.003,0.076]	[0.000,0.010]	[−0.006,0.030]	[−0.726,0.086]	[2.310,4.358]	
Observations	8046	8046	8046	8046	8046	1,174,642
Clusters	395	395	395	395	395	395
Treated	750	750	750	750	750	749
DepVarMean	9.03	2.15	2.57	19.40	157.82	9.30
	(b) Six period duration (menu distance treatment)					
treated × post	0.008	0.002	−0.001	−0.058	0.140	0.004
	[−0.038,0.055]	[−0.003,0.008]	[−0.022,0.020]	[−0.263,0.146]	[−0.672,0.951]	[−0.030,0.037]
post	0.058***	0.006***	0.018***	0.207***	0.583**	0.049***
	[0.025,0.090]	[0.003,0.009]	[0.005,0.031]	[0.064,0.349]	[0.057,1.110]	[0.033,0.065]
open	−0.004	0.001	−0.006	−0.070	2.519***	
	[−0.033,0.026]	[−0.003,0.005]	[−0.017,0.005]	[−0.163,0.023]	[1.654,3.385]	
Observations	11,016	11,016	11,016	11,016	11,016	1,640,854
Clusters	513	513	513	513	513	512
Treated	917	917	917	917	917	914
DepVarMean	8.77	2.13	2.44	19.10	161.15	9.19
	(c) Eight period duration (menu distance treatment)					
treated × post	0.023	0.002	0.002	0.165	1.008*	0.015
	[−0.041,0.087]	[−0.005,0.010]	[−0.023,0.027]	[−0.064,0.394]	[−0.139,2.154]	[−0.012,0.042]
post	0.077***	0.010***	0.017*	0.105**	−0.161	0.040***
	[0.033,0.122]	[0.005,0.015]	[−0.003,0.036]	[0.004,0.205]	[−1.019,0.698]	[0.024,0.057]
open	−0.008	0.001	−0.004	−0.109**	3.166***	
	[−0.059,0.043]	[−0.005,0.006]	[−0.020,0.013]	[−0.205,−0.014]	[2.162,4.171]	
Observations	8028	8028	8028	8028	8028	1,306,574
Clusters	309	309	309	309	309	309
Treated	539	539	539	539	539	538
DepVarMean	8.59	2.11	2.38	18.45	175.23	8.99

The sixth column shows results from an item-level regression. All specifications include restaurant fixed effects. In brackets we show 95% confidence intervals derived from standard errors clustered by entrant group. Significance levels: *** 1 percent, ** 5 percent, * 10 percent.

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 5, using the same format as earlier. In comparison with the geographic space competition results in Table 3, there are more entrant groups (shown in "Clusters" row) but fewer treated restaurants per entrant group. The precision of the estimates is roughly comparable in both tables, with the confidence intervals slightly larger in the product space table. The sample in Table 5 has higher average prices and longer menus (greater item count), and the post coefficients are also slightly higher than in Table 3. Across all eighteen specifications, only the negative coefficient on the 95th percentile price in the four week duration is statistically significant, with a magnitude equal to about 2% of the average restaurant's 95th percentile price. For the six and eight week durations, the coefficient is insignificant and even positive. In Appendix Figure A9 we plot the post-treatment coefficient from samples using different menu distance percentiles as the inner radius ρ_T, keeping a buffer of three percentiles, analogous to Fig. 7 for geographic space. The coefficients are small and statistically indistinguishable from zero, with nearly every point estimate less than one percent of the dependent variable. Panel d shows that the statistically significant coefficient on the 95th percentile price is the largest coefficient across the thirteen different menu distance percentiles, with most coefficients less than half of the magnitude. We cannot rule out the possibility that for this particular specification—competition in product space over a four week duration using the 2nd menu distance percentile to define competition—there is a negative effect only at the upper end of the menu. However, given that we find no other evidence consistent with this coefficient, still small at 2%, we think it unlikely.

5.1.1. *Heterogeneity across markets defined by cuisine*

In section C.2 we investigated heterogeneity across markets defined in geographic space. Models of monopolistic competition also allow for markets defined in product space, such as cuisine, to have heterogeneous competitive effects. For example, with a variable elasticity of substitution it is possible that an additional Italian restaurant causes incumbent Italian restaurants to decrease prices while an additional Japanese restaurant causes incumbent Japanese restaurants to raise prices. If pro-competitive effects in some cuisine markets are offset by anti-competitive effects in others, then the average effect may be zero.[24] We explore this possibility in Appendix section D.3 by running our baseline menu distance specification separately by cuisine. The point estimates for a couple cuisines ("Latin American", "Hamburgers") are significantly different from zero for some durations. Nonetheless, most point estimates are concentrated around zero and we do not see much evidence of strong heterogeneity.

5.2. *Multiple entrants*

Some restaurants receive multiple entrants while others receive a single entrant over the same window length. We now investigate whether the small average responses to entry we have found thus far could be masking larger responses when incumbents face multiple new entrants. We examine multiple entrants in both geographic space and product

[24] We thank a referee for this example.

space. However, a caveat to this analysis is that relatively few treated restaurants face multiple entrants, and when they do, the count of entrants is limited.[25] The reason for this pattern is our requirement that treated restaurants have no entry outside of the entry window, which again, is necessary for our identification strategy of comparing a pre-period with a post-period. There are parts of New York City where restaurants face many new entrants within the entry window, especially for large ρ_T, but then these restaurants also face many new entrants in the periods preceding and following the entry window. It's worth emphasizing that we are studying very close distances, or very similar restaurants: the difference between one new competitor and two or three competitors within 500 m (or with a very similar menu) could still be a significant increase in competition.

We split our $treated \times post$ variable into cases with one entrant and cases with multiple entrants.[26] Since treated and control restaurants are matched by predicted entrants, and restaurants receiving multiple entrants may be in different areas than those receiving single entrants, we also split the *post* variable into two cases to control for effects general to areas with a higher number of predicted entrants. Let E_{rt} be the number of entrants a treated restaurant r receives, with E_{rt} also assigned to the matching control. We estimate:

$$\begin{aligned} Y_{r,t} = {} & \beta_1 * (post_{rt} \times \mathbf{1}[E_{rt}=1]) + \beta_2 * (post_{rt} \times D_{rt} \times \mathbf{1}[E_{rt}=1]) \\ & + \gamma_1 * (post_{rt} \times \mathbf{1}[E_{rt}>1]) + \gamma_2 * (post_{rt} \times D_{rt} \times \mathbf{1}[E_{rt}>1]) \\ & + \beta_3 * open_{rt} + \eta_h + \eta_r + \varepsilon_{r,t} \end{aligned} \tag{9}$$

In the above specification, β_2 captures the difference between the post entry response for restaurants facing a single entrant and their matched controls, while γ_2 captures the treatment effect for restaurants facing multiple entrants compared to their matched controls. Thus Eq. (9) is somewhat similar to splitting the sample and estimating separate specifications for single and multiple entrant cases. We estimate Eq. (9) using median price as the outcome Y_{rt} and plot estimates of β_2 and γ_2. The first row of plots in Fig. 8 shows competition in geographic space using a range of inner radii from 500 m to 1500 m, and for all three durations. The confidence intervals for the multiple entrant indicator are wider than for the single entrant indicator since there are far fewer cases. Nonetheless, the point estimates do not show any consistent pattern: in each plot the coefficients for multiple entrants are both larger and smaller than those for single entrants, depending on the radius. The second row of plots shows competition in product space, with the coefficients on the multiple indicator quite close to those on the single indicator. Therefore, at least for cases with relatively few entrants, our results do not suggest a competitive response when facing multiple entrants. However, we cannot extrapolate our results to cases with many entrants (ex: 10 entrants).

6. 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 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 500 m of every restaurant's (eventual) location, where entry is again inferred from inspections and Yelp (see Section 2.2). We then estimate the causal effect of this entrant count on the hazard of exit for restaurants in our dataset from November 27, 2016 onwards.

6.1. 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. Below we give an outline of the strategy and provide a detailed description in Appendix section E.

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

[25] For competition in geographic space, using an inner radius of 500 m (ρ_T = 500 m) and a four week duration ($d = 4$), nearly 93% of treated restaurants face a single entrant, 6.9% face two entrants, and about 0.2% face three entrants. The percentage facing multiple entrants increases to 14% for $d = 4$ and $\rho_T = 1500$ m, and is higher for the other durations, ranging between 12% and 17% for $d = 6$ and between 8% and 18% for $d = 8$. For our analysis of Grubhub site entrants—competition in product space—multiple entry is somewhat more common, ranging from 15% to above 40%, and generally increasing with the menu distance percentile. The parameter set with the largest count of multiple entrants, $d = 6$ and a menu distance percentile of 12 ($\rho_T = 12$), has 42% of treated incumbents facing multiple entrants: about 22% facing two entrants, 10% facing three entrants, 5% facing four entrants, and another 5% facing five or more entrants. Nonetheless, for both dimensions of competition, most treated restaurants face just a single entrant, and the majority of multiple entrant cases have two entrants.

[26] We also tried a linear specification where we interacted the count of entrants, E_{rt}, directly with $post_{rt} \times D_{rt}$ and $post_{rt}$. The estimated coefficients were quite similar to those from the main specification in Table 3.

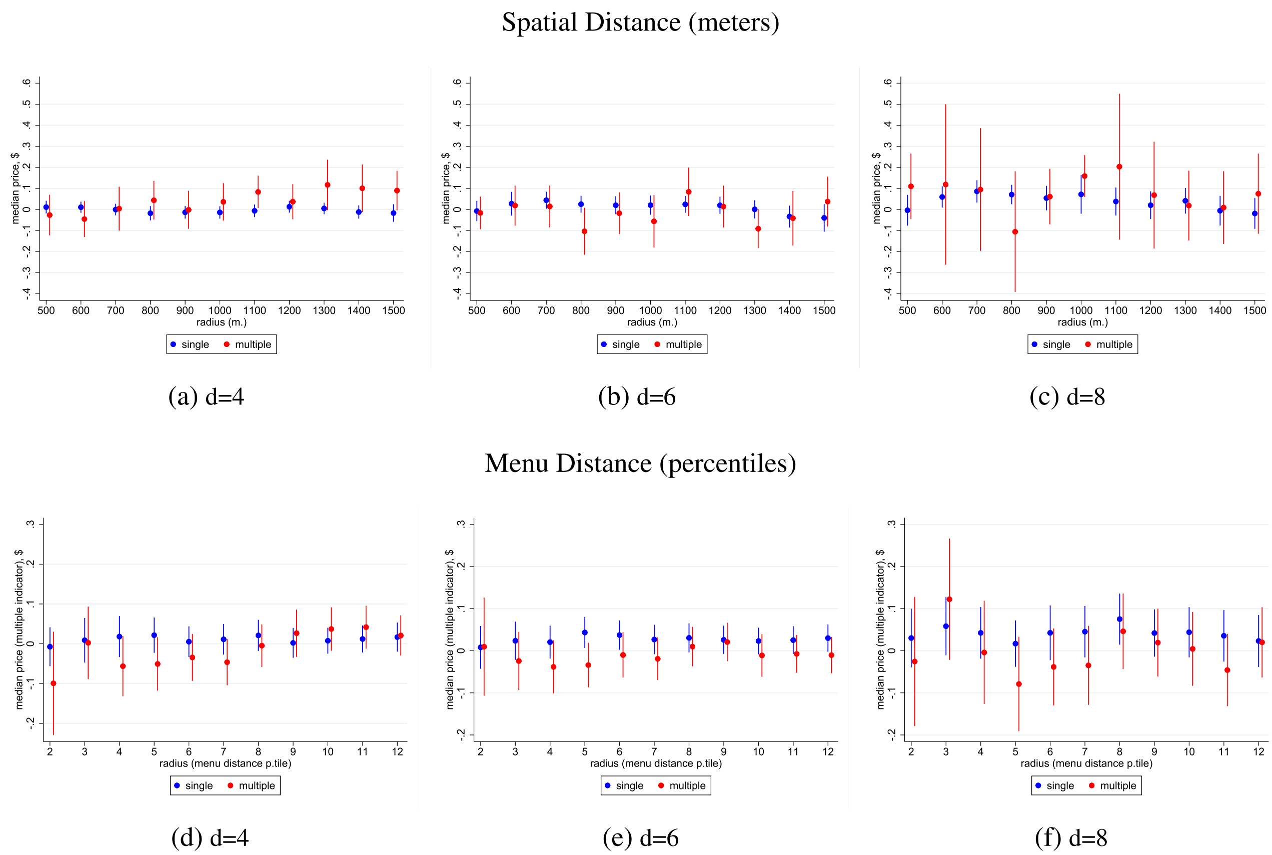


Fig. 8. Single versus multiple entrants, median price response.

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.[27] 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. In our application, we model the hazard a restaurant exits in any one week using a Cox proportional hazard model, with the number of entrants as the independent variable of interest. We calculate the dose-response function as the relative hazard of exit at a "dose" of n entrants. This estimated dose-response function shows the effect of being in a location with a given (pre-period) entry rate on the likelihood of later exit, and thus allows us to test whether greater competition (more entry) increases exit.

6.2. Exit analysis results

We start our analysis with 11,200 unique restaurants for which we have matching demographic characteristics and can predict pre-period entrant counts, and then apply two filters. First, we restrict the sample to only those restaurants observed from the first period of our data, which is about two-thirds of unique restaurants and is the same restriction we used when analyzing competition in product space in Section 5. Some of the restaurants that enter our data after the first period also exit after a short duration, behavior that is more likely to reflect exit from the delivery site than exit from the market. However, even if we drop all restaurants that survive fewer than ten periods, restaurants that enter after period 1 have a 52 week survival rate that is about 15 percentage points lower than the those we observe from the first period. This could reflect additional exits from the site but not the market, or may indicate a cohort effect. Therefore, for simplicity we focus only on those restaurants observed from the first period. Second, we drop restaurants whose GPS values are outside of a common support.[28]

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 Fig. 9 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

[27] Our estimation is also informed by the discussion of the GPS in Flores et al. (2012) and in Austin, 2019, who discusses using the GPS for survival modeling.

[28] 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 14 restaurants.

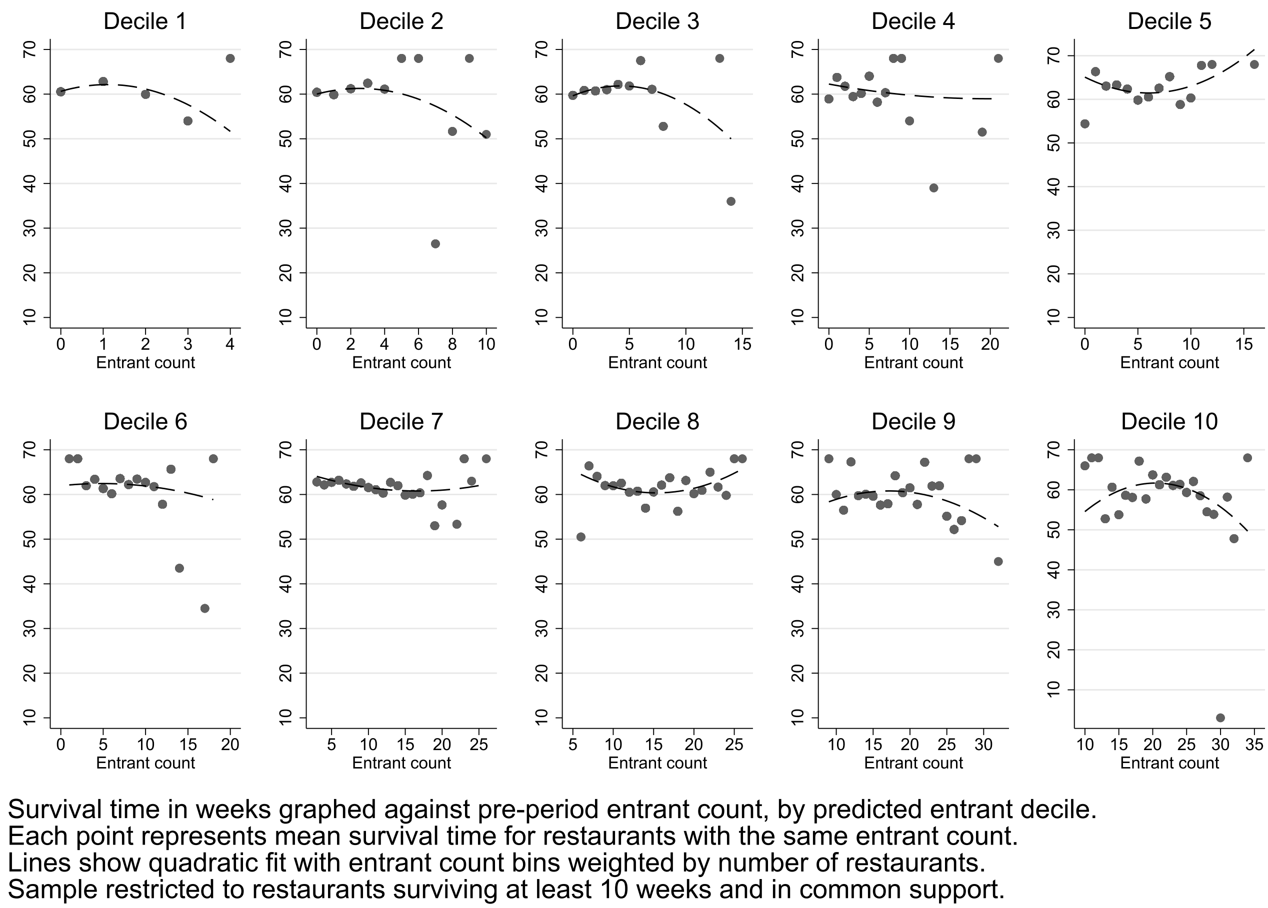


Fig. 9. Survival time against pre-period entrant count, by predicted entrant count decile.

OLS regressions. In Appendix Table A.11 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 much 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 and calculate the dose response function using the coefficients from our preferred model (see Appendix section E for discussion of choosing our preferred model, as well as testing the proportional hazards assumption). Fig. 10 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. 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 Fig. 10 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 26 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.[29] After 365 days, 86.6% of restaurants in the first decile are predicted to survive compared to 83.6% in the highest entry decile, implying that the 52 week failure rate in the top entry decile is 22% higher (16.4%/13.4%. The average 52 week failure rate across all entrant quantiles is 15%, thus the difference in failure rates is about 20% of the average (3%/15%). These results suggest that competition from new entrants 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.

7. Discussion of results

Across a large number of analyses we find little to no evidence suggesting that existing restaurants respond to new entrants by changing the prices or products observable on their menus. This result was consistent for cases with very close new competitors in both geographic and menu space, over different durations, across multiple forms of heterogeneity, and for a variety of different menu-based outcomes. We also estimated a specification where we restricted the sample to incumbent restaurants facing a new entrant—all restaurants are treated—and allowed potential responses to vary by distance (geographic, menu) to the entrant. This specification is our most direct test of the spatial competition model, but again we found no significant response to entry. Thus, for the New York City restaurant market, our results provide no evidence for the spatial competition model where firms compete locally and respond strategically to the closest competitors.

Our finding of no change in price is consistent with a constant elasticity of substitution model of monopolistic competition. However, our results could also be explained by a number of other models and we are not concluding that the restaurant market in New York City narrowly con-

[29] 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 $S_0(t)^{rh_q}$.

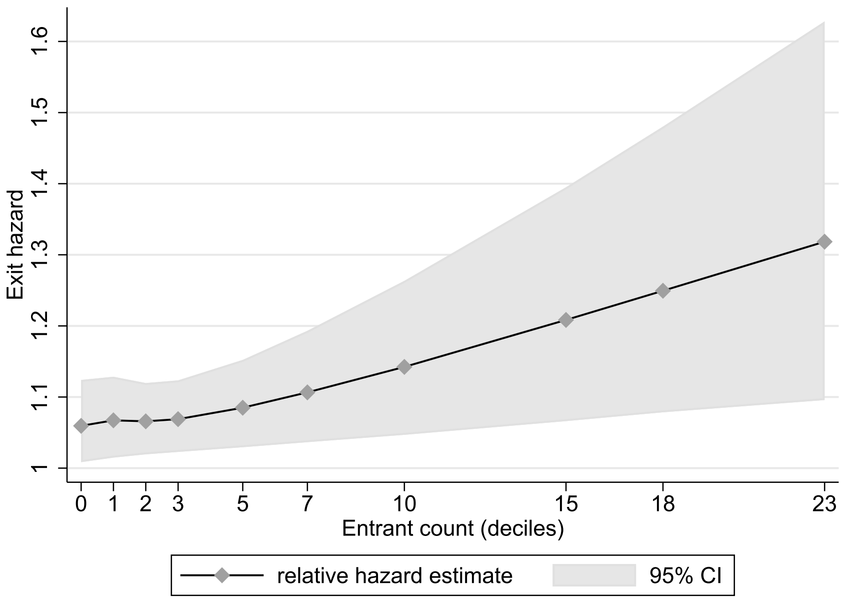


Fig. 10. Effect of entrant intensity on exit hazard. Plot shows the estimated relative hazard plotted at the median of each entrant count decile. The 95% confidence intervals are calculated from 1000 bootstrap samples.

forms to the CES model. As emphasized by Thisse and Ushchev (2018), variable elasticity of substitution models can lead to a broad set of outcomes, including small competitive or anti-competitive entry responses, which we then estimate as a zero response. Another possibility is the mechanism discussed in Gabaix et al. (2016), who show that in random utility models of monopolistic competition where consumer tastes follow a set of standard distributions, the competitive effects on mark-ups are quite small when the number of firms is large. A third potential explanation is that restaurants actually do compete locally, but that the density of restaurants is so high in New York City that the entry of an additional one or two restaurants induces a very small price change that is not discernible in our data. We tried to test for this possibility by comparing the response to entry across areas that vary in population density and the number of incumbent competitors (Appendix Table A.10), and by comparing cases of single entry versus multiple entry (Fig. 8), and did not find large differences in the comparison. Nonetheless, the population density and number of restaurants in New York City is orders of magnitude larger than in smaller US cities Schiff (2015), and therefore it is possible that many areas of the city are already near this competitive limit. While all of these models imply that the response to entry in large markets is small or non-existent, they make different predictions about whether mark-ups approach zero (perfect competition) as the number of firms becomes very large. Unfortunately, to try and distinguish between these different models would require data on sales and costs, which we do not have.[30] Instead, we interpret our results as a fairly robust example of minimal strategic interactions in a large differentiated market, and as therefore providing empirical support for one of the central assumptions of all monopolistic competition models.

Lastly, as noted in the introduction, our dataset has several constraints that limit our conclusions: our data on restaurants is mostly confined to online menus, not all restaurants use Grubhub, and we only observe restaurants in a single large city. First, with only online menus, we cannot refute the possibility that restaurants primarily respond to competition on dine-in menus or through dine-in service changes, such as improvements in decor or service quality. However, in Section 5 we examined the response to competition from Grubhub site entrants, rather than physical entrants. In this situation incumbents would be most likely to respond with changes to their online menu, yet again, we found no consistent response. Second, if there is selection of restaurants into Grubhub, then we cannot easily extrapolate our results to other incumbent restaurants not on the site. Nonetheless, a third of New York City restaurants were already on Grubhub during our sample period, making this an important segment of the market. Further, the share of restaurants offering delivery has risen dramatically as a result of the pandemic, and may remain high Ahuja et al. (2021). Finally, while we think that our results for New York City are likely applicable to other large and dense restaurant markets, we acknowledge that they may be less relevant to much smaller markets. Studying menu changes across restaurant markets of different sizes could show whether strategic interactions are absent in most cities or depend crucially on market size.

8. 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. Our primary approach to this potential endogeneity was a matching strategy that balanced location characteristics with 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). We also complemented this matching strategy with a continuous treatment specification comparing incumbent restaurants within a given radius of an entrant, but that vary in distance to the entrant.

Our findings suggest that incumbent restaurants do not change their menus in response to competition from new entrants. We examined com-

[30] In addition to these models of competition, some papers argue behavioral considerations and product design constraints may result in weak competitive responses. Arcidiacono et al. (2020) suggest that the failure of supermarkets to respond to Walmart despite revenue loss stems from managerial inattention. Another 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 et al., 2018. 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).

petition from entrants in both geographic and product space (menu distance) across a large set of specifications. 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. While there is noise in some of our menu data, the size of our panel and the high entry rates in the industry allow us to estimate fairly precise confidence intervals; even the 95% upper bound for most estimates is economically small. These findings are consistent across a number of robustness checks examining different outcomes, competitive distances, durations, and heterogeneity in the characteristics of incumbent restaurants and local areas. Further, we do not find any evidence that entrants strategically select locations to mitigate competition. However, we do find that restaurants in areas with many entrants are likely to exit the market sooner. Our results are broadly consistent with monopolistic competition models where firms ignore the actions of any individual competitor. In the context of large markets, assuming away local competition may be an empirically plausible simplification.

CRediT authorship contribution statement

Nathan Schiff: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Visualization, Writing – original draft, Writing – review & editing. **Jacob Cosman:** Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Visualization, Writing – original draft. **Tianran Dai:** Data curation, Formal analysis, Investigation, Methodology, Writing – review & editing.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2022.103509

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