

Agglomeration Economy Dynamics and Demand Spillovers: Restaurant Week Effect on Participants' Neighbors

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Abstract

Agglomeration economies generate benefits for agglomerated firms. How benefits propagate through an agglomeration is unclear because agglomerations are composed of multiple firms and each firm engages in various activities that might have spillover implications for neighbors. To observe this propagation and identify underlying mechanisms, we leverage some firms taking a specific strategic action that generates benefits for themselves that might spill over to neighbors. Focusing on demand-side (vs. supply-side) agglomeration benefits, we expect that neighbors will benefit more when geographically closer to the action-taking firms (due to lower customer search costs) or when selling similar goods to the action-taking firms (due to what customers the strategic action attracts), but benefit less when both geographically closer and selling similar goods (due to localized competition). Using a quasi-natural experiment featuring establishment-level micro-geographic data of restaurants participating in “Restaurant Week” in Washington DC (an event to stimulate demand), we find that Restaurant Week participants’ neighbors experience heightened demand for several weeks after Restaurant Week. Consistent with our expectations: neighbors benefit more when geographically closer or if selling similar goods to Restaurant Week participants. But the benefit for neighbors selling similar goods is delayed a couple weeks, which is consistent with localized competition initially offsetting spillover benefits. Our results demonstrate how firms taking specific strategic action can lead to spillovers propagating throughout an agglomeration, and how overlapping mechanisms determine who benefits and how much.

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1. Introduction

Agglomeration economies generate benefits that accrue to firms that geographically agglomerate, which can improve those firms' performance in two primary ways. Supply-side agglomeration economies can lower agglomerated firms' costs when workers and suppliers make industry-specific investments and novel knowledge spills over among firms (Marshall, 1920; Rosenthal and Strange, 2001; 2004). Demand-side agglomeration economies can increase agglomerated firms' revenues as more customers are drawn to agglomerated firms due to lower search costs for differentiated goods – as agglomerated firms offer customers greater variety (Ren et al., 2011) with lower transportation costs to assess and compare differentiated goods (Dudey, 1990; Stahl, 1982; Wolinsky, 1983).

Because of the potential to improve firms' performance, agglomeration economies have attracted strategy researchers' attention, especially supply-side agglomeration economies. Extant supply-side research highlights how firm heterogeneity affects which firms agglomerate (Shaver and Flyer, 2000), firms' location choice (Alcácer and Chung, 2007; Head et al., 1995), business formation (Buenstorf and Klepper, 2009; Stuart and Sorenson, 2003), and business performance (Rosenthal and Strange, 2003).

Strategy research on demand-side agglomeration benefits is more limited. McCann and Folta (2009) note the need to distinguish between supply-side versus demand-side agglomeration benefits; and a couple studies in demand-side agglomeration settings examine firms' location choice (Canina et al., 2005; Chung and Kalnins, 2001) and how competitors' exit affects other establishments' survival (Sharma and Chung, 2022) and other establishments' strategic choices (Ren et al., 2018).

Regardless of supply or demand-side agglomeration economies, observing specific mechanisms is difficult since an agglomeration is composed of multiple if not many firms, with each firm engaging in various activities that might have spillover implications for neighbors, and spillover

effects might take time to emerge. This makes understanding the dynamics of agglomeration benefits propagating throughout an agglomeration challenging. As a result, extant research typically treats agglomeration as a black box; to be added to or subtracted from – such as when firms enter an agglomeration or when firms exit – that affects other things, often in the longer-term – such as business formation, and business performance.

Besides firms entering and exiting, firms within an agglomeration regularly engage in strategic actions that have performance implications for other firms in the agglomeration. And research that considers firms within an agglomeration taking specific strategic actions (besides entry and exit) that has implications for other agglomerated firms is almost non-existent, as far as we know. An exception is Raj (2024) who examines whether and how much the strategic action of recording artists releasing a new product/album on Spotify helps other artists. But aside from this one strategic action in a digital setting the implications of firms taking specific strategic actions within an agglomeration have been unexplored.

To address this gap, in this paper, we leverage firms' use of a discrete, benefit creating, strategic action to assess how the benefit propagates through the firms' agglomeration: which neighbors benefit, how much, and why. We focus on demand-side (versus supply-side) agglomeration since extant knowledge is limited due to less prior research. A focus on demand-side agglomeration dovetails with a growing recognition of the demand-side's importance for firm strategy in general (Adner and Zemsky, 2006; Priem, 2007; Schmidt et al., 2024). We expect some of the heightened demand from the discrete strategic action to spill over to participants' neighbors. Therefore, we theorize and test mechanisms driving the spillover of demand-side agglomeration benefits.

The spillover to participants' neighbors will vary as a result of three potentially overlapping mechanisms. First is the fundamentals of "demand-side agglomeration economy spillovers": closer

firms benefit more because customers are more likely to cross-shop firms that are closer to one another, which heightens customer demand.¹ Second is “demand shaping spillovers” (Schmidt et al., 2024; Vinokurova, 2019): some firms use strategic action to heighten demand for their own differentiated good offering, but such demand shaping may also benefit other firms whose offerings are similar to the action-taking firms’ offerings. Third is a “localized competition” effect (Baum and Ingram, 1996; Kalnins, 2016): proximate firms that are similar to one another compete for each other’s customers, harming each other’s performance. As a result, we expect (i) neighbors to action taking firms will benefit (demand-side agglomeration spillover), (ii) with more proximate neighbors benefiting more (demand-side agglomeration spillover), (iii) with neighbors selling similar goods to the action taking firms benefiting more (demand shaping spillover), but (iv) neighbors more proximate to an action taking firm selling a similar good to benefit not as much (localized competition).

To test our expectations, for our demand-side agglomeration, we use restaurants in the Washington DC metropolitan area; and for our discrete strategic action, we use participation in “Restaurant Week” in Washington DC, an event to stimulate demand organized by the local restaurant association – where participating restaurants offer prix-fixe, three-course meals for a short time. We examine whether neighboring restaurants (“neighbors”) to action-taking firms (“Restaurant Week participants”) experience heightened demand spillovers, and heterogeneity of demand spillovers across neighbors. We use four semi-annual occurrences of Restaurant Week from 2022 to 2024. With each occurrence, about 350 restaurants participate that have about 1,500 neighbors

¹ In a demand-side agglomeration, firms experience heightened demand because customers’ search costs are lower to evaluate the firms’ differentiated goods among agglomerated firms versus if the firms had located separately. This means a firm has to be close enough to other firms for customers to include the firm in the cross-shopping among firms’ offerings.

(which we define as restaurants that are within 1/4 mile of any Restaurant Week participant) that do not themselves participate, but who may benefit from demand spillovers from participants.

After establishing the empirical antecedent that restaurants participating in Restaurant Week (“RW”) experience heightened demand during RW and for several weeks after, we find that participants’ neighbors also experience heightened demand during RW that persists for several weeks after. Neighbor restaurants benefit more when they are geographically closer (within 1/8 mile versus within 1/4 mile of a RW participant) or selling similar goods to RW participants (matched on five restaurant traits), which is consistent with demand-side agglomeration economies, and demand-shaping spillovers, respectively.

But for similar goods neighbors, the benefit is delayed a couple weeks: there is no benefit (relative to control restaurants) for the first couple weeks, but benefits do appear later, which is consistent with demand-shaping spillovers’ positive effect initially being offset by localized competition’s negative effect.

Our study makes several contributions. First, it provides insight for the demand agglomeration literature by highlighting how firms within an agglomeration taking discrete strategic actions can create demand-side spillovers, and how the demand-side spillovers propagate heterogeneously through the agglomeration. To explain the propagation, we draw on mechanisms from two other areas of strategy literature: demand-side strategy and localized competition. By doing so, we also contribute to each of these areas by: (i) highlighting agglomerations as a context with demand-side strategy implications and adding to a handful of other studies illustrating demand-shaping, and (ii) linking the localized competition literature with the agglomeration literature to explain why some firms within an agglomeration might not benefit as much as others. Overall, our results provide insight into the dynamics of some firms taking discrete strategic action leading to

demand spillovers that propagate heterogeneously throughout an agglomeration, with three mechanisms determining who benefits and how much.

We proceed as follows. Within an agglomeration, we argue how the heightened demand from some firms' discrete strategic action can spill over to their neighbors. We then introduce the three mechanisms: demand-side agglomeration spillovers, demand-shaping spillovers, and localized competition to argue that neighbors benefit more when geographically proximate or selling similar goods to action-taking firms; but neighbors benefit less when more geographically proximate to a similar goods action-taking firm. Next, we describe our agglomeration setting and firm strategic action: the Washington DC metropolitan area and some firms participating in Restaurant Week that may affect participants' neighbors. Our results follow. Finally, we discuss and conclude.

2. Theory

2.1 Demand-side agglomeration economies

Agglomeration economies can provide proximate firms with supply-side and/or demand-side benefits. Supply-side benefits include reducing proximate firms' costs of producing goods/services and/or fueling product/process innovation – when same industry firms agglomerate (Marshall, 1920) and/or when diverse firms agglomerate (Jacobs, 1969). The demand-side benefit is heightened demand for firms selling goods/services to customers.

Firms may heighten demand by agglomerating versus locating separately because agglomerated firms offer customers lower prices (Dudey, 1990), greater product diversity (Ren et al., 2011), and reduced search costs when choosing among differentiated products (Murry and Zhou, 2020; Stahl, 1982; Wolinsky, 1983). In a demand-side agglomeration, each firm can experience two opposing forces: (i) increased competition: the presence of more firms can reduce the demand for each firm, especially if firms offer the same varieties of differentiated goods/services; that is countervailed by (ii) heightened demand: the above benefits draw more customers to the

agglomerated firms versus if they located separately. Increased competition can decrease a firm's slice of the pie, but the overall pie can grow more than the slice's shrinkage; especially if firms mute the increased competition effect by not offering the same varieties of differentiated goods/services.

In a demand-side agglomeration economy, firms focus on revenue over production costs because the firms are more customer-facing. As a result, locations with many customer-facing firms are also locations with high customer demand (Kolko, 2010).

2.2 Firm actions and spillovers within a demand-side agglomeration economy

Within agglomerations, each firm is engaged in its typical economic activities, which generate agglomeration benefits. For generating demand-side agglomeration benefits, the typical activity is each firm selling goods and/or services, with each firm selling a differentiated version versus other agglomerated firms. This reduces customers' search costs for differentiated goods, which heightens demand for agglomerated firms versus if they had located separately. This is a baseline demand agglomeration benefit.

On top of firms' typical economic activities, firms within agglomerations may take additional actions to improve their performance. Firms try to improve their performance through strategic positioning (Brandenburger and Nalebuff, 1996; Porter, 1980), building and reconfiguring capabilities (Barney, 1991; Teece et al., 1997), as well as other actions. And within a demand-side agglomeration economy, firms likely take actions to improve the customer demand they experience. Focusing on demand-side firm actions, Schmit et al. (2024) reviews the demand-side literature and identifies "five value creation logics: (for firms) matching, leveraging, adapting, learning, and shaping customer preferences or market characteristics". Where value creation logic "describes a way in which firms can take into account, take advantage of, respond to, interact with, or influence demand-side characteristics at the level of specific customers, consumers, or users, or at the level of market aggregates of these."

Whatever action(s) a firm takes, when a firm is agglomerated with other firms, the firm's action affects not only its own performance but may spillover to affect neighboring firms' performance. If the firm's action heightens demand, then some of the heightened demand may spillover to neighbors.

For spillovers to occur, an antecedent is that a firm's action does heighten demand for the action-taking firm. This should be true, on average, due to self-selection. Firms will only select to take actions that improve their performance. Certainly, ex post, not all actions that firms take will improve their performance; but ex ante, firms expect actions they will take to improve their performance, otherwise they would not have selected to do so.

Some of the heightened demand from a firm's actions may spillover to the neighbors, if customers of the action-taking firm are also exposed to neighbors' offerings. If customers cross-shop a firm's neighbors. If so, some of these customers may also purchase from neighbors. Such exposure is more likely through the standard agglomeration economies mechanism of lower search cost – a customer is more likely to include another firm's offerings in their evaluation if the incremental search cost is smaller. Incremental search cost is smaller if a firm is geographically closer to another firm; in this case, if a firm is geographically close enough to be a neighbor to an action-taking firm. More formally:

H1: Heightened demand from a firm's action can spill over to that firm's neighbors.

2.3 Spillovers for neighbors: greater for geographically closer neighbors

Within an agglomeration, the distance between firms varies. Some are closer to each other, while others are farther. Greater distance between firms means greater transportation costs between them. Greater transportation costs between firms decrease the likelihood that one firm's customers will visit the other firm. In an agglomeration, firms in the periphery are farther from most other firms, which means greater transportation costs to cross-shopping those firms. Therefore, fewer

customers will cross-shop them, and therefore, firms in the periphery do not experience as much demand agglomeration benefit as firms in the core of an agglomeration. Thus, at an agglomeration level, being closer to the core – the center of activity – is more beneficial.

Similarly, when an individual firm takes strategic action, being closer to the firm taking action is more beneficial. We hypothesize in H1 that what matters is if a firm is a neighbor to an action-taking firm: whether a firm is geographically close enough so that customers' incremental search cost is smaller so that they are likely to cross-shop the firm. But among neighbors to the action-taking firm, the distance of each neighbor to the action-taking firm will vary. Some neighbors may be closer to the action-taking firm, while other neighbors may be farther. The closer the neighbor is to the action-taking firm, the lower the transportation costs between the two firms, and the higher the likelihood that the neighbor is visited by the action-taking firm's customers.

Consistent with this Sharma and Chung (2022) show that retail stores closer to an anchor store are more affected by a discontinuous change in the anchor store's performance. Therefore, a neighbor that is geographically closer to the action-taking firm is more likely to have the action-taking firm's heightened demand spillover. More formally:

H2: Demand spillovers are greater (lesser) for neighbors geographically closer (farther) to a firm taking action.

2.4 Spillovers for neighbors: greater for neighbors selling similar goods

We argue above in H1 that within a demand-side agglomeration economy, firms likely take actions to improve the customer demand they experience. For the demand-side, Schmit et al. (2024) identifies “five value creation logics”. All of these logics have a firm-specific nature. A firm (i) matches customer preferences through strategic decisions like changing its product offerings; a firm (ii) leverages its existing customer relationships; a firm (iii) adapts by developing capabilities to serve its customer needs; and a firm (iv) learns about customer preferences.

But the first four logics take customer preferences and market characteristics as fixed conditions, with the firm taking actions given these fixed conditions. Because firms using the first four logics take these conditions as fixed, and act within the fixed conditions, firms' actions within these logics are unlikely to have spillover implications for neighbors. For example, when a firm uses a learning logic to "identify the relevant characteristics of demand" (Schmit et al., 2024), the one firm pursuing learning does not change the characteristics of demand, and thus has little effect on other firms.

But the fifth logic, shaping, "wherein the firm strives to strategically shape customer and market characteristics to its advantage" (Schmit et al., 2024) treats customer preferences and market characteristics as malleable and has clear implications for other firms. Indeed, Schmit et al. (2024) note that "Importantly, one firm's effort to change customer preferences can affect all firms in the market". Prior research finds that such spillover effect occurs within agglomerations (McCann and Folta, 2009), but also within product categories (Vinokurova, 2019) and within firms using the same technology (Raffaelli, 2019).

We argue that a key determinant of whether there is a spillover effect from one firm taking demand-shaping action to its neighbors is how similar the action taking firm's goods are to the neighbors' goods.

Demand-side agglomeration benefits are based on the assumption that firms sell different varieties of differentiated goods. For example, passenger cars as a product category is a differentiated good, and in an auto mall, various dealers sell different varieties/brands of passenger cars. Assuming that firms sell differentiated goods also assumes that customers are heterogeneous: that some customers prefer one variety of differentiated goods, while other customers prefer other varieties. These heterogeneous customers can be thought of as composing multiple differentiated

segments within an overall distribution of customer demand (Adner and Zemsky, 2006; Adner et al., 2016; Benner and Waldfogel, 2023).

When a firm takes demand-shaping action, it is to increase demand from the segment of heterogeneous customers that would purchase the variety of differentiated goods that the firm itself sells, not from customer segments that wouldn't purchase the goods the firm sells. The firm's demand-shaping action likely has some specificity for the variety of differentiated goods that it sells.

Therefore, spillovers to neighbors will likely be affected by whether a neighbor offers a variety of differentiated good that is similar or not to the variety offered by the action-taking firm. If the neighbor offers a similar good, then heightened demand from the action-taking firm may spillover. But a neighbor offering a dissimilar good is unlikely to experience heightened demand spillovers because dissimilar goods are not what the customer segment attracted by the action-taking firm are interested in purchasing. More formally:

H3: Demand spillovers are greater (lesser) for neighbors that sell similar (dissimilar) goods to a firm taking action.

2.5 Spillovers for neighbors: less for neighbors competing more directly with participants

While agglomeration economies generate benefits for geographically proximate firms, there's the possibility that within an agglomeration some firms may not benefit due to a countervailing mechanism – “localized competition” (Baum and Ingram, 1996; Haveman and Baum, 1997; Kalnins, 2016). While agglomerated firms benefit on average, benefits can vary by firm. An important determinant is the extent that firms compete more directly with each other, with firms competing more directly with others benefiting less, not at all, or even being worse off. Reflecting greater localized competition, these authors show that when hotels are similar in size, price and geographic location, hotel survival decreases. More generally, when two firms offer goods that are similar to each other and when the two firms are geographically proximate, then the two firms compete

directly with each other. Recognizing the downside of competing directly with each other, firms make choices to limit localized competition. For example, Ren et al. (2011) show that when two prominent electronics retailers in the US (BestBuy and Circuit City) have stores geographically proximate to each other that their inventories are adjusted so that they have fewer products in common: the two chains' stores' Stock Keeping Units (SKU) overlap is smaller for BestBuy and Circuit City stores that are geographically closer together.

In an agglomeration, firms are likely cognizant of localized competition's effect. Each firm likely chooses its location in the overall geographic configuration cognizant of localized competition so that the negative effect is not greater than the agglomeration's positive spillover effect: a firm selling a similar product to another firm would take care to not locate too nearby, but to locate farther away. Therefore, when a firm takes demand-shaping action, neighbors selling a similar good get heightened demand spillovers, but neighbors selling a similar good that are also geographically closer experience less heightened demand spillovers due to the countervailing local competition mechanism. More formally:

H4: Demand spillovers are less for neighbors that are selling similar goods to and are geographically closer to the firm taking action.

3. Data / Context

To test our expectations, we use a demand-side agglomeration where some firms take "demand-shaping" action. As our demand-side agglomeration, we use restaurants in the Washington DC metropolitan area. As our observable demand-shaping action, we use restaurants' participation in Washington DC's Restaurant Week. Washington DC's Restaurant Week began in 2001, modeled after New York City's Restaurant Week, as a way to stimulate demand in the aftermath of the September 11 attacks by offering three-course, prix-fixe meals at value prices, but has evolved into a biannual event (in January and August) to boost demand during traditionally slower periods. To participate, restaurants have to (i) be members of the local association, (ii) sign up before the limited

number of slots (about 350) are filled, (iii) pay a participation fee (\$500), and (iv) create a prix-fixe menu at moderate price points (lunch: \$25 or \$35; dinner: \$40, \$55, or \$65).

Importantly, creating the prix-fixe menu differentiates Restaurant Week as a demand shaping action versus just being marketing/advertising. In defining demand shaping, Vinokurova (2019) argues that firms can alter the demand landscape to increase demand (increase a customer's willingness to pay) by altering the combination of product attributes of its goods. A firm can alter the combination by adding, removing, and/or transforming product attributes. Altering the combination of product attributes can “convince the customer that a different product attribute combination is more desirable”. One way to conceptualize restaurants' product is that they sell meals; and meals' attributes (cuisine, ingredients, preparation, price, etc.) are defined by items offered on restaurants' menus. With Restaurant Week's prix-fixe menu, restaurants typically offer a subset of existing items, bundled together at a lower fixed price. So, a Restaurant Week prix-fixe menu removes attributes (items from restaurants' full menus) and adds an attribute (a fixed-price), which is consistent with demand shaping, versus just marketing or advertising restaurants' existing menus at existing prices.

We use four iterations of Restaurant Week (“RW”): 2022 (summer), 2023 (summer & winter), and 2024 (winter) to identify the 335, 356, 310, and 371 restaurants that participated in those iterations, respectively.

For the population of restaurants in the Washington DC metropolitan area, and key traits of those restaurants, we obtain restaurant listings and related data from Yelp. Yelp data has been used extensively in finance, marketing, and strategy etc. (Huang, 2025; Seiler et al., 2017; Majzoubi and Zhao, 2023). Yelp is the largest and most popular restaurant listing platform in the United States with comprehensive and consistent coverage of restaurant traits such as cuisine, price, location, user rating, and reviews. Since Yelp lists restaurants currently in operation and also restaurants that have

closed, our population sample is uncensored; it is not survivor biased. Our starting sample includes 8,253 restaurants in the Washington DC metropolitan area.

Since restaurants choose to participate in Restaurant Week (“RW”) given some financial and non-financial costs (join the local association, a participation fee, and developing and implementing a prix-fixe menu), we expect that restaurants that chose to participate in RW benefit from doing so. We show that RW participants do benefit (at the start of the Results section) using the data on the weekly demand that each restaurant experiences.

For data on restaurant-specific demand, we use point of interest visitation patterns data from SafeGraph.² SafeGraph visitation patterns data “...includes anonymized visitor and demographic aggregations for points of interest (POIs) in the US and Canada over the course of a week. This contains aggregated raw counts of visits to POIs from a panel of mobile devices, showing how often people visit, how long they stay, where they came from, where else they go, and more. Data is aggregated and anonymized to provide insights into the volume of visitors to certain locations and overall behavioral patterns.” As each mobile device typically has a single user, the data is well suited as a measure of customer demand. SafeGraph visitation patterns data has been used extensively in economics, economic geography, finance and marketing, etc. (Aneja et al. 2025; Babar et al. 2023; Bizjak et al. 2022; Sharma et al. 2025). We also obtain restaurants’ physical size in square meters from the SafeGraph patterns data.

3.1 Sample construction

We obtain restaurants’ traits such as identifying information (name, address), categorical data (price and cuisine), and rating data (number of reviews and ratings) from Yelp. To get an exhaustive set of restaurants in the Washington DC metropolitan area, which includes Washington D.C. and

² We obtain the visitation patterns data from Advan, since effective January 2023 the patterns dataset previously provided by SafeGraph, is now offered by Advan. <https://www.deweydata.io/blog/advan-patterns-now-available>

parts of Maryland and Virginia, we first identify the list of neighborhoods that Yelp classifies as being part of the Washington DC metropolitan area. For these neighborhoods, we obtain the associated zip codes. Using these zip codes, we run automated search queries on Yelp for restaurants and capture the search results. We match the set of Yelp restaurants to the SafeGraph visitation patterns data by matching on name and address, using a fuzzy matching algorithm. To improve the accuracy of matching, since many street names occur frequently (for example: “Main Street”), we match by street names, street numbers, and zip codes. While exact matches are automatically recorded, we manually inspect matches with high but not exact match scores, and keep those that do appear to match.³ We then drop restaurants who have missing visitation patterns information for a RW iteration (in any of the several weeks before and after RW that we investigate). Due to differences in SafeGraph coverage over time, some restaurants might appear in some RW iterations but not in others. Finally, we end up with a starting sample of 8,253 restaurants. Of these, 385 restaurants participated in at least one RW iteration.

To assess whether RW participants’ heightened demand spills over to neighbors, we use a difference-in-difference study design. We compare differences in customer demand for neighbor restaurants from before and after RW, versus differences in customer demand for a set of control restaurants from before and after RW. We discuss specifics of the difference-in-difference study design, below in the “Study Design and Methodology” section.

We define “neighbors” as being within 1/4 mile of RW participants; we use 1/4 mile as the distance within which demand spillovers are likely to occur, which is based on how far people likely walk from their mode of transportation to the RW restaurant, and along the way observe other restaurants that they may also frequent, as we explain in detail in section 3.3 below.

³ Sometimes street names are abbreviated such as Av. for Avenue or St. for Street. Other times, it might be a minor typo preventing an exact match. The manual lookup addresses most of these challenges.

For the set of control restaurants, we use restaurants that are beyond 1/2 mile from any RW participant in any iteration of RW; we use 1/2 mile since we do not expect any demand spillovers for restaurants that far away, because people tend to exit their mode of transportation around 1/4 mile or less to the RW restaurant, as we explain in detail in section 3.3 below. Importantly, any restaurant that is ever a “neighbor” is never included as a control restaurant. There are 5,888 restaurants beyond 1/2 mile of any RW participant in any iteration of RW, which is our preliminary set of control restaurants.

Besides the neighbors that are within 1/4 mile of any RW participant and the control restaurants that are beyond 1/2 mile of any RW participant, there is a band from over 1/4 mile but within 1/2 mile, with 498 restaurants. We use this band as an empirical buffer zone, excluding the 498 restaurants in this band from our empirical analysis since (i) we want distinct differences between the sets of restaurants likely versus unlikely to experience demand spillovers (neighbors versus controls), (ii) the visitation patterns data shows that these 498 restaurants do experience greater customer visitation during RW, but do not experience greater customer visitation following RW, and (iii) we have plenty of other observations for the control set. Though as a robustness check, we do include these 498 restaurants (in the 1/4 – 1/2 mile band) with the 1,460 neighbor restaurants (within 1/4 mile of any RW participant), and obtain qualitatively similar results, as we discuss in the Robustness section.

To get a final set of similar neighbor and control restaurants, we winnow down the 1,460 neighbors and preliminary set of 5,888 control restaurants through two stages of matching using Coarsened Exact Matching (CEM, in R with default settings) (Blackwell et al. 2009). We do so recognizing that substantial heterogeneity exists across restaurants in the population of Washington DC metropolitan area restaurants. We want the range of heterogeneity in the neighbor and control restaurant sets to be more similar.

In the first stage, we match the 1,460 neighbors and 5,888 potential control restaurants based on the restaurant density in the zip codes they are located in. We calculate zip codes' restaurant densities knowing all restaurants' zip codes from their Yelp addresses, and each zip codes' size in square miles.

This match results in 840 neighbors matched to 5,881 potential control restaurants. Notably, with this zip code density matching, many neighbors are dropped while very few control restaurants are dropped. This is because RW participants and their neighbors tend to be in more dense zip codes, and for the densest neighbor restaurant zip codes there are no comparable dense zip codes among the control restaurants.

For these resulting matched restaurants (840 neighbors and 5,881 potential control restaurants) we match a second time based on neighbors' and potential control restaurants' cuisine, price, average rating, number of reviews received, and physical size. Each review of a restaurant on Yelp is accompanied by a rating value on a five-point scale. Average rating is the simple average of all the ratings obtained till a given date. Since both the number of reviews and average rating are time variant values, we calculate the value of these two variables on the first day of each RW iteration. Some restaurants do not have reviews, which are likely newer; we record number of reviews as 0 and average rating as NA; we do match restaurants with NA to other restaurants with NA. For the cuisine, some restaurants list multiple cuisines, in which case, we consider the first cuisine listed to be the restaurant's cuisine. Finally, each restaurant on Yelp indicates the average price of their offering on a four-point scale. Those restaurants without a price are recorded as NA for the price; as with average rating, we do match using NA for price. Both cuisine and price are time invariant traits. After this second stage of matching, we end up with 672 neighbors and 4,809 matched control restaurants, a total of 5,481 restaurants, which we use in our empirical analysis.

And in additional tests discussed in the Robustness section, we use both a less matched set (only the first stage of matching by zip code restaurant density) and an unmatched set (neither stage of matching) of control restaurants; and find results similar in coefficient estimate signs and statistical significance to those presented in the “Results” section.

3.2 Dependent Variable

For data on restaurant-specific demand, we use visitation patterns data by SafeGraph. While SafeGraph provides daily visitation measures for businesses, visits to restaurants are cyclical: usually higher during the weekend compared to the weekdays. To sidestep this cyclical, we use a week’s average daily visitation as our measure of firm performance: we sum up a restaurant’s visitation over the seven days of a week and divide this sum by seven. In some cases, the visitation data is missing for a day(s). In such cases, we average the weekly sum over the number of days for which the data is available and then divide by the number of days that have data.

We examine visitation for several weeks before and after Restaurant Week given RW’s purpose of boosting demand during traditionally slower periods. RW is a biannual event occurring in late January, when demand slows down after the Christmas/New Year’s holidays, and in August, when many people take vacation out of town before the start of the school year. We use a span of six weeks before RW and six weeks after (or more accurately the week of RW and the five weeks after). We choose six weeks before to make sure that we reach beyond the time span when demand slows down. For example, for January RW, six weeks before is mid-December when demand is high for office parties and holiday gatherings. Since we choose six weeks before RW, we also choose to examine six weeks after. Therefore, for restaurants in our sample, we have visitation data for the six weeks before, the week of, and five weeks after Restaurant Week.

3.3 Independent Variables

The key independent variable is whether a restaurant is a “neighbor” to a Restaurant Week participant. Conceptually, we use a 1/4 mile cutoff: a restaurant is a neighbor if it is within a 1/4 mile of any RW participant; as the spillover mechanism is driven by how far people likely walk from their mode of transportation to the RW restaurant, and along the way observe other restaurants which they may also frequent. We expect people to park their cars, use a subway station, or exit their rideshare usually within a 1/4 mile of their destination RW restaurant.

Our choice of 1/4 mile is based upon research on typical walking distances using data from the National Household Travel Study. Watson et al. (2021) find that the average walking trip is .6 miles or approximately 12 minutes. Splitting walking trips into those that are home-transit and transit-destination, Yu and Lin (2016) find that transit-destination trips average less than 8 minutes (or less than .5 miles). Yang and Diez-Roux (2012) find that trips for meals (versus for other purposes including work, study, shopping, social events, recreation, and dog-walking) are the shortest, with an average of 0.25 miles.

While we use a restaurant being within a 1/4 mile of a RW participant as our definition of “neighbor”, as a robustness test, we increase this threshold to be restaurants within a 1/2 mile of a RW participant and obtain qualitatively similar results to those presented later.

To determine distances between restaurants, we start with their addresses from Yelp. Then using the Bing maps API, we (i) convert the restaurants’ addresses into latitude and longitude, and then (ii) obtain the travel distance, using routing along streets versus using “as a bird flies”, between two restaurants’ latitude and longitude coordinates.

As we use four iterations of RW, and RW participants vary across iterations, the restaurants that are neighbors also vary: a particular restaurant may be classified as a neighbor for some

iterations but not a neighbor for other iterations. Across the four iterations, there are 672 restaurants that are “neighbors” at least one iteration.⁴

We further classify neighbor restaurants along two dimensions: (i) geographic distance and (ii) whether it sells a similar good to the proximate RW participant. For geographic distance, there are “closer neighbors” – that are within 1/8 mile of a RW participant and “farther neighbors” – that are between 1/8 and 1/4 mile of a RW participant. Besides this binary measure, we also use a continuous distance measure between neighbors and their closest RW participant, and obtain results consistent with the binary measure results, which we discuss in the Robustness section. There are 399 closer neighbors and 273 farther neighbors across the four iterations of RW.

For whether the neighbor sells a similar good to the RW participant that it neighbors, we use a binary “similar” or “dissimilar” measure. To determine “similar” or “dissimilar”, we use Coarsened Exact Matching (CEM, in R with default settings) and match neighbors to RW participants on five traits (price, cuisine, rating, number of reviews, and physical size). To be “similar” a neighbor and RW participant that it neighbors have to be in the same CEM bin for all five traits. This results in 93 similar neighbors and 618 dissimilar neighbors.⁵

Finally, when testing hypothesis 4 – demand spillovers are less for neighbors that are selling similar goods to and are geographically closer to the firm taking action – we use the subset of “similar” neighbors that are also geographically “closer” (within 1/8 mile) to a RW participant, which is 70 neighbors.

In our study design we have treated and control restaurants. “Neighbors” are “treated” – they are exposed to potential spillovers from RW participants’ demand-shaping action. As “control”

⁴ Before the two stages of CEM as described above in section 3.1, there were 1,460 neighbors.

⁵ As $93 + 618 = 711$; this does not seem consistent with the 672 total neighbor restaurants. But the same focal neighbor restaurant can be similar in one iteration and dissimilar in another (or vice versa) depending upon what nearby restaurants participate in RW, since what restaurants participate in RW changes by iteration.

restaurants, we want restaurants that are not exposed to potential spillovers from RW participants' demand-shaping action. For restaurants that we do not expect spillovers, we use restaurants that are 1/2 mile or more away from any RW participant (in any of the four RW iterations), as Yang and Diez-Roux (2012) find that trips for meals (versus for other purposes including work, study, shopping, social events, recreation, and dog-walking) are the shortest, with an average of 0.25 miles. This means that people are not usually parking their cars, using a subway station, or exiting their rideshare a 1/2 mile from their destination RW restaurant, and therefore restaurants a 1/2 mile from any RW restaurant are unlikely to have additional customers due to RW. While expecting restaurants a 1/2 mile from any RW restaurant are unlikely to have additional customers due to RW, we verify that this is empirically true in our data – that restaurants a 1/2 mile or more from any RW restaurant do not experience increased customer visitation during or after RW.

In our empirical difference-in-difference specifications, the other key independent variable is “post” – whether the time period is during and after the treatment. Our measure of time is weeks. And for each iteration of RW, we examine customer visitation for a 12-week period: 6 weeks before, the week of, and 5 weeks after RW. Post takes a value of 0 for the six weeks leading up to RW. Post takes a value of 1 starting the week of RW and continuing for the five subsequent weeks.

Descriptive statistics for the full sample, neighbor restaurants, and control restaurants are shown in Table 1.

[Table 1 about here]

To further describe our data, in the Appendix, we also present binned scatterplots for (i) average daily visits for “Neighbors” versus “Controls” for several weeks before and after RW and (ii) for the key subgroup (closer vs. farther neighbor, similar vs. dissimilar neighbor). Starr and Goldfarb (2020) advocate for binned scatter plots of the nonparametric relationships – the raw data without any control variables – between the dependent and key independent variables; this

“...allows researchers to quickly detect the shape of that relationship, examine outliers, and assess which part of the support may be driving a relationship.” Detecting the shape of the relationship in the raw data informs the researcher as to what parametric relationships might be more appropriate to hypothesize. Versus scatter plots, where it is difficult to observe patterns with large samples, “A binned scatterplot condenses the information from a scatterplot by partitioning the x-axis into bins, and calculates the mean of the dependent variable within each bin.”

Finally, also in the Appendix, given the geographic nature of our context, we present maps showing the location of individual restaurants belonging to the three relevant groups: (i) RW participants, (ii) neighbors of RW participants, and (iii) control restaurants.

4. Study Design and Methodology

As an antecedent, we show that RW does heighten demand for the participating restaurants. Then, to test our first hypothesis, we examine whether the heightened demand experienced by RW participants spills over to their neighbors. Next, we investigate the spillovers for the neighbors along two dimensions: whether neighbors are geographically closer or farther to RW participants (H2) and whether neighbors sell similar goods or not to RW participants (H3). Finally, we also examine how a closer neighbor selling a similar good to the RW participant within 1/8 of a mile away affects that neighbor’s spillovers (H4).

Methodologically, we employ a stacked two-way fixed effects (TWFE) DiD model (Cengiz et al., 2019; Gromley and Matsa, 2011), which is a derivative of the standard TWFE DiD model. We use a stacked DiD because our setting has multiple treatment periods (four iterations of RW). Additionally, across the four iterations, some restaurants switch between being a RW participant and not being a participant, which affects which restaurants are treated (are neighbors) or not. A stacked DiD allows us to combine separate DiDs for each iteration of RW into a single DiD.

We analyze twelve weeks of data for each RW iteration; six weeks before the start of RW, the week of RW, and five weeks after RW. We set up the data for the stacked DiD by aligning the twelve weeks of data across the four iterations so that the weeks are indexed 1 to 12 for the 6 weeks before, the week of, and the five weeks after RW. We then stack up the data of the different iterations on top of each other to create a single dataset.

One potential concern in the case of multiple treatment periods is when treated units are treated more than once. While this is true in our data – many restaurants participate in multiple RW iteration, which means that their neighbors may repeatedly experience demand spillovers; fortunately, the effect of participation in RW and related spillovers for neighbors is very short versus the time between subsequent treatments. The increased demand from RW participation as well as neighbor spillovers persist for only several weeks, while the time between RW iterations is typically 20 weeks or more. This short-lived spillover treatment effect ensures the restaurants who have already been neighbors to participating restaurants once, can be again included in the sample in future iterations as if they have not been treated before.

While DiD specifications control for time-invariant group differences (of treated versus control restaurants) and time fixed effects (that affect treated and control restaurants equally), we include two sets of interaction fixed-effects: (i) Establishment-RW_iteration and (ii) Zipcode-Week.

We include these interaction fixed-effects because when checking the balance – the comparability of traits between the neighbor and control restaurant groups – several traits show imbalance. We examine restaurant traits that can affect demand/visitation: “price”, “rating”, “number of reviews”, “physical size”, and “zip code (restaurant) density”. The balance table appears as Appendix Table 1, and shows that these five traits have normalized difference values in excess of 0.25, which as a general rule of thumb indicates potential for problematic imbalance between the neighbor/treated and control groups (Baker et al. 2025; Imbens and Rubin, 2015).

Thus, the central assumptions for DiD specifications of “parallel trends” – that the neighbor restaurants (the treated group) and the control restaurants trend in the same way were the neighbor restaurants untreated – is unlikely to be true unless we control for covariates. In other words, in our empirical setting, the parallel trends assumption is inappropriate for an unconditional DiD specification; rather we should use a conditional DiD specification – where appropriate control variables are included – with the assumption that parallel trends hold conditional on accounting for covariates.

After including control variables for the conditional DiD specification, we evaluate the plausibility of the parallel trends assumption by examining pre-treatment trends across the neighbor/treated and control groups, with a standard event-study model. The event-study results are consistent with the parallel trends assumption being plausible, which we discuss in the Results section.

For the conditional DiD specification, given the imbalance for “price”, “rating”, “number of reviews”; we want to control for individual establishment/restaurant effects, as these three traits are restaurant specific. But instead of just establishment-specific fixed effects, we include an “Establishment-RW_iteration” fixed-effect. This interaction fixed-effect is establishment, iteration of RW specific; essentially every restaurant has a different fixed-effect for each of the four iterations of RW, which accounts for time-varying changes at the restaurant level for each iteration of RW such as restaurants changing décor and improving facilities. Also, some restaurants’ relative distance from a RW participant might change from one iteration to another as the specific restaurants that are RW participants change with each iteration. Similarly, given the imbalance for “zip code (restaurant) density”, we want to control for zip code effects. But instead of just zip code-specific fixed effects, we include a “Zipcode-Week” fixed-effect that is zip code, week specific; allowing each zip code a different fixed-effect for every week, which accounts for any zip code having events (a

convention, sports event, road closures, etc.) that affect demand in that zip code in any particular week.

5. Results

Before examining whether heightened demand spills over to action-taking firms' neighbors, we need to establish that the action-taking firms do themselves experience heightened demand from their demand-shaping action. To test this, we use a DiD approach to compare RW participants to control restaurants. We discuss this DiD approach and sample construction in detail in the Appendix. These DiD model specifications show that those restaurants participating in RW do experience heightened demand versus control restaurants, which is unsurprising because they are choosing to participate. The average RW participant experiences an 11.0% increase in demand during RW that persists at a similar level for a couple weeks, but then drops off substantially after several weeks. For robustness of results, we use three variants of RW participants and control restaurants with different levels of matching (no matching, matching by zip code restaurant density, matching by zip code restaurant density and five restaurant traits) as described in the Appendix, with the corresponding results in Appendix Tables 2A, 2B, and 2C. A detailed discussion of these results is in the Appendix.

Turning to tests of our expectations, we test Hypothesis 1 “Heightened demand from a firm’s action can spill over to that firm’s neighbors” in Table 2. Recall that we define “neighbors” as restaurants that did not participate in RW, but are within 1/4 mile of a restaurant that did participate in RW. For the neighbor definition, in additional tests discussed in the Robustness section, we increase this threshold to being within 1/2 mile of a RW participant; and find results similar in coefficient estimate signs and statistical significance to those presented below.

We use a difference-in-differences (DiD) specification comparing neighbors before and after Restaurant Week (“RW”) versus a control set of restaurants. The restaurants in the control set are

not “neighbors” to RW participants; they are 1/2 mile or farther from any RW participant; and are matched to neighbor restaurants by zip code restaurant density and five restaurant traits (price, cuisine, rating, number of reviews, and physical size). And for the control set of restaurants, in additional tests discussed in the Robustness section, we use both a less matched set (only matching by zip code restaurant density) and an unmatched set (no matching) of control restaurants; and find results similar in coefficient estimate signs and statistical significance to those presented below.

In addition to matching, we also include two sets of interaction fixed-effects: (i) Establishment-RW_Iteration and (ii) Zipcode-Week, to account for restaurant specific differences across restaurants as well as restaurant specific differences for each of the four RW iterations (each restaurant has a different fixed-effect each RW iteration), and zip code specific differences across zip codes as well as zip code specific differences for each of the 12 weeks (each zip code has a different fixed-effect each of the 12 weeks around RW) that might affect restaurants’ demand.

An antecedent to discussing the results from the DiD specifications is establishing that the identifying assumption of “parallel trends” are plausible for the neighbor versus control restaurants – that the neighbor restaurants (the treated group) and the control restaurants trend in the same way were the neighbor restaurants untreated. We can evaluate the plausibility of this assumption by examining pre-treatment trends. To do so, we use a standard event-study model using the week before the treatment, the week before RW, as the reference week. Results are shown graphically as Figure 1. For several weeks before RW, we observe no evidence of different pre-trends for the two groups, suggesting that our matching approach to constructing a control group performs well.

[Figure 1 about here]

Turning to the DiD results, Table 2 has 6 columns. Each column corresponds to a different pre-treatment and post-treatment period duration: the span of weeks that are being compared before and after (and during) RW. The first column is the “1 week” pre/post-period comparison, which is

the week before RW versus the week of RW. The second column is the “2 weeks” pre/post-period comparison, which is the two weeks before RW versus RW plus the week after. The third column is the “3 weeks” pre/post-period comparison; the three weeks before RW versus RW plus the two weeks after. And so on, up to the sixth column, which is the “6 weeks” pre/post-period comparison. Correspondingly the number of observations from the first column is double in the second column, triple in the third column, etc. Number of observations in the first column – 36,664 – comes from the number of treated and control restaurants in each of the four RW iterations, with one pre-treatment week and one post-treatment week observation. While there are 5,481 unique restaurants across the four RW iterations, not every unique restaurant appears in all four iterations. On average, a restaurant participates in 3.34 RW iterations.

[Table 2 about here]

The focal variable is “Neighbor * Post”, which captures how neighbors’ demand (visitation) changed from before RW versus during and after RW, versus similar control restaurants’ change in demand from before RW versus during and after RW. Across all six columns (1 week up to 6 weeks pre/post-period comparisons) the coefficient estimates are positive and significantly different from zero at a 1% level. The coefficient estimates’ magnitudes range from a high of 0.982 (2 weeks pre/post comparison) to a low of 0.523 (6 weeks pre/post comparison). According to the SafeGraph data, on average, neighbor restaurants experience 12.83 daily visits in the six weeks before RW, so this range of coefficient estimates indicates a 7.7% to 4.1% increase in demand ($0.982/12.83 = 0.077$) for neighbors of RW participants, relative to change in demand for control restaurants. This compares to an 11.0% increase in demand for RW participants during RW, which indicates that neighbors experience roughly two-thirds the magnitude of the heightened demand that RW participants experience. These results indicate that RW does heighten demand for neighbors, on average, and that the heightened demand persists beyond just RW for several weeks following.

These results support Hypothesis 1 that action taking firms' heightened demand spills over to their neighbors.

To test Hypothesis 2, "Demand spillovers are greater for neighbors geographically closer to a firm taking action", we break neighbors into two subsets based on distance from RW participants. As discussed in the data section, the "closer" neighbors are within 1/8 mile of any RW participant, and the "farther" neighbors are within a band of 1/8 to 1/4 mile from any RW participant. We add "Neighbor_Closer * Post" to the DiD specification. Doing so, "Neighbor_Closer * Post" estimates the effect for the "closer" neighbors, while "Neighbor * Post" now estimates the effect for the "farther" neighbors, since "Neighbor" now only picks up the remaining neighbors with the closer neighbors separated out with "Neighbor_Closer". The DiD results are shown as Table 3. Besides this binary distance split, we also use a continuous distance measure between neighbors and their RW participant, and obtain results consistent with the binary measure results, which we discuss in the Robustness section.

[Table 3 about here]

Looking at Table 3, there is a clear difference between the spillover effects for closer and farther neighbors. Heightened demand from RW participants spills over to closer neighbors, but not to farther neighbors. For "Neighbor_Closer * Post", across all six columns (1 week up to 6 weeks pre/post-period comparisons) the coefficient estimates are positive and significantly different from zero. The coefficient estimates' magnitudes range from a low of 0.925 (1 week pre/post comparison) to a high of 1.903 (4 weeks pre/post comparison), which corresponds to a 7.2% to 14.8% increase in demand for closer neighbors of RW participants, relative to change in demand for control restaurants. In contrast for farther neighbors, "Neighbor * Post", across the first four columns is not significantly different from zero, and is significantly negative in the fifth and sixth columns: farther neighbors see less demand than control restaurants for longer pre/post treatment

durations. These results support Hypothesis 2 that demand spillovers are greater for geographically closer neighbors.

To test Hypothesis 3, “Demand spillovers are greater for neighbors that sell similar goods to a firm taking action”, we break neighbors into two subsets based on whether they sell similar goods to a RW participant or not, as discussed in the data section. We add “Neighbor_Similar * Post” to the DiD specification. Doing so, “Neighbor_Similar * Post” estimates the effect for the “similar” neighbors, while “Neighbor * Post” now estimates the effect for the “dissimilar” neighbors, since “Neighbor” now only picks up the remaining neighbors with the similar neighbors separated out with “Neighbor_Similar”. The DiD results are shown as Table 4.

[Table 4 about here]

Looking at Table 4, there are interesting differences between the spillover effects for similar and dissimilar neighbors. For similar neighbors the pattern of coefficient estimates is mixed. For shorter pre/post-periods (1, 2, and 3 weeks), “Neighbor_Similar * Post” coefficient estimates are not significantly different from zero; but for longer pre/post-periods (4, 5 and 6 weeks), coefficient estimates are significantly different from zero and positive. And for these longer pre/post periods, the coefficient estimates range from a high of 3.258 (4 weeks pre/post comparison) to a low of 2.582 (6 weeks pre/post comparison), indicating a 25.4% to 20.1% increase in demand, relative to change in demand for control restaurants. These increases in demand are greater than for neighbor restaurants in general (from Table 2, a range of 4.1% to 7.7% increase) and also for “closer” neighbors (from Table 3, a range of 7.2% to 14.8% increase).

While expecting larger coefficient estimates, the lack of significant coefficient estimates for shorter pre/post-periods (1, 2, and 3 weeks) is not expected. One possibility is that this is the effect of localized competition. While we hypothesize about localized competition in H4 that there will be a reduced positive effect for neighbors that are both similar and closer to RW participants, the

results here suggest that there is a localized competition effect among neighbors that are just similar to RW participants. And the duration of the localized competition effect is only a couple weeks. If the explanation for lack of significant coefficient estimates for shorter pre/post-periods is localized competition for similar neighbors, then dissimilar neighbors should not be similarly affected.

For dissimilar neighbors, looking at the coefficient estimates for “Neighbor * Post”, across all six columns (1 week up to 6 weeks pre/post-period comparisons) the coefficient estimates are positive and significantly different from zero. The pattern of estimates for dissimilar neighbors is different from estimates for similar neighbor – positive across both shorter and longer pre/post durations versus non-significant for shorter but significant for longer duration. This difference supports the explanation that localized competition is affecting similar neighbors for a couple weeks: similar neighbors have coefficient estimates that are not significantly different from zero for shorter pre/post-periods (1, 2, and 3 weeks).

A possible explanation for the localized competition’s short duration is that customers make multiple purchases within our observation time frame: they visit restaurants during RW, but also visit restaurants again several weeks after RW. During RW, a neighbor selling a similar good to a RW participant that is within a 1/4 mile is in direct competition with the RW participant; the customer dines at only one restaurant and the neighbor similar to the RW participant does not benefit. But several weeks later, the customer dines again, and the similar neighbor does benefit.

The significant difference for longer pre/post-periods supports H3 that “Demand spillovers are greater (lesser) for neighbors that sell goods similar (dissimilar) to a firm taking action”. But the lack of a significant difference for the shorter pre/post-periods suggests that demand shaping is offset by localized competition, which has a duration of only a couple weeks.

To test Hypothesis 4, “Demand spillovers are less for neighbors that are selling similar goods to and are geographically closer to the firm taking action”, we use the subset of similar

neighbors that are also closer geographically to a RW participant, as described in the data section.

We add “Neighbor_Similar_Closer * Post” to the DiD specification. Doing so,

“Neighbor_Similar_Closer * Post” estimates the effect for the “similar-closer” neighbors, while

“Neighbor * Post” estimates the effect for all other neighbors: “similar-farther” neighbors and

“dissimilar” neighbors. The DiD results are shown as Table 5.

[Table 5 about here]

Looking across the six columns of Table 5, the pattern of coefficient estimates is mixed. For shorter pre/post-periods (1 and 2 weeks), “Neighbor_Similar_Closer * Post” coefficient estimates are not significantly different from zero; but for longer pre/post-periods (3, 4, 5 and 6 weeks), coefficient estimates are significantly different from zero and positive. And for these longer pre/post periods, the coefficient estimates range from a high of 4.700 (4 weeks pre/post comparison) to a low of 3.616 (6 weeks pre/post comparison), indicating a 36.7% to 28.2% increase in demand, relative to change in demand for control restaurants.

This pattern of coefficient estimates is similar to those in Table 4, for similar goods neighbors. As with the results for similar goods neighbors, these results suggest an overlap of localized competition with demand-shaping spillovers. The lack of significant difference for shorter pre/post-periods is consistent with localized competition initially offsetting demand-shaping spillovers, but abating later as demand-shaping spillovers persist. This lack of significant differences for shorter pre/post-periods supports H4 that demand spillovers are less for neighbors that are selling similar goods to and are geographically closer to the firm taking action. But the lack of significance not persisting for longer pre/post-periods highlights an un-hypothesized dimension: that mechanisms can have different durations.

Overall, we find support for H1, heightened demand from action taking firms spills over to their neighbors; and H2, demand spillovers are greater for geographically closer neighbors. These

results are consistent with demand-side agglomeration spillovers. We also find support for H3, demand spillovers are greater for similar neighbors and H4, demand spillovers are less for neighbors that are selling similar goods to and are geographically closer to the firm taking action. But for H3 and H4, the results are not persistent across both shorter and longer pre/post-period comparisons. This lack of persistence is consistent with demand-shaping spillovers and localized competition's effects overlapping; and localized competition's effect having limited duration. They also introduce a temporal dimension that was unexpected since duration of mechanisms is not something prior literature raises, but can be important since an overlap of mechanisms matters as long as the mechanisms' effects persist. Overall, our results reveal dynamics among action-taking firms and their neighbors that result from overlapping mechanisms, some of which appear to have different time durations.

6. Robustness Tests

Before settling on these results, we conduct a couple robustness tests. The first is for how we define restaurants as a “neighbor” to RW participants. In our results presented above, a restaurant is a neighbor if it is within 1/4 mile of a RW participant. As a robustness test, we increase this threshold to being within 1/2 mile of a RW participant.

With this definition, there are 1,042 “neighbors” versus 672 when we use the 1/4 mile definition.⁶ We replicate Table 2, as Appendix Table 3, which tests H1: “Heightened demand from a firm’s action can spill over to that firm’s neighbors”. Across five columns for the “1 week” up to “5 weeks” pre/post-period comparisons, the results in Appendix Table 3 are similar to those in Table 2 in coefficient estimate sign and level of significance. And in the sixth column for the “6 weeks” pre/post-period comparison, the coefficient estimate is not significantly different from zero, which is different from Table 2. These results suggest that H1 also holds for this broader definition of

⁶ Before the two stages of matching there are 1,977 “neighbors” versus 1,460 when we use the 1/4 mile definition.

neighbor, but for a shorter duration, which is expected as spillover benefits should drop off with greater distance.

Second is how we measure closer versus farther neighbor for testing H2 “Demand spillovers are greater (lesser) for neighbors geographically closer (farther) to a firm taking action.” In our results presented above, “closer” are neighbors within 1/8 mile of a RW participant, while “farther” are neighbors farther than 1/8 mile but within 1/4 mile. As a robustness test, instead of this binary measure, we use a continuous measure of how far a neighbor is from the RW participant. For each neighbor restaurant, we use the travel distance (using routing along streets) to the RW participant.

And if a neighbor restaurant is within 1/4 mile of multiple RW participants, we use the average travel distance to these multiple RW participants. We first generate travel distances for neighbors defined by within 1/4 mile of a RW participant and generate a set of results. But we also generate a set of results using neighbors as being defined as within 1/2 mile of a RW participant. We include neighbor distance by adding “Neighbor_Distance * Post” to the DiD specification. The results are shown as Appendix Table 4A and 4B. In both tables “Neighbor_Distance * Post” coefficient estimates are negative and significant. Pairing these negative estimates with the positive “Neighbor * Post” coefficient estimates, the results indicate that from a positive starting value, as distance for neighbors increases, the less demand spillover a neighbor restaurant experiences.

Third is how we use two stages of matching to winnow down the 1,460 neighbor restaurants and 5,888 potential control restaurants to 672 neighbors matched to 4,809 control restaurants. To show that our results are robust to less specific matching, we show results with (i) matching only by the first stage (zip code restaurant density) but not by the five restaurant traits (which leaves 840 neighbors matched to 5,881 control restaurants) and also (ii) no matching at all (using all 1,460 neighbors and all 5,888 potential control restaurants). We show results analogous to Table 2 (testing H1) using these two neighbor-control set variants. The results are shown in Appendix Table 5A with

only the first stage matching (zip code restaurant density) and Appendix Table 5B for no matching. The results in both tables are similar to those in Table 2: across the six columns (1 week through 6 weeks pre/post-period comparisons) the coefficient estimates are positive and significantly different from zero.

7. Discussion / Conclusions

Agglomeration economies generate benefits that accrue to firms that geographically agglomerate, which can improve those firms' performance. Demand-side agglomeration economies can increase agglomerated firms' revenues as more customers are drawn to agglomerated firms due to lower search costs for differentiated goods, since agglomerated firms offer customers greater variety (Ren et al., 2011) with lower transportation costs to assess and compare differentiated goods (Dudey, 1990; Stahl, 1982; Wolinsky, 1983).

Because of the potential to improve firms' performance, agglomeration economies have attracted strategy researchers' attention. But understanding the dynamics of agglomeration benefits propagating throughout an agglomeration is difficult since an agglomeration is composed of multiple if not many firms, with each firm engaging in various activities that might have spillover implications for other firms in the agglomeration, and spillover effects might take time to emerge. As a result, extant research typically treats agglomeration as a black box; to be added to or subtracted from – such as when firms enter an agglomeration or when firms exit – that affects outcomes, often in the longer-term – such as business formation, and business performance.

Besides firms entering and exiting, resident firms regularly engage in other strategic actions that have performance implications for other firms in the agglomeration, but research on such is almost non-existent. To address this gap, we leverage firms' use of a discrete, benefit creating, strategic action to assess how the benefit propagates through the firms' agglomeration: which neighbors benefit, how much, and why.

We expect some of the discrete strategic action's benefit to spill over to participants' neighbors. We expect (i) neighbors that are more geographically proximate to action taking firms will benefit more (due to demand-side agglomeration spillover), (ii) with neighbors selling similar goods to the action taking firms benefiting more (due to demand shaping spillover), but (iii) neighbors more geographically proximate to an action taking firm selling a similar good to benefit not as much (due to localized competition).

To test our expectations, for our demand-side agglomeration, we use restaurants in the Washington DC metropolitan area; and for our discrete strategic action, we use "Restaurant Week" in Washington DC – some restaurants offering prix-fixed, three-course meals for a short time. We examine whether neighboring restaurants to action-taking firms (Restaurant Week participants) experience greater customer demand, and what types of neighbors experience greater demand.

After first establishing that restaurants participating in Restaurant Week ("RW") experience heightened demand from RW, we find that participants' neighbors (restaurants within 1/4 mile of a RW participant) experience heightened demand that persist for several weeks after RW ends. Neighbor restaurants benefit more when they are geographically closer (within 1/8 mile versus 1/8 to 1/4 mile away) or selling similar goods (matched on five traits) to the proximate RW participant, which is consistent with demand-side agglomeration and demand-shaping spillovers, respectively.

But for neighbors selling similar goods to the proximate RW participant, the benefit is delayed a couple weeks: there is no benefit (relative to control restaurants) for the first couple weeks, which is consistent with demand-shaping spillovers' positive effect initially being offset by localized competition's negative effect. And this benefit delay also occurs for neighbors selling similar goods to the proximate RW participant that are geographically closer (within 1/8 mile versus 1/4 mile) to the proximate RW participant.

Our study makes several contributions. First, we look within an agglomeration and examine how a specific strategic action by some firms within the agglomeration creates demand spillovers that propagate differentially to other firms. Firms engage in strategic actions all the time, but the agglomeration literature has tended to look at little besides whether or not firms agglomerate. Also, our particular strategic action generates benefits for other firms that are temporary – lasting several weeks. As far as we know, examining duration of agglomeration benefits is novel. And the temporary benefit from this particular strategic action raises questions about the portfolio of actions/activities that firms engage in that result in the long-term agglomeration benefits shown by extant research (for firm formation and performance). What are the other actions/activities that firms engage in that generate benefits for others, which others, and for how long?

We also highlight specificity in agglomeration benefits – benefits are greater for certain agglomerated firms, which stems from specificity in benefit creation – firms take strategic action to help themselves, which leads to specificity of spillover benefits – neighbors benefit more when geographically closer and when more similar to the action taking firms. This specificity provides insight into how spillovers propagate throughout an agglomeration economy – starting with certain firms taking strategic action that benefits themselves, that then also benefits other neighbors and certain neighbors more.

While we examine demand-side agglomeration, specificity of agglomeration benefits can also apply to supply-side agglomeration. Specificity of benefits amplifies the fundamental strategic agglomeration arguments of Shaver and Flyer (2000) – that firms contribute unequally to agglomeration economies: more capable firms contribute more but gain less, while less capable firms contribute less while gaining more, which leads more capable firms to not agglomerate – to help their competitors less – while less capable firms do agglomerate. But given specificity of agglomeration benefits, more capable firms, instead of responding by not agglomerating, especially

when changing locations is difficult, can tailor their actions and activities to generate benefits more specific for themselves, which would narrow the set of neighbors that the benefits would spillover to.

Second, along with agglomeration benefits, the literature also introduces competition costs that arise when firms agglomerate, but with the benefits typically outweighing the cost, which leads firms to agglomerate. But evidence of competition costs has been limited since firms in agglomerations may act to forestall benefits being outweighed by costs, by not entering an agglomeration or relocating away. To observe agglomeration competition costs, we leverage a discrete strategic action that temporarily changes the prevailing pattern of agglomeration benefits, so that some establishments benefit more while others benefit less due to competition costs. And while the studies demonstrating agglomeration competition costs (Baum and Ingram, 1996; Haveman and Baum, 1997; Kalnins, 2016) examine a long-term outcome, establishment survival; we add a short-term outcome, weekly changes in customer demand. Thus, we further empirically support the presence of agglomeration competition costs, which suggests that firms need to consider both agglomeration benefits and competition costs within agglomerations.

Finally, our focus on demand-side agglomeration dovetails with a growing recognition of the importance of the demand-side as a complement to the supply-side for firm strategy (Adner and Zemsky, 2006; Priem, 2007; Schmidt et al., 2024). Within the demand-side literature, Schmidt et al. (2007) identify five value creation logics: (for firms) matching, leveraging, adapting, learning, and shaping customer preferences or market characteristics; with the fifth, demand shaping, having the least extant research. Schmidt et al. note that “...the few studies that examine shaping in existing markets found that firms can act strategically to influence customer characteristics to their advantage. In addition, firms must be mindful of the fact that their actions collectively shape market outcomes.” Our study fits the first point – that firms can act to influence customers to their

advantage, which is what Restaurant Week does by increasing customer demand in a time of year with historically lower demand. Our study also extends the second point – that firms' actions collectively shape market outcomes – by hypothesizing and showing how outcomes can spillover to affect other firms.

Our study has some limitations. First it is a demand-side agglomeration setting, so our theorizing of mechanisms and their effects do not generalize to supply-side agglomeration. Second, within demand-side agglomeration, our setting features goods with relatively frequent purchasing; so, our findings may not generalize to demand-side settings with less frequent purchasing, such as durable goods. Third, we examine only a single agglomeration, restaurants in Washington DC for four occurrences of Restaurant Week; so, tests of Restaurant Weeks effect in other locations would help generalize our findings.

These limitations suggest areas for other research. Examining demand-side agglomeration differences for durable versus non-durable goods. Examining other discrete strategic actions that firms take and whether they have spillover implications for which firms and for how long. Examining specificity of strategic actions in supply-side agglomeration settings.

Overall, our results provide insight into the dynamics of how spillovers propagate throughout an agglomeration. Certain firms take strategic action that can spill over to neighbors. The extent of spillovers are determined by three mechanisms – demand-side agglomeration spillover, demand-shaping spillover, and localized competition. These mechanisms affect which neighbors benefit and how much.

8. References

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Figure 1: Plot of Event Study Estimates for Neighbor versus Control Restaurants

Difference in average daily visits for Neighbor vs. Control restaurants. 95% confidence intervals in red. We use the week before the treatment, the week before Restaurant Week, “-1”, as the reference week. Restaurant Week itself is week “1”. The week after Restaurant Week is week “2”.

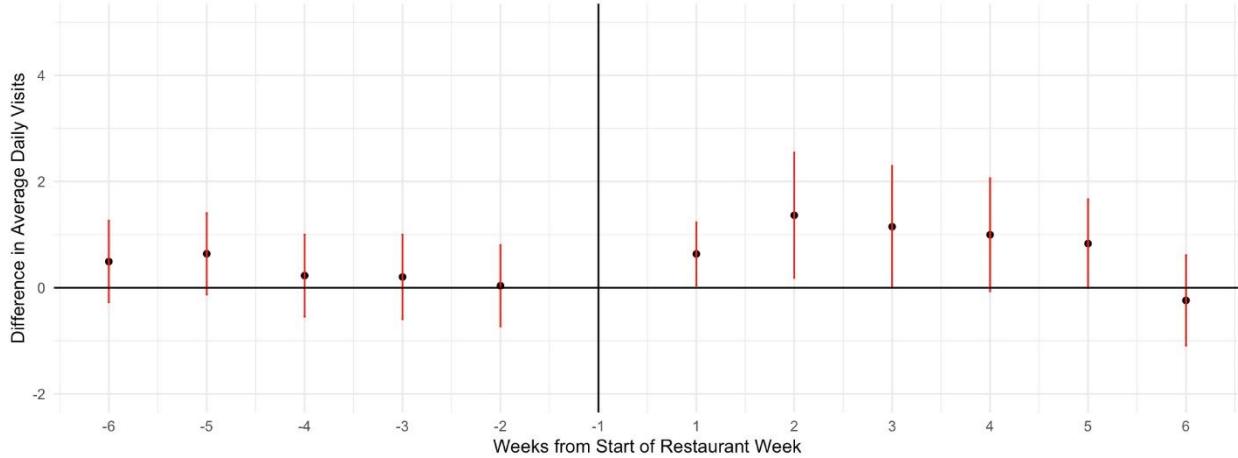


Table 1: Descriptive Statistics

Restaurant Trait	Full Sample			Neighbor Restaurants Only			Control Restaurants Only		
	N	Mean	Std Dev	N	Mean	Std Dev	N	Mean	Std Dev
Visits (avg daily)	219,828	8.87	21.08	21,720	12.91	35.68	198,108	8.42	18.75
Price	219,828	1.67	0.52	21,720	1.85	0.48	198,108	1.65	0.52
Rating	219,828	3.51	0.70	21,720	3.70	0.53	198,108	3.49	0.72
# of Reviews	219,828	185.25	212.76	21,720	336.58	320.90	198,108	168.65	190.14
Physical Size	219,828	723.30	1904.30	21,720	1209.90	2271.10	198,108	669.94	1851.94
Density (zip code)	219,828	12.20	16.12	21,720	34.57	20.44	198,108	9.75	13.48

Price is on a 4 point scale. Rating is on a 5 point scale. Physical size in square meters. Density (zip code) is zip code restaurant density - the number of restaurants per square mile for the zip code that the focal restaurant is in. 219,828 observations from 5,481 unique restaurants (672 neighbor and 4809 control restaurants) for 12 weeks for four iterations of RW, but each restaurant participates in 3.34 iterations, on average.

Table 2: Restaurant Week Participants' Effect on their Neighbors

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.636** (0.266)	0.982*** (0.364)	0.970*** (0.307)	0.920*** (0.262)	0.774*** (0.223)	0.523*** (0.199)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5481	5481	5481	5481	5481	5481
Observations	36664	73328	109992	146656	183320	219984
R ²	0.983	0.944	0.933	0.931	0.932	0.931

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any RW participant. The independent variable is an interaction of a dichotomous variable, Neighbor (equals one if the business is located within 0 miles to 1/4 mi of a RW participant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5481 unique restaurants across iterations in the sample leading to 36,664 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).□

Table 3: Restaurant Week Participants' Effect on their Closer Neighbors (within 1/8 mile)

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.148 (0.150)	0.15 (0.139)	0.019 (0.129)	-0.085 (0.110)	-0.195** (0.099)	-0.200** (0.090)
Neighbor_Closer * Post	0.925* (0.538)	1.575** (0.784)	1.801*** (0.651)	1.903*** (0.554)	1.835*** (0.470)	1.370*** (0.420)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5481	5481	5481	5481	5481	5481
Observations	36664	73328	109992	146656	183320	219984
R ²	0.983	0.944	0.933	0.931	0.932	0.931

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any RW participant. The independent variable is an interaction of a dichotomous variable, Neighbor_Closer (equals one if the neighbor is located within 0 miles to 1/8 mi of a RW participant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5481 unique restaurants across iterations in the sample leading to 36,664 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor_Closer, Neighbor, and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).□

Table 4: Restaurant Week Participants' Effect on Neighbors Selling Similar Goods

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.520** (0.235)	0.718** (0.308)	0.682*** (0.262)	0.615*** (0.223)	0.475** (0.189)	0.282* (0.170)
Neighbor_Similar * Post	1.242 (1.529)	2.825 (2.295)	3.078 (1.913)	3.258** (1.638)	3.208** (1.395)	2.582** (1.242)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5481	5481	5481	5481	5481	5481
Observations	36664	73328	109992	146656	183320	219984
R ²	0.983	0.944	0.933	0.931	0.932	0.931

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The independent variable is an interaction of a dichotomous variable, Neighbor_Similar (equals one if the business is located within 0 - 1/4 miles of a RW participant and is similar to the RW participant on the five matching traits) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5481 unique restaurants across iterations in the sample leading to 36,664 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor_Similar, Neighbor, and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).

Table 5: Restaurant Week Participants' Effect on Similar, Closer Neighbor

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.526** (0.232)	0.733** (0.304)	0.698*** (0.258)	0.635*** (0.219)	0.499*** (0.187)	0.304* (0.167)
Neighbor_Similar_Closer * Post	1.814 (2.165)	4.115 (3.255)	4.491* (2.713)	4.700** (2.322)	4.556** (1.978)	3.616** (1.761)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5481	5481	5481	5481	5481	5481
Observations	36664	73328	109992	146656	183320	219984
R ²	0.983	0.944	0.933	0.931	0.932	0.931

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The independent variable is an interaction of a dichotomous variable, Neighbor_Similar_Closer (equals one if the business is located within 0 - 1/8 miles of a RW participant and is similar to the RW participant on the five matching traits) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5481 unique restaurants across iterations in the sample leading to 36,664 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor_Similar_Closer, Neighbor, and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).

APPENDIX: Binned scatterplots

To further describe our data, we also present binned scatterplots for (i) average daily visits for “Neighbors” versus “Controls” for several weeks before and after RW and (ii) for the key subgroup (closer vs. farther neighbor, similar vs. dissimilar neighbor). Starr and Goldfarb (2020) advocate for binned scatter plots of the nonparametric relationships – the raw data without any control variables – between the dependent and key independent variables; this “...allows researchers to quickly detect the shape of that relationship, examine outliers, and assess which part of the support may be driving a relationship.” Detecting the shape of the relationship in the raw data informs the research as to what parametric relationships might be more appropriate to hypothesize. Versus scatter plots, where it is difficult to observe patterns with large samples, “A binned scatterplot condenses the information from a scatterplot by partitioning the x-axis into bins, and calculates the mean of the dependent variable within each bin.”

First, in Appendix Figure 1a is a baseline binned scatterplot of average daily visits for several weeks before and after RW for “Neighbors” (in red) versus “Controls” (in grey). These are unconditional plots – the effect of possible control variables is not removed. For the neighbor restaurants, there is a small increase in visitations five weeks before RW, which is likely due to the pre-Christmas season that precedes the winter iterations of RW and the July 4th holiday that precedes the summer iterations of RW; but more importantly there appears to be increased visitations from two weeks after RW that persists for a couple weeks. This raw pattern in the data appears appropriate for a differences-in-differences specification to test whether neighbor versus control restaurants experience heightened demand after RW.

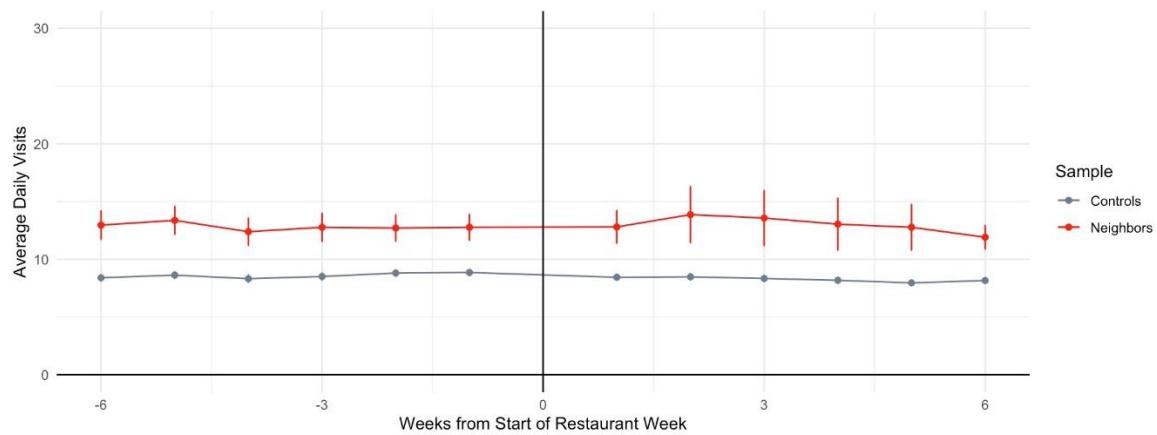
Next in Appendix Figure 1b is a binned scatterplot of average daily visitation split by distance for “Neighbor_Closer” (in blue) and “Neighbor_Farther” (in grey), along with “Neighbors” (both groups together in red) as a reference. These plots indicate that the bulk of increased visitation

for “Neighbors” originates with the closer neighbors. This raw pattern appears appropriate for a formal test of whether closer neighbors experience greater heightened demand than farther neighbors after RW.

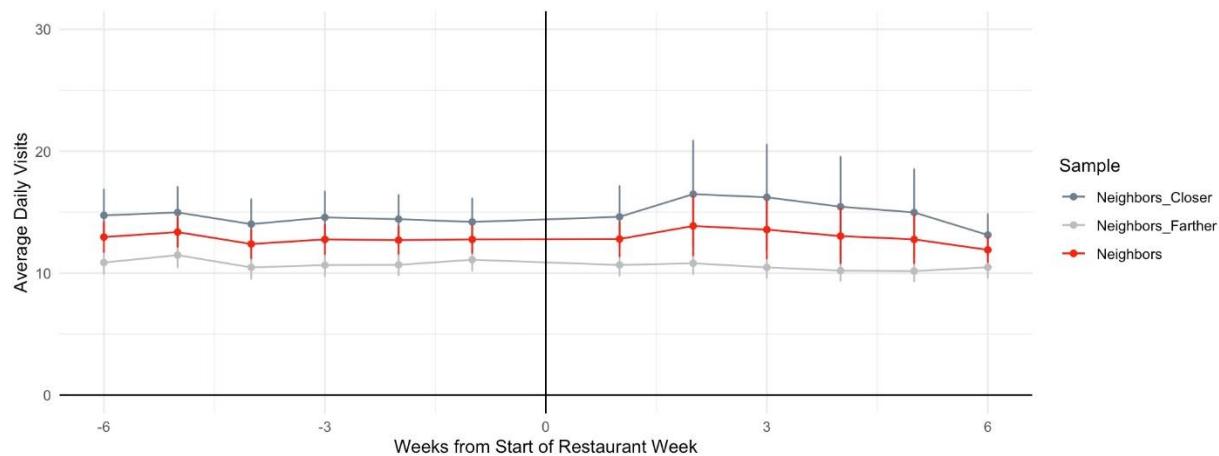
Next in Appendix Figure 1c is a binned scatterplot of average daily visitation split into two groups: neighbors selling similar goods to the RW participant that they neighbor, “Neighbor_Similar” (in blue); and neighbors selling dissimilar goods, “Neighbor_Dissimilar” (in grey), along with “Neighbors” (both groups together in red) as a reference. These plots indicate that before RW that Neighbor_Similar experience fewer visits versus Neighbor_Dissimilar, but there is a reversal starting at two weeks after RW that lasts for a couple weeks. This raw pattern appears appropriate for a formal test of whether similar neighbors experience greater heightened demand than dissimilar neighbors after RW.

Finally in Appendix Figure 1d is a binned scatterplot of average daily visitation for neighbors selling similar goods to RW participant that they neighbor, but that are also closer to this RW participant (within 1/8 versus 1/4 mile), “Neighbor_Similar_Closer” (in blue); and all other neighbors (neighbors selling similar goods that are farther from the RW participant that they neighbor and neighbors selling dissimilar goods), “Neighbor_Not_(Similar_Closer)” (in grey), along with “Neighbors” (both groups together in red) as a reference. These plots indicate that Neighbor_Similar_Closer experience more visitation than other neighbors two weeks after RW, which lasts for a couple weeks. This raw pattern appears appropriate for a formal test of whether similar neighbors that are closer to the RW participant experience different heightened demand than other neighbors after RW.

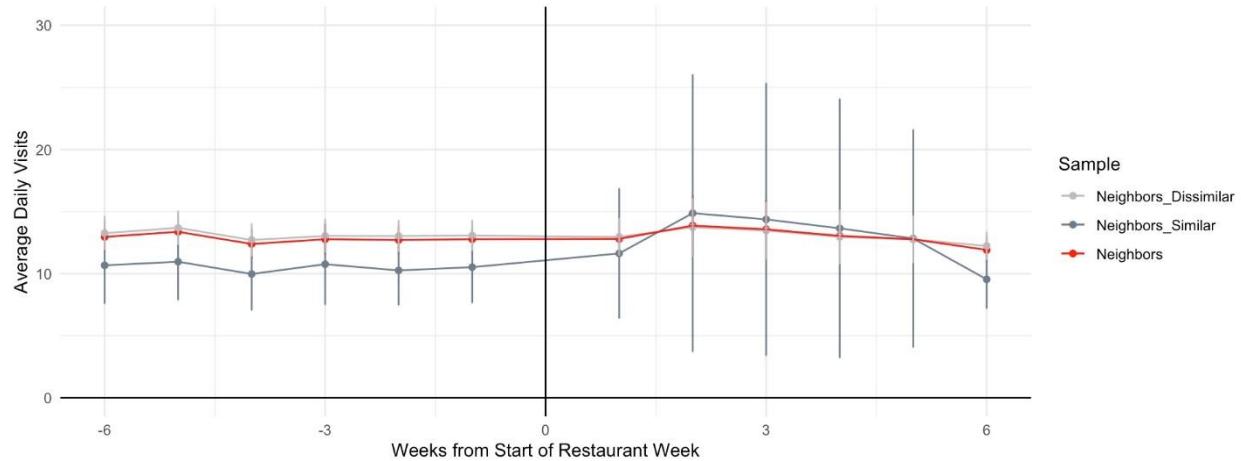
Appendix Figure 1a: Binned Scatterplot: Visits for Neighbor and Control Restaurants



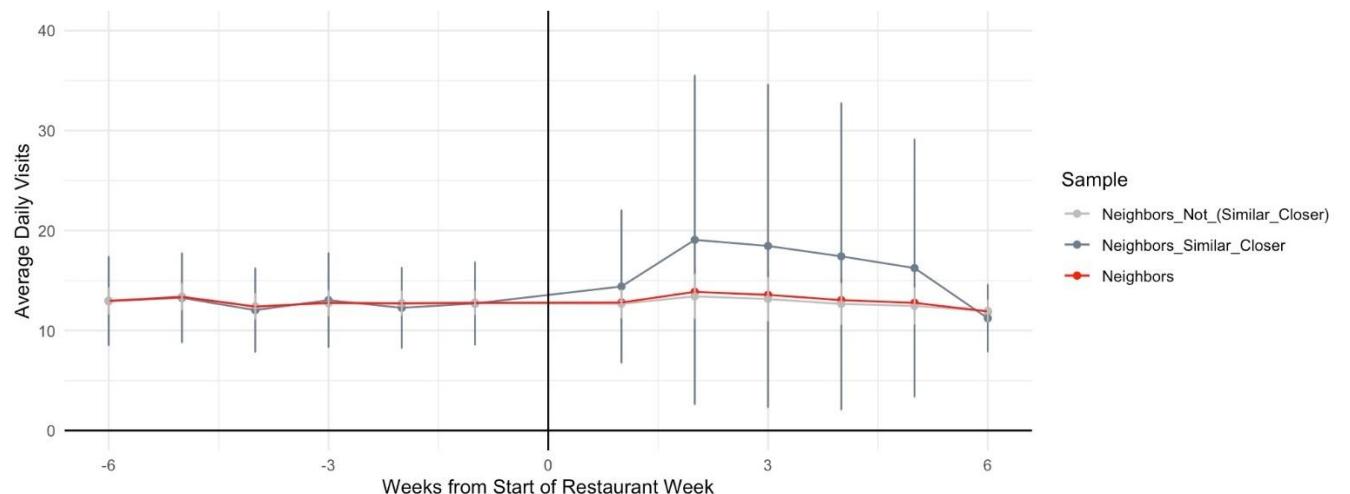
Appendix Figure 1b: Binned Scatterplot: Visits for Neighbor_Closer & Neighbor_Farther



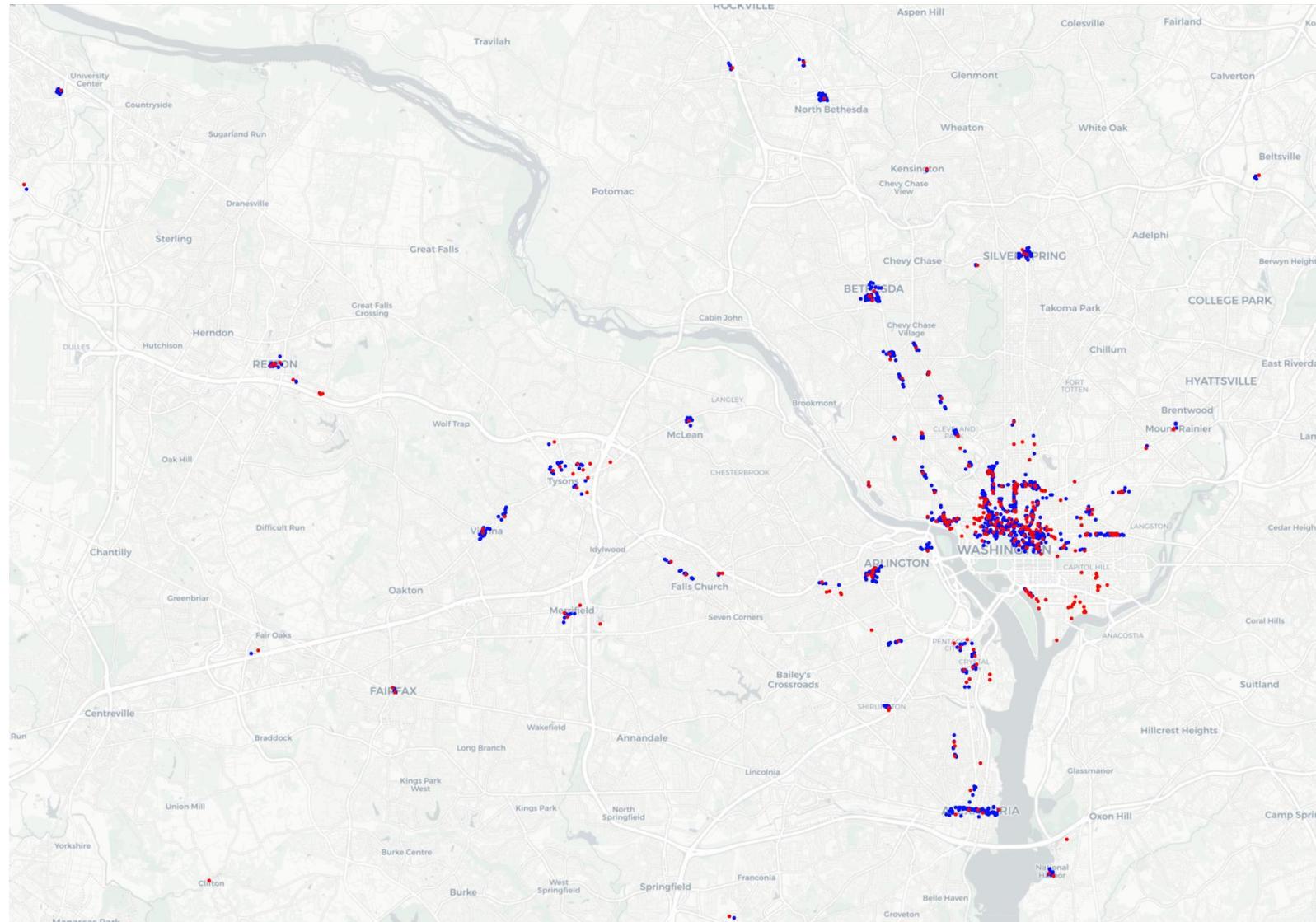
Appendix Figure 1c: Binned Scatterplot: Visits for Neighbor_Similar & Neighbor_Dissimilar



Appendix Figure 1d: Binned Scatterplot: Visits for Neighbor_Similar_Closer

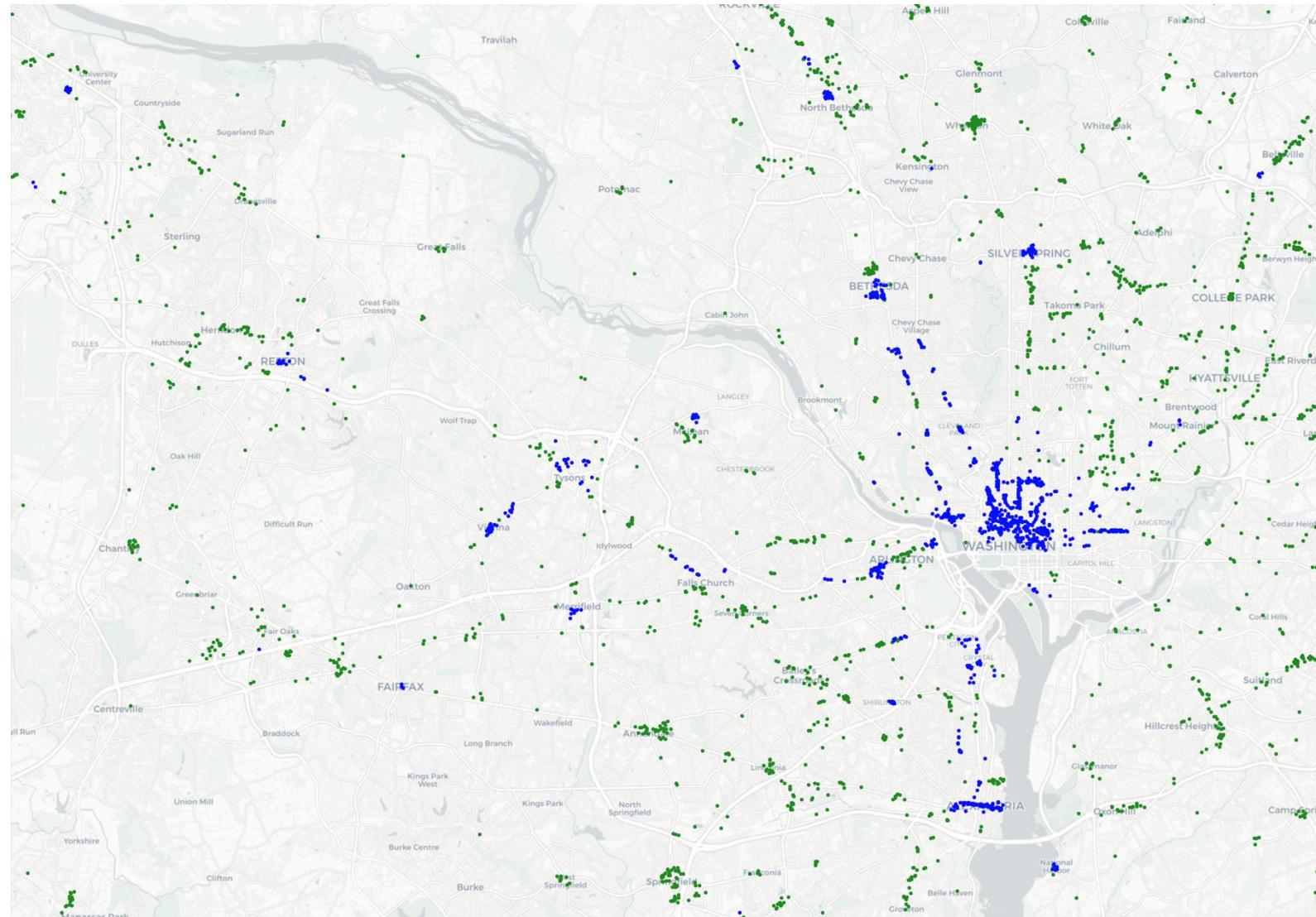


Map of RW participants and neighbors (385 RW participants and 1,460 neighbors)
RW participants in red; neighbors to RW participants in blue.



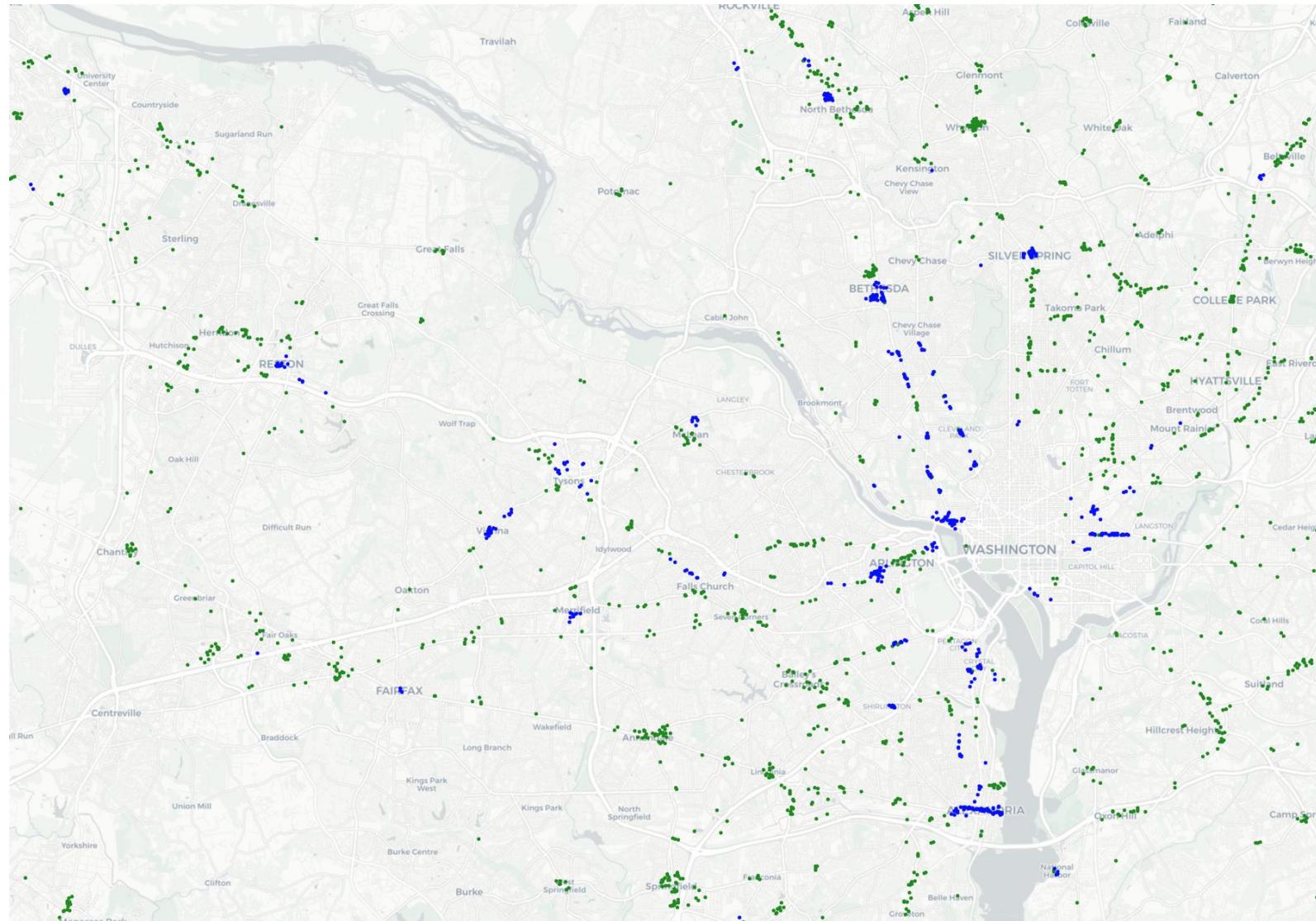
Map of neighbors and control restaurants (1,460 neighbors and 5,888 potential control restaurants)

Neighbors to RW participants in blue; control restaurants in green. Some control restaurants in farther outlying areas cropped.



Map of matched neighbors and control restaurants. (672 neighbors matched to 5,481 control restaurants)

Neighbors to RW participants in blue; control restaurants in green. Some control restaurants in farther outlying areas cropped.



Appendix 7

APPENDIX: Effect of Restaurant Week (RW) on participants' performance

An antecedent to whether heightened demand spills over to action-taking firms' neighbors is that the action-taking firms themselves do experience heightened demand from their demand-shaping action. To test this, we use a DiD model to compare the demand experienced by RW participants versus control restaurants.

Empirical design and methodology

To test whether RW participants experience heightened demand from RW, we use a stacked DiD model, as described in the methodology section of the main document. But here the treatment group is not the neighbors of RW participants but the restaurants participating in RW themselves. We do a difference-in-difference comparison of RW participants, before and after RW; versus control restaurants, before and after RW. And the control restaurants are matched to the RW participants themselves (not neighbors of RW participants, as in the main document). As described in the methodology section of the main document, we use a conditioned DiD specification – include appropriate control variables, which are two sets of interaction fixed-effects: (i) Establishment-RW_iteration and (ii) Zipcode-Week.

Construction of sample to test effect of participating in Restaurant Week

In our starting sample of 8,253 restaurants in the Washington DC metropolitan area, we have 385 RW participants. For the set of preliminary control restaurants, we use restaurants beyond 1/2 mile from any RW participant as a control restaurant, since we do not expect any heightened demand to spill over more than 1/2 mile away, which is 5,888 restaurants. This preliminary set of control restaurants then undergoes two levels of matching using Coarsened Exact Match (CEM, in R with default settings) (Blackwell et al. 2009). We do this because the 5,888 includes restaurants that are dissimilar to RW participants, such as McDonalds, restaurants in food courts, standing-room only restaurants, etc. First, we match the 385 RW participants and 5,888 potential control

restaurants based on the restaurant density in the zip codes they are located in. We calculate zip codes' restaurant densities knowing all restaurants' zip codes from their Yelp addresses, and each zip codes' size in square miles. RW participants and therefore their neighbors tend to be in more dense zip codes – which would affect levels and change in levels of demand; so, we want control restaurants in similarly dense zip codes. These resulting matched restaurants (152 RW participants and 5,802 potential control restaurants), we match a second time based on RW participants' and potential control restaurants' cuisine, price, average rating, and number of reviews received, and physical size. Each review of a restaurant on Yelp is accompanied by a rating value on a five-point scale. Average rating is the simple average of all the ratings obtained till a given date. Since both the number of reviews and average rating are time variant values, we calculate the value of these two variables on the first day of each RW iteration to reflect any additional reviews and ratings obtained by a restaurant in the intervening period. For restaurants without a review, number of reviews is recorded as 0 and average rating as NA. For the cuisine, some restaurants list multiple cuisines, in which case, we consider the first cuisine listed to be the restaurant's cuisine. Finally, each restaurant on Yelp indicates the average price of their offering on a four-point scale. Those restaurants without a price are recorded as NA for the price. Both cuisine and price are time invariant traits. After the two levels of matching, we end up with a sample of 111 RW participants and 2,675 matched control restaurants.

Results

DiD results for the three variants of RW participants and control restaurants are shown in Appendix Tables 2A, 2B, and 2C. Appendix Table 2A is RW participants and control restaurants with no matching (385 RW participants and 5,888 control restaurants). Appendix Table 2B is RW participants and control restaurants matched by zip code restaurant density (152 RW participants and 5,802 control restaurants). Appendix Table 2C is RW participants and control restaurants

matched by zip code restaurant density and five restaurant traits – price, cuisine, rating, number of reviews, and physical size (111 RW participants and 2,675 control restaurants).

Each of tables have six columns, with each column corresponding to a different pre-treatment and post-treatment period duration. The first column is the “1 week” pre/post-period comparison, which is the week before RW versus the week of RW. The second column is the “2 weeks” pre/post-period comparison, which is the two weeks before RW versus RW plus the week following. The third column is the “3 weeks” pre/post-period comparison; the three weeks before RW versus RW plus the two weeks following. And so on, up to “6 weeks” comparison in the sixth column.

For the “1 week” comparison, the coefficient estimates range from 1.695 to 2.560. The 2.560 indicates that RW participants see an increase of 2.560 customer visits a day during RW versus the week beforehand, relative to change in demand for control restaurants. The average RW participant saw 23.34 customers a day (in the SafeGraph data) before RW, so the 2.560 customers represent a 11.0% increase in demand. 2, 3, and 4 weeks comparisons show similar magnitude of effect. The 5 and 6 weeks comparison shows a decreasing magnitude of effect. These results indicate that RW does heighten demand for participants, on average, and that the heightened demand persists beyond just RW for several weeks following.

Appendix Table 1: Variable Balance Statistics

Restaurant Trait	Neighbor Restaurants	Control Restaurants	Normalized Difference
Price	1.85	1.65	0.41
Rating	3.70	3.49	0.33
# of Reviews	336.58	168.65	0.64
Physical Size	1209.99	669.94	0.26
Density (zip code)	18.38	6.96	0.72

This table reports the variable balance between 672 neighbor restaurants and 4809 control restaurants. Shown are each trait's average for each group, and the normalized difference for each trait. Traits averages are calculated using the six weeks of data before the start of RW. Normalized differences greater than 0.25 indicate potential for imbalance between treated (neighbor restaurants) and control groups (Baker et. al 2025; Imbens and Rubin, 2015), and the need for a conditioned DiD specification (control variables should be included) versus an unconditioned specification (no control variables).

Appendix Table 2A: Effect of Participating in Restaurant Week
All RW participants & potential control restaurants
(385 RW participants and 5,888 control restaurants)

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
RW Restaurant * Post	1.695*** (0.590)	1.590** (0.754)	1.435** (0.651)	1.481*** (0.564)	1.180** (0.492)	0.797* (0.440)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	6273	6273	6273	6273	6273	6273
Observations	44376	88752	133128	177504	221880	266256
R ²	0.992	0.981	0.970	0.965	0.963	0.963

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any participating RW Restaurant. The independent variable is an interaction of a dichotomous variable, RW Restaurant (equals one if the business is a participating RW Restaurant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 6273 unique restaurants across iterations in the sample leading to 44,376 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of RW Restaurant and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.1 (two-tailed).

Appendix Table 2B: Effect of Participating in Restaurant Week
RW participants & control restaurants matched by zip code restaurant density only
(152 RW participants matched to 5,802 control restaurants)

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
RW Restaurant * Post	2.069*** (0.732)	1.947** (0.936)	1.839** (0.808)	1.883*** (0.699)	1.497** (0.609)	1.008* (0.544)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5954	5954	5954	5954	5954	5954
Observations	42110	84220	126330	168440	210550	252660
R ²	0.992	0.981	0.971	0.966	0.965	0.964

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any participating RW Restaurant. The independent variable is an interaction of a dichotomous variable, RW Restaurant (equals one if the business is a participating RW Restaurant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5954 unique restaurants across iterations in the sample leading to 42,110 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of RW Restaurant and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.1 (two-tailed).

Appendix Table 2C: Effect of Participating in Restaurant Week
RW participants & control restaurants matched by zip code restaurant density & five restaurant traits

(111 RW participants matched to 2,675 control restaurants).

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
RW Restaurant * Post	2.560** (1.072)	2.774* (1.425)	2.540** (1.210)	2.407** (1.046)	1.865** (0.907)	1.198 (0.808)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	2786	2786	2786	2786	2786	2786
Observations	18252	36504	54756	73008	91260	109512
R ²	0.986	0.960	0.949	0.946	0.945	0.944

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any participating RW Restaurant. The independent variable is an interaction of a dichotomous variable, RW Restaurant (equals one if the business is a participating RW Restaurant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 2786 unique restaurants across iterations in the sample leading to 18,252 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of RW Restaurant and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.1 (two-tailed).

Appendix Table 3: 1/2 mile neighbor definition (versus 1/4 mile definition)

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	1.029*** (0.197)	0.823*** (0.207)	0.625*** (0.182)	0.497*** (0.155)	0.296** (0.136)	0.061 (0.123)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5851	5851	5851	5851	5851	5851
Observations	39266	78532	117798	157064	196330	235596
R ²	0.992	0.973	0.965	0.962	0.961	0.961

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any RW participant. The independent variable is an interaction of a dichotomous variable, Neighbor (equals one if the business is located within 0 miles to 1/2 mi of a RW participant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5851 unique restaurants across iterations in the sample leading to 39,266 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).□

Appendix Table 4A: Restaurant Week Participants' Effect on Neighbors by Distance using continuous distance measure, with 1/4 mile neighbor definition

	Average Weekly Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	2.200** (1.117)	4.033** (1.645)	4.422*** (1.365)	4.515*** (1.162)	4.287*** (0.983)	3.349*** (0.878)
Neighbor_Distance * Post	-12.202* (6.850)	-23.805** (10.105)	-26.927*** (8.372)	-28.045*** (7.125)	-27.401*** (6.029)	-22.047*** (5.383)
Time Horizon	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5481	5481	5481	5481	5481	5481
Observations	36664	73328	109992	146656	183320	219984
R ²	0.983	0.945	0.933	0.931	0.932	0.931

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The independent variable is an interaction of Neighbor, Post, and Distance (in miles to a neighbor's RW participant (or the average distance if a neighbor is within 1/4 mile of multiple RW participants), while control restaurants have their Distance set to 0). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5481 unique restaurants across iterations in the sample leading to 36,664 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor_Distance, Neighbor, and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.1 (two-tailed).

Appendix Table 4B: Restaurant Week Participants' Effect on Neighbors by Distance using continuous distance measure, with 1/2 mile neighbor definition

	Average Weekly Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.79 (0.906)	3.120** (1.300)	3.834*** (1.081)	4.071*** (0.922)	4.014*** (0.784)	3.373*** (0.700)
Neighbor_Distance * Post	0.354 (3.274)	-9.382** (4.549)	-12.918*** (3.793)	-14.312*** (3.236)	-14.889*** (2.765)	-13.231*** (2.471)
Time Horizon	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	5851	5851	5851	5851	5851	5851
Observations	39128	78256	117384	156512	195640	234768
R ²	0.992	0.973	0.965	0.962	0.961	0.960

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The independent variable is an interaction of Neighbor, Post, and Distance (in miles to a neighbor's RW participant (or the average distance if a neighbor is within 1/2 mile of multiple RW participants), while control restaurants have their Distance set to 0). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 5851 unique restaurants across iterations in the sample leading to 39,128 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor_Distance, Neighbor, and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.1 (two-tailed).

Appendix Table 5A: Restaurant Week Participants' Effect on their Neighbors
Neighbors & control restaurants matched by zip code restaurant density only
(840 neighbor restaurants matched to 5,881 control restaurants)

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.741*** (0.265)	1.088*** (0.322)	1.082*** (0.274)	1.081*** (0.235)	0.826*** (0.202)	0.402** (0.186)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	6721	6721	6721	6721	6721	6721
Observations	46102	92204	138306	184408	230510	276612
R ²	0.990	0.970	0.961	0.957	0.957	0.955

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any RW participant. The independent variable is an interaction of a dichotomous variable, Neighbor (equals one if the business is located within 0 miles to 1/4 mi of a RW participant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 6721 unique restaurants across iterations in the sample leading to 46,102 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).

Appendix Table 5B: Restaurant Week Participants' Effect on their Neighbors
All neighbors & potential control restaurants
(1,460 neighbor restaurants and 5,888 potential control restaurants)

	Average Daily Footfall					
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor * Post	0.720*** (0.262)	1.055*** (0.318)	1.042*** (0.271)	1.034*** (0.232)	0.773*** (0.200)	0.351* (0.185)
Pre-Post Treatment Period Duration	1 week	2 weeks	3 weeks	4 weeks	5 weeks	6 weeks
FE: Establishment - Iteration	Yes	Yes	Yes	Yes	Yes	Yes
FE: Zipcode - Week	Yes	Yes	Yes	Yes	Yes	Yes
Unique Establishments in Sample	7348	7348	7348	7348	7348	7348
Observations	48910	97820	146730	195640	244550	293460
R ²	0.989	0.969	0.961	0.957	0.957	0.955

Notes: Robust standard errors in parentheses. The unit of analysis is the establishment-week. The control sample is all restaurants beyond the 1/2 mile radius of any RW participant. The independent variable is an interaction of a dichotomous variable, Neighbor (equals one if the business is located within 0 miles to 1/4 mi of a RW participant) and Post (equals one if the focal week falls on or after the start of RW). There are four iterations in the sample from Summer 2022 to Winter 2024. Total of 7348 unique restaurants across iterations in the sample leading to 48,910 observations (Model 1) with two observations per restaurant (one in each of pre and post treatment week), for each of the four iterations of RW (though every restaurant does not participate in each iteration of RW). The main effects of Neighbor and Post are absorbed by fixed effects.

***p < 0.01; **p < 0.05; *p < 0.01 (two-tailed).