HOW INFORMATION SHAPES THE GEOGRAPHY OF URBAN CONSUMPTION*

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Abstract

Can new information change the geographic boundaries of an urban market? We study this question using professional restaurant reviews and smartphone mobility data from 11 U.S. metropolitan areas. Using staggered synthetic difference-in-differences, we find that review publication immediately increases visits to the reviewed restaurant by 5%, with the effect declining over time. The increase comes from consumers who live both outside and inside the restaurant's trade area, with a larger increase among non-local consumers. Reviews also draw consumers who were not recently observed in the restaurant's census tract, suggesting that information can bring consumers into areas with which they have limited prior exposure. The effects extend beyond the reviewed restaurant. Visits to nearby non-reviewed restaurants increase by 1.9%, driven by non-local consumers, and the estimates remain positive through the eighth week, after the direct effect has faded. Finally, reviews increase visits from majority-White neighborhoods to restaurants in majority-non-White neighborhoods, both at the reviewed restaurant and at nearby restaurants. Visits in the opposite direction also rise, but the estimates are imprecise. Together, these findings show that information can expand the geographic reach of urban restaurant demand, draw consumers across demographic boundaries, and generate demand for an area beyond the establishment receiving the information shock.

Keywords: restaurant demand, information intermediaries, professional reviews, consumer mobility, demand spillovers, cross-demographic mobility

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1 Introduction

In 1994, *The Los Angeles Times* critic Jonathan Gold reviewed Guelaguetza, a family-owned Oaxacan restaurant in Central Los Angeles. Before the review, the restaurant served primarily the local Oaxacan immigrant community. Bricia Lopez, the owners' daughter, recalled her father's surprise at the sudden arrival of White customers: "Where are all these people coming from?" One customer replied: "Don't you know? You're in the LA Times."[1]

This anecdote raises a broader question: *can new information broaden the reach and composition of urban demand?* Demand for restaurants is typically local. Consumers are more likely to visit establishments near where they live and work, and visits and spending decline with travel distance (Agarwal et al., 2017; Davis et al., 2019). Consumers are also less likely to visit restaurants in neighborhoods that differ demographically from their own (Davis et al., 2019). We investigate whether information expands the geographic reach of restaurant demand and changes the neighborhoods from which restaurants draw customers.

We study this question using 564 professional restaurant reviews from major local newspapers across 11 U.S. metropolitan areas, combined with high-frequency smartphone mobility data from Cuebiq. The data allow us to measure weekly restaurant visits and characterize consumers by their home census block groups (CBGs), while also identifying 10,415 non-reviewed restaurants in the same census tracts. We examine whether reviews expand a restaurant's geographic reach, bring consumers into areas they have not recently visited, and draw consumers across demographic boundaries. We also examine spillovers to nearby restaurants, where the review can either increase demand by making the neighborhood a more attractive destination or reduce demand through substitution toward the reviewed restaurant.

A simple model formalizes how a review affects demand for the reviewed restaurant and nearby alternatives. Restaurants can be high or low quality, and consumers are uncertain about their quality. Consumers can be of two types, near and distant, who differ in visit costs,

[1] See the documentary film *City of Gold* (2015).

awareness of the neighborhood and its restaurants, and prior beliefs about quality. A positive review increases beliefs about the reviewed restaurant, awareness of the restaurant, and consideration of its neighborhood, generating both an information effect and a neighborhood-discovery effect. The model predicts a larger proportional direct effect for distant consumers. For nearby restaurants, neighborhood discovery increases demand while substitution toward the reviewed restaurant reduces it, so the spillover is positive when neighborhood discovery outweighs substitution and can be proportionally larger for distant consumers.

We use the mobility data to map these theoretical concepts into observable measures of consumer demand. We classify consumers as local if their home CBG falls within the restaurant's pre-review trade area and as non-local otherwise. We also examine whether reviews bring consumers into areas they have not recently visited, identifying consumers who were and were not observed in the restaurant's census tract during the 30 days before publication. To examine demographic differences, we classify consumers' home CBGs and restaurants' CBGs as majority-White or majority-non-White and compare demographically distant and close visitors at the same restaurants. For the spillover analysis, we examine visits to non-reviewed restaurants within 300 meters of the reviewed restaurant and compare effects across distance and consumer groups.

Reviews are not randomly assigned to restaurants. Critics choose which restaurants to review, and they may choose restaurants that are already growing. However, critics select and visit a restaurant over several weeks, and writing and editorial schedules then determine the publication date. This process generates variation in the timing of reviews: restaurants are reviewed at different points in time. The exact week of publication is hard to anticipate and plausibly unrelated to short-run changes in demand. Because publication dates differ across restaurants, a shock in one calendar week also cannot explain the average change at publication.

Budget and time constraints add a second source of variation. Critics cannot review all restaurants in the same locality, so many similar restaurants are not reviewed, or are

reviewed later. We use this feature in a staggered synthetic difference-in-differences design (Arkhangelsky et al., 2021). For each cohort of restaurants reviewed in the same week, the estimator compares their average outcome with the average outcome of non-reviewed and not-yet-reviewed restaurants over the same calendar weeks. It does so for each week before and after publication. The comparison restaurants are weighted to match the cohort's visit trend before publication. The high-frequency mobility data then allow us to examine whether visits change at publication and in the subsequent weeks. Our identifying assumption is that, absent the review, the cohort and its comparison restaurants would have followed parallel trends. We provide evidence for this assumption with a placebo that moves the publication date one week earlier and does not use the placebo week to construct the weights. We expect an insignificant effect in the week before publication. Section 5 discusses the identifying assumptions and the estimation procedure in detail.

We study the spillover effect to non-reviewed restaurants in the same way. A non-reviewed restaurant within 300 meters of a reviewed restaurant is treated in the week of the first nearby review. Its comparison restaurants are non-reviewed restaurants more than 300 meters from every reviewed restaurant, and non-reviewed restaurants treated later. Section 6.4 reports robustness checks for both effects: longer pre-publication windows for constructing the synthetic control, and a two-stage difference-in-differences estimator (Gardner et al., 2024) with lagged outcomes and Yelp controls (Reimers and Waldfogel, 2021).

We find that reviews change the composition of restaurant demand. Publication increases visits to the reviewed restaurant by about 5% over the first eight post-publication weeks, with the increase beginning in the publication week. A placebo test that moves the publication date one week earlier finds no significant effect before publication. The increase comes from consumers who live both outside and inside the restaurant's trade area. Visits from non-local consumers rise by 4.2%, compared with 2.1% among local consumers, although the difference is only marginally significant. The estimated expansion of the weekly trade area is 4.8%, but it is imprecisely estimated. Reviews also attract consumers with less recent exposure

to the area: visits from consumers who were not observed in the restaurant's census tract during the preceding 30 days increase by 2.9%, compared with 0.9% among recently exposed consumers. Thus, information broadens the set of consumers willing to visit the area.

The effects extend beyond the reviewed restaurant. Visits to non-reviewed restaurants within 300 meters increase by 1.9% after publication. The effect is positive in all four distance groups and weaker and not statistically significant at the outer edge of the 300-meter zone. These additional visits come primarily from outside the nearby restaurants' existing trade areas: visits from non-local consumers increase by 2.6%, while the estimate for local consumers is close to zero. The trade areas of nearby restaurants expand by 5.0%. The positive spillover indicates that, on average, the increase in demand for the area outweighs the business-stealing effect of the reviewed restaurant. Reviews therefore appear to create a broader destination effect rather than simply reallocating visits among existing customers.

The demographic results show that reviews also draw consumers across demographic boundaries. Visits from majority-W home CBGs increase by 7.4% at reviewed restaurants in majority-NW CBGs and by 4.0% at nearby restaurants in majority-NW CBGs. Visits in the opposite direction also increase, but the estimates are imprecise (2.1% at the reviewed restaurant and 0.8% at nearby restaurants). At a given restaurant, demographically distant visitors do not respond consistently more than close visitors. We classify visits by the demographic composition of home and restaurant neighborhoods and do not observe the race of individual visitors, so we view these results as suggestive.

In summary, these findings show that information about a single restaurant can change both the level and composition of urban demand. Reviews draw consumers who were not recently in the area, and they increase visits from majority-W neighborhoods to restaurants in majority-NW neighborhoods. They also generate positive effects for nearby restaurants, indicating that information can expand demand for a location rather than merely shift demand across establishments. More broadly, the results suggest that the geographic boundaries of local markets are not fixed: they depend in part on the information consumers have about

opportunities outside their usual spatial and social environments. Section 2 reviews related work, Section 3 presents the model and empirical predictions, Section 4 describes the data, Section 5 describes the empirical strategy, Section 6 reports the results, Section 7 discusses mechanisms, magnitudes, and limitations, and Section 8 concludes.

2 Related Literature

Our analysis relates to three literatures: frictions in urban consumption, information and restaurant demand, and spillovers across nearby businesses.

2.1 Frictions in Urban Consumption

Consumers do not visit restaurants at random across a city. Where they choose to eat is shaped by distance, familiarity with different areas, and demographic differences between their home neighborhoods and restaurant locations.

Travel costs make more distant destinations less attractive to consumers. Using credit-card transactions, Agarwal et al. (2017) show that consumer spending declines with distance from home, with substantial differences across consumption categories. This concentration of demand near consumers' homes also shapes supply. Waldfogel (2008) shows that the demographic composition of local consumers affects which restaurants and other private goods are offered in a neighborhood.

Familiarity matters as well. Dean et al. (2026) experimentally vary exposure to work locations in Nairobi and show that workers are willing to give up 22% of the median daily wage to avoid an unfamiliar location. A single one-hour visit eliminates this premium.

Consumers' demographics further influence where they go. Using Yelp reviews in New York City, Davis et al. (2019) show that consumers are less likely to visit restaurants in neighborhoods that differ demographically from their own, even after accounting for transit time. Mobility data reveal similar patterns. Wang et al. (2018) find that residents of

predominantly Black and Hispanic neighborhoods remain less exposed to higher-income and White middle-class neighborhoods as they move through cities, while Candipan et al. (2021) show that segregation in mobility is related to, but not fully explained by, residential segregation. Nilforoshan et al. (2023) document similar patterns using large-scale colocation data. Athey et al. (2021) find that racial isolation is lower in the places people visit than in the neighborhoods where they live, but remains substantial in the former.

Distance, lack of familiarity, and demographic differences therefore create distinct frictions that shape where consumers go.

In a setting closely related to ours, Kim (2023) study the response to *New York Times* restaurant reviews. They find that restaurants located in areas with a larger Black population experience a larger increase in Black diners following a review. Information can also change the demographic composition of restaurant visitors. Aneja et al. (2025) show that labeling a restaurant as Black-owned on Yelp increases visits, primarily from White customers and primarily at restaurants in majority-White neighborhoods.

Some destinations are more likely than others to induce consumers to overcome these frictions. de la Prada and Small (2024) show that certain types of destinations are particularly likely to draw consumers to demographically different neighborhoods. Relatedly, we ask whether information about a specific destination can expand where consumers are willing to go.

2.2 Information and Restaurant Demand

Restaurants are experience goods, meaning their quality is difficult to judge before consumption (Nelson, 1970). Information from critics, consumers, and other sources can therefore affect demand by changing what consumers know and believe. Similar effects have been documented for other experience goods, including movies, wine, and books (Reinstein and Snyder, 2005; Hilger et al., 2011; Villas-Boas et al., 2021).

In restaurants specifically, Heiman (1997) document an approximately 11% change in

revenue in the month following a professional review.[2] Luca (2016) shows that Yelp ratings affect restaurant revenue, and Luca et al. (2026) finds that establishing a Yelp listing also increases revenue.

Despite the decline in newspaper circulation and readership over the past two decades (Abernathy, 2018), Reimers and Waldfogel (2021) provide evidence that professional critics retain real economic influence on book demand.

2.3 Spillovers Across Nearby Businesses

A signal about one seller can increase demand for the surrounding market or divert demand from nearby competitors. The literature provides evidence of both effects.

Several papers find positive spillovers to other sellers. Guryan and Kearney (2008) show that after a retailer sells a large-prize winning lottery ticket, ticket sales rise throughout the ZIP code rather than simply shifting toward the winning store. Sahni (2016) finds that restaurant advertising also increases sales at non-advertised restaurants, while Sorensen (2007) finds that the market-expansion effect of bestseller lists appears to outweigh business stealing from other books. Liang et al. (2019) and Karaman et al. (2023) document similar positive spillovers in other settings.

Other papers find substitution instead. Dewan et al. (2023) show that Airbnb Plus certification diverts bookings from nearby uncertified listings. Bairathi et al. (2025) find that a quality badge on an e-commerce platform reduces clicks and orders for competing products displayed nearby. Reschke et al. (2018) show that a prestigious prize for one academic article reduces citations to related work.

Restaurant reviews may be particularly likely to generate positive spillovers because establishments serving similar cuisines tend to cluster (Liu et al., 2018), and such clustering can create demand externalities across nearby restaurants (Leonardi and Moretti, 2023). A review of one restaurant may also raise consumers' beliefs about the quality of the surrounding

[2] They do not report the direction of the effect, and the sample is small (37 restaurants). The effect dissipates in subsequent months.

area (Glaeser et al., 2001), making neighboring restaurants more attractive.

3 Model and Empirical Predictions

We present a simple model of how a positive professional review changes visits to the reviewed restaurant and a nearby non-reviewed restaurant, and derive empirical predictions.

3.1 Setup

The market consists of M consumers, J neighborhoods, and N restaurants. Each restaurant has quality $\tilde{V} \in \{V^L, V^H\}$, where $V^L < V^H$. At time t^*, a food critic publishes a review of restaurant j in neighborhood ω. We focus on this neighborhood throughout and suppress the neighborhood subscript to simplify notation. We study the effect of the review on visits to restaurant j and to a nearby non-reviewed restaurant k. Consumers are either local (L) or non-local (N) relative to the neighborhood, indexed by $i \in \{L, N\}$. Let M_i denote the number of consumers in group i. We denote the periods immediately before and after the review by $t^* - 1$ and $t^* + 1$, respectively.

We model restaurant choice in two stages. First, restaurants enter the consumer's consideration set. Second, the consumer chooses among the restaurants in her consideration set. Let $\alpha_i^t \in (0, 1]$ denote the probability that a consumer in group i considers dining in neighborhood ω at time t. Let $p_{ir}^t \in (0, 1]$ denote the probability that the consumer is aware of restaurant $r \in \{j, k\}$, conditional on considering the neighborhood. For simplicity, we assume that awareness of j and k is independent conditional on considering the neighborhood.

$$\Pr\{r \text{ enters } i\text{'s consideration set at } t\} = \alpha_i^t p_{ir}^t. \tag{1}$$

Let $\mu_{ir}^t \in (0, 1)$ denote consumer i's belief that restaurant r is high quality. The consumer's expected utility from visiting restaurant r, where the expectation is over the restau-

rant's unknown quality, is

$$U_{ir}^t = \mu_{ir}^t V^H + (1 - \mu_{ir}^t)V^L - c_i + \varepsilon_{ir}, \qquad r \in \{j, k\}, \tag{2}$$

where $c_i > 0$ is the consumer's cost of visiting the neighborhood and ε_{ir} is an idiosyncratic taste shock drawn independently from a type-I extreme value distribution. Because j and k are nearby, the same cost applies to both restaurants. The cost c_i can capture both geographic and social frictions associated with visiting the neighborhood. The outside option yields utility ε_{i0}, drawn from the same distribution. For notational convenience, define

$$x_{ir}^t \equiv \exp\{\mu_{ir}^t V^H + (1 - \mu_{ir}^t)V^L - c_i\}, \qquad r \in \{j, k\}. \tag{3}$$

Given neighborhood consideration and restaurant awareness, the type-I extreme-value taste shocks imply the following choice probabilities:

$$\mathbb{P}_{ij}^t = \alpha_i^t p_{ij}^t \left[p_{ik}^t \frac{x_{ij}^t}{1 + x_{ij}^t + x_{ik}^t} + (1 - p_{ik}^t) \frac{x_{ij}^t}{1 + x_{ij}^t} \right], \tag{4}$$

$$\mathbb{P}_{ik}^t = \alpha_i^t p_{ik}^t \left[p_{ij}^t \frac{x_{ik}^t}{1 + x_{ij}^t + x_{ik}^t} + (1 - p_{ij}^t) \frac{x_{ik}^t}{1 + x_{ik}^t} \right]. \tag{5}$$

Non-local consumers face a higher cost of visiting the area due to greater geographic and/or demographic distance from their home neighborhood. They are also less likely to consider the neighborhood and, conditional on considering it, are less likely to be aware of its restaurants before the review. Because local consumers are more familiar with restaurants in their neighborhood, they have more precise prior beliefs. Since we focus on positive reviews, we assume that local consumers have more favorable prior beliefs about the reviewed restaurant, while non-local consumers have a neutral prior.

Assumption 1 (Local v Non-Local Parameters)**.**

$$\alpha_N^{t^*-1} < \alpha_L^{t^*-1}, \quad p_{Nj}^{t^*-1} < p_{Lj}^{t^*-1}, \quad p_{Nk}^{t^*-1} < p_{Lk}^{t^*-1}, \quad c_N > c_L, \quad \mu_{Nj}^{t^*-1} = \frac{1}{2} < \mu_{Lj}^{t^*-1} < 1. \tag{6}$$

3.1.1 The Effect of a Critic's Review

We model the review as a binary signal about restaurant j's quality, $s \in \{+, -\}$. The signal is informative: $P(s = + \mid V^H) > P(s = + \mid V^L)$. We focus on positive reviews, consistent with the fact that professional reviews in our sample are predominantly positive.

Consideration and awareness. For simplicity, we assume that everyone in the relevant market is exposed to the review. Exposure increases both the probability that a consumer considers the neighborhood and the probability that she is aware of the reviewed restaurant j. We assume that the review does not change awareness of the neighboring restaurant k:

$$\alpha_i^{t^*+1} = \alpha_i^{t^*-1} + \left(1 - \alpha_i^{t^*-1}\right)\theta \geq \alpha_i^{t^*-1}, \qquad p_{ij}^{t^*+1} = p_{ij}^{t^*-1} + \left(1 - p_{ij}^{t^*-1}\right)\theta \geq p_{ij}^{t^*-1}, \tag{7}$$

$$p_{ik}^{t^*+1} = p_{ik}^{t^*-1}. \tag{8}$$

The parameter $0 \leq \theta \leq 1$ captures how effectively exposure to the review changes neighborhood consideration and awareness of the reviewed restaurant. For simplicity, the same parameter governs both updates.

Beliefs. After observing a positive review, the consumer updates her belief about restaurant j's quality using Bayes' rule. Because the review provides no information about restaurant k's quality, her belief about k remains unchanged:

$$\mu_{ij}^{t^*+1} = \frac{P(s = + \mid V^H)\mu_{ij}^{t^*-1}}{P(s = + \mid V^H)\mu_{ij}^{t^*-1} + P(s = + \mid V^L)(1 - \mu_{ij}^{t^*-1})} > \mu_{ij}^{t^*-1}, \tag{9}$$

$$\mu_{ik}^{t^*+1} = \mu_{ik}^{t^*-1}. \tag{10}$$

3.2 Results

Our empirical analysis uses $Y_{i,t} = \log(\text{visits}_{i,t} + 1)$ as the outcome. For sufficiently large visit counts, $\log(\text{visits} + 1) \approx \log(\text{visits})$, so the estimated treatment effect can be interpreted

approximately as a proportional change in visits. To connect the model to the empirical specification, we therefore characterize the review effect as the proportional change in expected visits.

Let R_{ir}^{t} denote the number of visitors from group i to restaurant r at time t, and let R_{ir}^{0,t^*+1} denote the counterfactual number of visits at t^*+1 in the absence of the review. Since $\mathbb{E}[R_{ir}^{t}] = M_i \mathbb{P}_{ir}^{t}$ and, absent the review, $\mathbb{P}_{ir}^{0,t^*+1} = \mathbb{P}_{ir}^{t^*-1}$, the proportional change in expected visits is $\dfrac{\mathbb{E}[R_{ir}^{t^*+1}]}{\mathbb{E}[R_{ir}^{0,t^*+1}]} = \dfrac{\mathbb{P}_{ir}^{t^*+1}}{\mathbb{P}_{ir}^{t^*-1}}$. A ratio above one implies an increase in expected visits, while a ratio below one implies a decrease. For $r = j$, the ratio characterizes the direct effect; for $r = k$, it characterizes the spillover effect.

3.2.1 Direct Effect

Proposition 1. *A positive review increases expected visits to the reviewed restaurant j for both local and non-local consumers:* $\dfrac{\mathbb{E}\left(R_{ij}^{t^*+1}\right)}{\mathbb{E}\left(R_{ij}^{t^*-1}\right)} = \dfrac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}} > 1$ *for* $i \in \{L, N\}$.

See Online Appendix A.1 for the proof.

Proposition 1 shows that a positive review increases visits to the reviewed restaurant for all consumers by increasing neighborhood consideration, restaurant awareness and beliefs about the restaurant's quality.

Lemma 1. *For $\theta > 0$ and $i \in \{L, N\}$,*

$$\frac{\partial}{\partial \alpha_i^{t^*-1}} \left(\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}} \right) < 0, \qquad \frac{\partial}{\partial p_{ij}^{t^*-1}} \left(\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}} \right) < 0, \qquad \frac{\partial}{\partial c_i} \left(\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}} \right) > 0, \tag{11}$$

$$\frac{\partial \left(\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1} \right)}{\partial \mu_{ij}^{t^*-1}} < 0 \qquad \textit{for } \mu_{ij}^{t^*-1} \geq \frac{1}{2}. \tag{12}$$

See Online Appendix A.2 for the proof.

The first two comparative statics have a similar intuition: lower initial neighborhood consideration or restaurant awareness produces a larger proportional response to the review. Higher travel costs also increase the proportional response by making the restaurants less

attractive relative to the outside option. Finally, consumers with more favorable prior beliefs learn less from a positive review.

Proposition 2. *Suppose $\theta > 0$. There exists a finite c^* such that, for all $c_N > c^*$:*

(a) Non-local consumers have a larger proportional increase in visits: $\dfrac{\mathbb{P}_{Nj}^{t^*+1}}{\mathbb{P}_{Nj}^{t^*-1}} > \dfrac{\mathbb{P}_{Lj}^{t^*+1}}{\mathbb{P}_{Lj}^{t^*-1}}$.

(b) The non-local share of visits increases: $\dfrac{M_N \mathbb{P}_{Nj}^{t^*+1}}{M_L \mathbb{P}_{Lj}^{t^*+1} + M_N \mathbb{P}_{Nj}^{t^*+1}} > \dfrac{M_N \mathbb{P}_{Nj}^{t^*-1}}{M_L \mathbb{P}_{Lj}^{t^*-1} + M_N \mathbb{P}_{Nj}^{t^*-1}}$.

See Online Appendix A.3 for the proof.

Proposition 2 shows that, for a sufficiently large travel cost, a positive review generates a larger proportional increase in visits from non-local consumers. Their larger response reflects lower initial neighborhood consideration and restaurant awareness, less favorable prior beliefs, and higher costs of visiting the neighborhood. As a result, the share of visits from non-local consumers also increases following the review.

3.2.2 Spillovers to a Nearby Restaurant

Proposition 3. *Suppose $\theta > 0$. For consumers in group i, the proportional effect of a positive review of restaurant j on visits to a neighboring restaurant k is*

$$\frac{\mathbb{P}_{ik}^{t^*+1}}{\mathbb{P}_{ik}^{t^*-1}} = \left(\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}}\right) \left(\frac{\dfrac{p_{ij}^{t^*+1}}{1 + x_{ij}^{t^*+1} + x_{ik}^{t^*-1}} + \dfrac{1 - p_{ij}^{t^*+1}}{1 + x_{ik}^{t^*-1}}}{\dfrac{p_{ij}^{t^*-1}}{1 + x_{ij}^{t^*-1} + x_{ik}^{t^*-1}} + \dfrac{1 - p_{ij}^{t^*-1}}{1 + x_{ik}^{t^*-1}}} \right). \tag{13}$$

The first factor captures the neighborhood discovery effect and is weakly greater than one, while the second factor captures the substitution effect toward the reviewed restaurant and is strictly less than one. The spillover is positive if and only if the neighborhood discovery effect dominates the substitution effect. In particular:

(a) The proportional spillover is decreasing in pre-review neighborhood consideration, $\dfrac{\partial}{\partial \alpha_i^{t^*-1}} \left(\dfrac{\mathbb{P}_{ik}^{t^*+1}}{\mathbb{P}_{ik}^{t^*-1}} \right) < 0$.

(b) *If* $\alpha_L^{t^*-1} = 1$*, the review generates a negative spillover among local consumers,* $\frac{\mathbb{P}_{Lk}^{t^*+1}}{\mathbb{P}_{Lk}^{t^*-1}} < 1$.

(c) *If* $\alpha_N^{t^*-1} < 1$*, there exists a finite* c^* *such that, for all* $c_N > c^*$*, the review generates a positive spillover among non-local consumers,* $\frac{\mathbb{P}_{Nk}^{t^*+1}}{\mathbb{P}_{Nk}^{t^*-1}} > 1$.

(d) *If the conditions in (b) and (c) hold, the share of visits to the neighboring restaurant from non-local consumers increases following the review,*
$\frac{M_N \mathbb{P}_{Nk}^{t^*+1}}{M_L \mathbb{P}_{Lk}^{t^*+1} + M_N \mathbb{P}_{Nk}^{t^*+1}} > \frac{M_N \mathbb{P}_{Nk}^{t^*-1}}{M_L \mathbb{P}_{Lk}^{t^*-1} + M_N \mathbb{P}_{Nk}^{t^*-1}}$.

See Online Appendix A.4 for the proof.

The neighborhood discovery effect increases visits to the neighboring restaurant by increasing neighborhood consideration, while the substitution effect reduces visits by making consumers more likely to choose the reviewed restaurant. For local consumers who already consider the neighborhood with probability one, only the substitution effect remains, resulting in a negative spillover. For non-local consumers facing sufficiently high costs, the substitution effect becomes negligible, so the neighborhood discovery effect dominates and the spillover is positive. Together, these results imply that the share of visits to the neighboring restaurant from non-local consumers increases following the review.

3.3 Empirical Predictions

In the empirical analysis, we observe consumer and neighborhood characteristics, including geographic and demographic relationships between consumers and restaurant neighborhoods. Many of the model's primitives are not directly observed, including the cost of visiting the neighborhood, c_i, restaurant awareness, p_{ir}^t, and prior beliefs about restaurant quality, μ_{ir}^t.

We proxy for the pre-review probability of considering the neighborhood, $\alpha_i^{t^*-1}$, using consumers' recent physical exposure to the restaurant's census tract. Recent exposure provides an opportunity to become familiar with the neighborhood as a dining destination, making consumers more likely to consider it when deciding where to eat. This interpretation

is consistent with evidence that unfamiliar neighborhoods are less likely to enter an individual's consideration set and that experimentally induced exposure reduces this gap (Dean et al., 2026).

We first consider predictions related to consumers' geographic relationship to the restaurant neighborhood. Proposition 1, Lemma 1, and Proposition 2 imply the following:

Empirical Prediction 1 (Direct Effect)**.** *A positive critic's review:*

(a) increases visits to the reviewed restaurant;

(b) generates a larger proportional increase among consumers who have not been recently exposed to the neighborhood.

When non-local consumers face sufficiently high costs of visiting the neighborhood, the review:

(c) generates a larger proportional increase in visits from non-local consumers;

(d) increases the reviewed restaurant's share of visits from non-local consumers.

Proposition 3 implies the following predictions about spillovers:

Empirical Prediction 2 (Spillover Effect)**.** *A positive review:*

(a) generates a larger proportional spillover among consumers who are initially less likely to consider the neighborhood as a dining destination;

(b) generates a negative spillover for local consumers who consider the neighborhood with probability one;

(c) generates a positive spillover for non-local consumers who face sufficiently high costs of visiting the neighborhood;

(d) increases the share of visits to the neighboring restaurant from non-local consumers when (b) and (c) hold.

We next examine whether reviews generate visits across demographic boundaries. We define consumers as demographically distant when their home neighborhood and the restaurant neighborhood differ in whether they are majority-White or majority-non-White. We treat demographically distant consumers as analogous to non-local consumers in the geographic analysis. For example, for a restaurant in a majority-White neighborhood, consumers from majority-White neighborhoods are demographically close, while those from majority-non-White neighborhoods are demographically distant.

We apply the same assumptions to demographic distance: the differences specified in Assumption 1 are assumed to hold between demographically distant and demographically close consumers. We obtain the following predictions:

Empirical Prediction 3 (Cross-Demographic Visits)**.** *A positive review:*

(a) increases visits to the reviewed restaurant among both demographically close and demographically distant consumers;

(b) generates a larger proportional increase in visits to the reviewed restaurant among demographically distant consumers;

(c) generates a larger proportional spillover to neighboring restaurants among demographically distant consumers;

(d) generates a positive spillover to neighboring restaurants among demographically distant consumers.

4 Background and Data

4.1 Professional Restaurant Reviews

Professional food critics follow practices designed to preserve the independence of their reviews. Critics typically dine anonymously, make multiple visits, pay for their meals, and

avoid special treatment. The *Association of Food Journalists* describes these practices in its code of ethics.[3] These practices also matter for the timing of treatment in our empirical design. A restaurant generally does not know which anonymous visits are part of a critic's evaluation or the date on which the review will be published.

Professional critics publish relatively few negative reviews.[4] Only 2% of the reviews in our sample are classified as negative (Online Appendix B). We therefore treat publication primarily as a positive signal about the reviewed restaurant.

4.2 Data Sources

We collaborate with Cuebiq[5], a location intelligence platform, to obtain geolocation data from consenting mobile devices. The data begin on January 1, 2019, which determines the start of the analysis period. We end the sample in mid-March 2020, before the first COVID-19 lockdowns sharply changed restaurant activity and urban mobility. Later privacy changes also reduced the coverage of mobile location data, particularly for iOS devices (Bian et al., 2023). The primary unit of observation is the restaurant-week.

Critics' reviews. We collect professional restaurant reviews across 11 U.S. metropolitan areas. For each market, we identify the largest-circulation local newspaper with an active professional food critic. Table I reports review counts by publication, and Online Appendix B describes the collection procedure in detail.

Our treatment is a published review of one restaurant. We retain reviews centered on an

[3]See https://www.poynter.org/ethics-trust/2021/association-of-food-journalists-code-of-ethics/.

[4]For example, *The Guardian* critic Jay Rayner stated, "if I can't be generally positive, I won't review and will move on." See https://www.theguardian.com/food/2020/sep/17/negative-restaurant-reviews-on-pause-lockdown-heres-why-jay-rayner.

[5]Aggregated mobility data is provided by Cuebiq (https://cuebiq.com/), a location intelligence platform. Data is collected from anonymized users who have opted in to provide access to their location data anonymously, through a CCPA and GDPR-compliant framework. Through its Social Impact program, Cuebiq provides mobility insights for academic research and humanitarian initiatives. The Cuebiq responsible data sharing framework enables research partners to query anonymized and privacy-enhanced data, by providing access to an auditable, on-premise Data Cleanroom environment. All final outputs provided to partners are aggregated in order to preserve privacy.

individual establishment rather than guides, lists, or articles covering multiple restaurants. For each review, we observe the publication date, restaurant name, and street address. Reviews are published throughout the sample, so treatment dates occur in many different calendar weeks.

Non-reviewed restaurants. Professional critics primarily review independent restaurants; Online Appendix B.1.1 documents this pattern. We therefore use SafeGraph Places to identify independent, full-service restaurants (NAICS 722511) in the same census tracts as reviewed restaurants. We exclude limited-service establishments such as fast-food chains. The resulting sample contains 10,415 non-reviewed restaurants. Non-reviewed restaurants within 300 meters of a reviewed restaurant form the treated group in the spillover analysis, and those farther away serve as controls (Section 5).

Restaurant information. We match restaurants to Yelp using establishment name and address and obtain GPS coordinates and operating hours from the matched listings.

Foot traffic. We collaborate with Cuebiq, a location intelligence provider that collects geolocation data from consenting mobile devices. We infer a restaurant visit when a device is observed within 25 meters of a restaurant during operating hours and remains at the location for between 4 and 180 minutes (Athey et al., 2018; Shoshani et al., 2024). Additional filters exclude observations consistent with employees and nearby residents. Online Appendix B provides the full procedure.

For each device, the data provider infers a home CBG from nighttime location patterns. We use the home CBG to measure home-to-restaurant distance and demographic origin. We also measure recent area exposure: whether the device was observed in the restaurant's census tract at least once during the preceding 30 days.

Demographic origin and destination. We link home and restaurant CBGs to American Community Survey data. Following Athey et al. (2021), we classify a CBG as majority-White ("majority-W") when the non-Hispanic White population share exceeds 50%, and as majority-non-White ("majority-NW") otherwise. We refer to a cross-demographic visit as a visit from a majority-W home CBG to a restaurant in a majority-NW CBG, or from a majority-NW home CBG to a restaurant in a majority-W CBG.

4.3 Descriptive Statistics

Table II summarizes the 564 reviewed restaurants and 10,415 non-reviewed restaurants used in the estimation. For reviewed restaurants, characteristics defined relative to treatment are measured before publication. For non-reviewed restaurants, the table reports the corresponding characteristics over their observed analysis sample. Mean weekly visits are 18 for reviewed restaurants and 25 for non-reviewed restaurants.

The table also describes where restaurant demand originates. Approximately 40% of visits to reviewed restaurants and 42% of visits to non-reviewed restaurants come from outside the restaurant's trade area, using the trade-area definition in Section 6.1. We also observe many visits from devices that had not been observed in the restaurant's census tract in the preceding 30 days: 76% of visits to reviewed restaurants and 74% of visits to non-reviewed restaurants. These consumers have had less recent exposure to the neighborhood and its restaurants and may therefore be less familiar with the area.

The origin-destination counts also show demographic sorting. Restaurants in majority-W CBGs receive 68% of visits from majority-W home CBGs and 32% from majority-NW home CBGs. Similarly, restaurants in majority-NW CBGs receive 58% of visits from majority-NW home CBGs and 42% from majority-W home CBGs. In both cases, the in-group share exceeds the cross-demographic share. This pattern is consistent with prior evidence on segregation in restaurant consumption and everyday urban mobility (Davis et al., 2019; Athey et al., 2021).

5 Empirical Approach

We examine the causal effect of a critic's review on restaurant foot traffic. Outcomes include total visits, visits by consumer group, and the restaurant's trade area. Except for the trade-area analysis, the dependent variable is $\ln(\text{Visits} + 1)$, where visits refer to all consumers or a particular group. Our estimator is staggered synthetic difference-in-differences (SDID).

5.1 Identification Strategy

Consider the direct effect of a review on the reviewed restaurant and the spillover effect on a nearby non-reviewed restaurant. Two sources of selection pose identification challenges. First, reviewed restaurants may differ systematically from non-reviewed restaurants: they may be higher quality or newer, or may have received more publicity or experienced greater demand prior to the review. Second, publication timing could create bias if critics scheduled reviews to coincide with short-term increases in demand.

We first consider publication timing. A critic generally decides to evaluate a restaurant a few weeks before publication since the process typically involves several restaurant visits followed by preparation and editorial review. Selection would therefore be a concern if critics could anticipate a sharp, restaurant-specific increase in demand several weeks in advance and coordinate publication with it. However, the critic cannot know in advance exactly when the editorial process will be completed, making it difficult to target a particular publication week. Moreover, because critics typically dine anonymously, restaurants are unlikely to anticipate an upcoming review and take actions in response before it appears.

Although our analysis is at the weekly level, the daily pattern of publication is informative about how publication dates are set. Online Appendix Figure D.2 shows that outlets tend to publish reviews on fixed days of the week (e.g., Seattle Times on Thursdays). This regularity is consistent with a fixed editorial calendar that determines when a finished review appears. It is difficult to reconcile with publication dates chosen in response to the circumstances of

individual restaurants.

A separate concern is that demand may vary systematically across calendar weeks. We address this by aligning each restaurant relative to its own publication week and comparing restaurants reviewed in the same week with controls observed over the same calendar weeks. Thus, a demand shock common to a particular calendar week cannot by itself generate an average change at publication across the sample. To assess whether publication activity itself varies systematically around federal holidays, Online Appendix Figure D.3 plots the daily number of critic reviews published during the analysis period and marks U.S. federal holidays. The figure shows no clear systematic change in review volume around these dates.

Assumption 2 (Timing)**.** *Conditional on the restaurant being selected for review, the exact publication week is unrelated to changes in demand that would otherwise occur during the short estimation window.*

We cannot verify Assumption 2 directly. We provide evidence for it in three ways. The first is the weekday pattern of publication and the absence of a holiday pattern, discussed above. The second is the placebo test, which we describe after Assumption 3 below. The third is the timing of the response: we examine whether visits change in the publication week itself and in the following weeks, and whether they change before publication.

Controlling for Selection in the Direct-Effect Estimation

Selection into review is a separate concern. Critics may choose restaurants that are already becoming more popular. Online Appendix Figure D.1 shows that reviewed restaurants tend to gain visits before publication. Without an appropriate comparison group, this pre-existing growth could be attributed incorrectly to the review.

Critics have limited budget and time and cannot review all restaurants in the same locality. Many similar restaurants are therefore not reviewed, or are reviewed later. We use these restaurants as controls: non-reviewed restaurants and not-yet-reviewed restaurants

(the *Both* specification, defined in Section 5.2) with similar pre-publication outcome trajectories. To avoid contamination from spillovers, we use only non-reviewed restaurants located more than 300 meters from every reviewed restaurant (we explain this threshold choice later in the section). SDID places more weight on control restaurants whose outcomes track those of reviewed restaurants during the eight weeks before publication, helping account for factors already affecting demand, such as underlying growth, ongoing marketing, or increasing social-media attention. We apply the same procedure separately to each consumer-group outcome, matching treated and control restaurants on the corresponding pre-publication outcome trajectory.[6]

Assumption 3 (Conditional Parallel Trends: Reviewed Restaurants)**.** *Conditional on their matched pre-publication outcome paths, the reviewed restaurants and their controls would have experienced similar changes in the outcome over the short post-publication window if the review had not been published.*

Again, we cannot test Assumption 3 directly, because we do not observe the outcomes of reviewed restaurants after publication in the absence of a review. We therefore provide supportive evidence. A common way to assess parallel trends is to estimate an event study and examine the effects in each pre-treatment period. Effects close to zero and statistically insignificant are taken to support the assumption. In our setting, however, these pre-publication estimates may not be informative on their own. SDID chooses the control weights so that the weighted controls track the reviewed restaurants over the eight weeks before publication. The pre-publication estimates are therefore close to zero by construction.

Our main evidence is a placebo test that does not have this feature. We move each publication date one week earlier. We then construct the SDID weights using only weeks -9 to -2 relative to the true publication date, and we treat week -1 as the placebo publication

[6]For example, consider majority-W-origin visits to reviewed restaurants in majority-NW CBGs. The treated restaurants are reviewed restaurants in majority-NW CBGs. The potential-control pool consists of non-reviewed restaurants located more than 300 meters from every reviewed restaurant, and SDID places weight on those restaurants whose pre-publication trajectories in majority-W-origin visits are most similar to the treated restaurants.

week. The weights do not use week -1. The placebo estimate therefore shows whether controls that match the reviewed restaurants over the earlier weeks continue to track them in the following week. If critics chose restaurants on a steeper growth path than their controls, or if publication was timed to a short-term increase in demand, visits should already rise in the placebo week. We expect no significant effect in that week.

In addition, we examine whether visits change in the publication week and in the following weeks. A change that begins at the true publication date, and not before, is difficult to reconcile with factors that are already reflected in the matched pre-publication trajectory. Section 6 reports the estimates and Section 6.4 reports the placebo test.

Controlling for Selection in the Spillover-Effect Estimation

The spillover analysis raises a different selection concern. Although nearby restaurants are not reviewed themselves, critics may choose focal restaurants in neighborhoods where demand is increasing. An increase in visits to neighboring restaurants could therefore reflect a neighborhood-level demand increase rather than a spillover from the review.

We define the spillover zone as the area within 300 meters of a reviewed restaurant.[7] Treated restaurants are non-reviewed restaurants inside this zone. A restaurant inside the zone is treated in the week of the first review published within 300 meters. Potential controls are non-reviewed restaurants located more than 300 meters from every reviewed restaurant. In the *Both* specification, potential controls also include restaurants whose first nearby review is published later (Section 5.2). They are observed over the same calendar period. The direct-effect and spillover analyses use the same set of potential controls. SDID weights these controls to form the synthetic control. Figure I illustrates the spatial design.

Assumption 4 (Conditional Parallel Trends: Spillover Zone)**.** *Conditional on their matched pre-publication visit paths, non-reviewed restaurants inside the spillover zone and their con-*

[7]We use a 300-meter radius because prior work on consumer mobility and spatial agglomeration uses similar 250–300 meter thresholds to characterize nearby destinations and short-distance urban access (Kabra et al., 2020; Piovani et al., 2017; Miyauchi et al., 2025).

trols outside the spillover zone would have experienced similar changes in visits over the short post-publication window if the nearby review had not been published.

We provide evidence for Assumption 4 in the same way as for Assumption 3. We apply the placebo test described above to the nearby restaurants and their controls, using the date of the first nearby review.

A neighborhood shock coinciding with publication remains an alternative explanation. For example, a local event could raise visits to several restaurants near the focal establishment. We assess this explanation in Section 6 by examining the timing of the response and by estimating spillovers by distance from the reviewed restaurant. A response that begins at publication and weakens with distance from the reviewed restaurant is more consistent with the review than with a broader neighborhood-wide increase in demand.

5.2 Estimation

We estimate effects using the SDID estimator of Arkhangelsky et al. (2021), adapted to staggered treatment adoption. Let c index a cohort of restaurants with the same treatment week. In the direct-effect analysis, a cohort consists of restaurants whose reviews are published in the same week. In the spillover analysis, a cohort consists of nearby non-reviewed restaurants associated with focal reviews published in the same week. For simplicity, we refer to both as restaurants treated in the same week.

For each group c, SDID assigns weights to potential control restaurants and to the eight pre-publication weeks so that the weighted control outcome closely matches the treated restaurants before publication. See Online Appendix C.1 for more details.

The estimator requires at least two pre-publication weeks to construct the weights. Reviews published in the first weeks of our visit data do not meet this requirement, so 17 reviewed restaurants drop out of the direct-effect estimation. The direct-effect estimates therefore use 547 reviewed restaurants.

Let $\mathcal{I}_c$ denote the treated restaurants in group c, let n_c be the number of treated restau-

rants, and let t_c denote their treatment week. The treated-group outcome mean in calendar week t is

$$\bar{Y}_{c,t}^{\text{tr}} = \frac{1}{n_c} \sum_{i \in \mathcal{I}_c} Y_{i,t}. \tag{14}$$

For each group c, let $\mathcal{D}_c$ denote the pool of potential control restaurants. We report two control specifications. The *Never* specification uses restaurants that are never treated during the sample period. The *Both* specification also includes restaurants treated later, provided their treatment occurs after group c's post-publication window. The *Both* specification provides a larger control pool and can improve pre-publication fit, while the *Never* specification uses only restaurants that remain untreated throughout the sample.

Let $\hat{\omega}_{c,i}$ denote the SDID weight assigned to control restaurant i, and let $\hat{\lambda}_{c,\ell}$ denote the weight assigned to pre-publication week $\ell \in \{-8, \ldots, -1\}$. The weights within each set sum to one. The weighted pre-publication average outcome for the treated restaurants is

$$\bar{Y}_{c,\text{pre}}^{\text{tr}} = \sum_{\ell=-8}^{-1} \hat{\lambda}_{c,\ell} \bar{Y}_{c,t_c+\ell}^{\text{tr}}. \tag{15}$$

The corresponding weighted pre-publication average for the synthetic control is

$$\bar{Y}_{c,\text{pre}}^{\text{sc}} = \sum_{\ell=-8}^{-1} \hat{\lambda}_{c,\ell} \left(\sum_{i \in \mathcal{D}_c} \hat{\omega}_{c,i} Y_{i,t_c+\ell} \right). \tag{16}$$

At any calendar week t, the outcome for the synthetic control is

$$\bar{Y}_{c,t}^{\text{sc}} = \sum_{i \in \mathcal{D}_c} \hat{\omega}_{c,i} Y_{i,t}. \tag{17}$$

Let $k = t - t_c$ denote the week relative to treatment. Thus, $k = 0$ is the publication week in the direct-effect analysis, $k = -1$ is the preceding week, and $k = 1$ is the following week. The treatment effect for group c at relative week k is

$$\hat{\tau}_{c,k} = \left[\bar{Y}_{c,t_c+k}^{\text{tr}} - \bar{Y}_{c,\text{pre}}^{\text{tr}} \right] - \left[\bar{Y}_{c,t_c+k}^{\text{sc}} - \bar{Y}_{c,\text{pre}}^{\text{sc}} \right]. \tag{18}$$

Equation (18) compares the treated restaurants' change from their pre-publication average with the corresponding change for the synthetic control.

Let $\mathcal{C}_k$ denote the groups for which an estimate is available at relative week k. To obtain k-specific average treatment effects on the treated restaurants, we aggregate over the same k the group-specific estimates using the number of treated restaurants as weights:

$$\widehat{ATT}_k = \frac{\sum_{c \in \mathcal{C}_k} n_c \hat{\tau}_{c,k}}{\sum_{c \in \mathcal{C}_k} n_c}. \tag{19}$$

We display event-study estimates from eight weeks before through eight weeks after publication to show the dynamics of the effects. Our summary estimates average the effects over weeks 0 to +8, when estimates are less exposed to subsequent restaurant- or neighborhood-specific changes.

For the placebo test used to provide evidence for Assumptions 2 and 3, we replace each treatment week t_c with $t_c - 1$ and apply the same procedure. The weights $\hat{\omega}_{c,i}$ and $\hat{\lambda}_{c,\ell}$ are then estimated from the eight weeks before the placebo date, which are weeks -9 to -2 relative to the true publication week. The placebo effect is the estimate for the placebo publication week, aggregated across groups as in Equation (19). It does not use the true publication week or any week after it, and the weights do not use the placebo week.

The same staggered SDID procedure is used for the spillover analysis. Treatment occurs when a review is published within 300 meters of a non-reviewed restaurant. If a non-reviewed restaurant lies within 300 meters of multiple reviewed restaurants, we use the first nearby review as its single treatment date. Relative weeks for that restaurant are measured from that first publication date.

For demographic analyses, we construct separate restaurant-week outcomes for each origin-destination pair. For example, one outcome is visits from majority-W home CBGs to restaurants in majority-NW CBGs. The SDID unit and time weights are re-estimated for each subgroup-specific outcome.

Following Algorithm 2 in Arkhangelsky et al. (2021), we compute standard errors using 100 nonparametric cluster-bootstrap replications at the census tract level. Each replication samples census tracts with replacement, retains the complete weekly panels of all restaurants in each sampled tract, and re-estimates the full staggered SDID procedure. Clustering at the census tract level allows outcomes of nearby restaurants to be correlated, including restaurants that are treated by the same review. For comparisons between visitor groups at the same restaurants, we use the same resampled census tracts for both outcomes, preserving the covariance between the estimates when testing whether the two effects differ.

Finally, Online Appendix C.2 re-estimates the main direct and spillover effects using the two-stage difference-in-differences estimator of Gardner et al. (2024). This estimator includes restaurant and week fixed effects estimated from the untreated observations. In additional specifications, it also includes the lagged outcome and Yelp review controls, which are cumulative ratings, review volume, and their interaction. Section 6.4 reports this estimator, the placebo test, and longer pre-publication windows.

6 Results

We present the results in three parts. We first estimate the direct effect of a review on the reviewed restaurant and examine how the composition of its visitors changes after publication. We then estimate spillovers to nearby non-reviewed restaurants and similarly examine how visitor composition changes. Finally, we ask whether reviews draw consumers across demographic boundaries.

For each outcome, the event-study figures report weekly estimates from eight weeks before publication through eight weeks after publication. The tables summarize the average effect over the first eight post-publication weeks, weeks 0 to +8. We use this short window for the main estimates because it reduces the likelihood that the estimates reflect other changes occurring over the longer horizon.

6.1 Direct Effect

6.1.1 Direct Effect on Total Visits to the Reviewed Restaurant

Figure II plots the event-study estimates for the direct effect. The pre-publication estimates are close to zero by design: the SDID weights are chosen to match the treated restaurants' pre-publication outcome paths. Visits rise sharply in the publication week, from zero in the week before publication to 5.3% ($p = 0.002$) in the publication week. The effect peaks in the second week after publication at about 8%, remains elevated for approximately six weeks, and returns toward baseline by weeks seven and eight. Importantly, the increase begins when the review becomes public, rather than emerging gradually before publication. This timing is consistent with a causal effect of the review on restaurant visits. Section 6.4 reports a placebo test that is not fitted by construction: it moves the publication date one week earlier and does not use the placebo week to construct the weights, and finds no significant effect in the placebo week.

The first two columns of Table III report the average direct effect over the first eight post-publication weeks. Visits increase by 4.7% to 4.9% across the two control-group specifications, with $p < 0.001$ in both. Thus, publication produces a sizable and precisely estimated increase in visits to the reviewed restaurant, consistent with Empirical Prediction 1(a).

Restaurants used as controls in the direct-effect analysis may be located near reviewed restaurants. The spillover effect of these reviews could increase visits to the nearby control restaurants and bias the estimated direct effect downward. For this reason, the control group includes only non-reviewed restaurants located more than 300 meters from every reviewed restaurant (Section 5.1).

6.1.2 Geographic Composition of Restaurant Demand

We examine whether the aggregate effect reflects a broadening of the restaurant's geographic market. We consider three measures: visits from local versus non-local consumers, the

restaurant's weekly trade area, and visitors' recent exposure to the restaurant's census tract.

Local and non-local visitors. We first define a restaurant's *trade area* as the 60th percentile of the distribution of distances between the restaurant and its visitors' home CBGs, following the retail trade-area literature (Reilly, 1953; Aversa et al., 2024). We use a restaurant-specific trade area because restaurants draw visitors from different distances; the same distance may fall within the typical market of one restaurant but outside that of another. For each reviewed restaurant, we calculate its trade area once using all pre-publication visits and use this fixed trade area to classify visitors as local or non-local.[8] A visitor whose home CBG lies within the trade area is classified as *local*, while a visitor whose home CBG lies outside the trade area is classified as *non-local*. The median pre-publication trade-area radius is 18.76 km[9].

Panel (a) of Figure VIII shows positive direct effects for both local and non-local consumers. Table VII reports a 4.2% increase among non-local consumers ($p < 0.001$), compared with 2.1% among local consumers ($p = 0.12$). The difference of 2.1 percentage points is marginally significant ($p = 0.09$). Thus, the increase in visits comes from consumers who live both outside and inside the restaurant's trade area, although the estimate for local consumers is not statistically significant. The larger point estimate among non-local consumers is in the direction of part (c) of Empirical Prediction 1, which predicts a larger proportional response among non-local consumers when they face sufficiently high costs of visiting the neighborhood. The difference, however, is not precisely estimated.

[8]For non-reviewed restaurants, we use all observations throughout the analysis period to calculate their trade area.

[9]Tonin et al. (2026) find that the average trade area of fast food restaurants is about 13 km across major US cities, suggesting a lower willingness to travel compared to our sample of full-service, independent establishments. This is consistent with Agarwal et al. (2017), who show that consumer expenditure declines more rapidly with distance in sectors with more frequent transactions. Because fast food is typically purchased more frequently than full-service restaurant meals, this pattern suggests that consumers may travel shorter distances for fast food.

Trade-area expansion. We next examine whether the review expands the geographic reach of the restaurant. To do so, we calculate the trade area separately for each restaurant-week, rather than using the fixed pre-publication trade area used above to classify visitors as local or non-local. Panel (a) of Figure IX shows a positive but noisy response of the trade area after publication. Table VIII reports an average increase of 4.8%, which is not statistically significant ($p = 0.10$). The point estimate is in the direction of part (d) of Empirical Prediction 1, which predicts an increase in the share of visits from non-local consumers, but we cannot rule out that the trade area of the reviewed restaurant is unchanged.

Recent area exposure. Finally, we examine whether the direct review effect varies with consumers' pre-review likelihood of considering the neighborhood as a dining destination. As described in Section 3.3, we proxy for neighborhood consideration using consumers' recent physical exposure to the restaurant's census tract, which provides a measure of prior familiarity with the neighborhood.

We classify consumers based on whether they had recently been observed in the restaurant's census tract. A consumer is *recently exposed* if the device was observed in the tract at least once during the preceding 30 days, and *not recently exposed* otherwise. In the figures and tables, we refer to these two groups as *familiar* and *unfamiliar*, respectively.[10]

Panel (a) of Figure VII shows a larger direct effect among consumers who had not been recently exposed to the neighborhood. Table VI reports an eight-week effect of 2.9% for consumers who had not been recently exposed ($p = 0.005$), compared with 0.9% for consumers who had been recently exposed ($p = 0.27$), a statistically significant difference of 2.0 percentage points ($p = 0.035$). This finding is consistent with part (b) of Empirical Prediction 1.

In summary, the direct effect increases visits from both local and non-local consumers.

[10]For computational and data requirement reasons, the analysis is restricted the following geographies: Los Angeles County, the five New York City counties (boroughs), San Francisco County, King County, Cook County, Suffolk County, and Philadelphia County. The familiarity analysis therefore covers 337 of the 564 reviewed restaurants.

The response is larger among consumers with no recent exposure to the area, and this difference is statistically significant. The larger response among non-local consumers is only marginally significant, and the expansion of the trade area is imprecisely estimated. The review therefore broadens the composition of demand at the reviewed restaurant to a limited extent. The clearer evidence of a broader market appears in the spillover results below.

6.2 Local Demand Spillovers

6.2.1 Spillover Effect on Total Visits to Nearby Non-Reviewed Restaurants

We next examine the effect of a review on non-reviewed restaurants within 300 meters of the reviewed restaurant. Figure III plots the spillover event study. The pre-publication estimates are close to zero by design. The spillover becomes positive in the publication week, and the estimates remain positive throughout the eight post-publication weeks, although several weekly estimates are imprecise. Columns (3) and (4) of Table III report an average increase in visits of 1.9% to 2.0% over the first eight post-publication weeks, with $p < 0.01$ under both control-group specifications. The positive spillover is consistent with the neighborhood discovery effect outweighing the substitution effect, as characterized in Proposition 3: the review increases consideration of the neighborhood, increasing visits to nearby restaurants, while substitution toward the reviewed restaurant reduces them. The placebo test in Section 6.4 finds no significant spillover in the week before publication.

Spillover as a function of distance. Figure IV presents separate estimates of the spillover effect for four samples of restaurants, defined by their distance from the reviewed restaurant. The four distance bins span 0–300 meters and contain approximately equal numbers of restaurants. The estimated effects are 1.9% at 0–99 meters ($p = 0.07$), 2.8% at 99–170 meters ($p = 0.003$), and 1.9% at 170–235 meters ($p = 0.051$). In the 235–300 meters bin, the estimated effect is 1.1% and statistically indistinguishable from zero ($p = 0.28$). The spillover is therefore positive in all four bins, and it is weaker and not statistically significant

at the outer edge of the zone (Online Appendix Table D.3 reports the estimates).

The weaker response at the outer edge of the zone is consistent with a spillover that is tied to the reviewed restaurant. However, the estimates do not decline monotonically with distance, and the confidence intervals of the four bins overlap. The distance pattern therefore provides only limited evidence against a contemporaneous neighborhood shock. The evidence on timing is more informative: the response begins in the publication week, as in the direct effect, and the placebo test finds no significant effect in the week before publication.

6.2.2 Who Generates the Spillover?

Local and non-local visitors. We first compare visitors who reside inside and outside the nearby restaurant's trade area. Panel (b) of Figure VIII shows the difference between the two groups. Table VII reports a 2.6% spillover among non-local visitors ($p = 0.014$), compared with -0.1% among local visitors, with the latter statistically indistinguishable from zero ($p = 0.89$). The difference between the two groups is 2.6 percentage points ($p = 0.005$).[11] The positive spillover among non-local visitors is consistent with part (c) of Empirical Prediction 2, which predicts a positive spillover when non-local consumers face sufficiently high visit costs. The spillover among local visitors is close to zero, rather than negative as predicted in part (b) for the case in which local consumers consider the neighborhood with probability one.

The near-zero local estimate does not imply that local consumers are unaffected by the review. Rather, it may reflect offsetting substitution and neighborhood discovery effects, resulting in little net change in visits to nearby establishments. In contrast, visits to nearby restaurants increase among non-local consumers who reside beyond those restaurants' trade areas. This pattern is consistent with neighborhood discovery: the review increases consideration of the neighborhood among non-local consumers, who may subsequently discover

[11] For non-reviewed restaurants in the spillover analysis, the local-versus-non-local cutoff is calculated from the distribution of distances between the restaurant and its visitors' home CBGs over the analysis period.

and visit nearby establishments.

Trade-area expansion. As in the direct-effect analysis, we next calculate the trade area separately for each restaurant-week and use it as the outcome. Panel (b) of Figure IX shows that the trade area of nearby non-reviewed restaurants expands after publication. Table VIII reports an average increase of 5.0% ($p = 0.038$). Applied to the median trade-area radius of 18.76 km, this corresponds to approximately one additional kilometer. The expansion grows over the eight post-publication weeks. Thus, the review increases both visits to nearby restaurants and their geographic reach, consistent with the increase in the share of visits from non-local consumers predicted in part (d) of Empirical Prediction 2.

Recent area exposure. Finally, we examine whether the spillover effect varies with consumers' recent exposure to the area. Panel (b) of Figure VII shows positive spillovers for both groups, with a larger effect among consumers who were not recently exposed. Table VI reports effects of 2.3% for consumers who were not recently exposed ($p = 0.001$) and 0.6% for those who were recently exposed ($p = 0.05$), a difference of 1.6 percentage points ($p = 0.015$). This finding is consistent with part (a) of Empirical Prediction 2, which predicts a larger proportional spillover among consumers who are initially less likely to consider the neighborhood as a dining destination.

The exposure measure captures recent exposure to the area at the time of each visit but does not identify whether the review induced a consumer's initial exposure to the area. Because our mobility data are aggregated for privacy reasons, we cannot link visits by the same consumer across the reviewed and nearby restaurants. For example, a consumer may first visit the area because of the reviewed restaurant, become exposed to a neighboring restaurant, and subsequently visit that restaurant. This consumer will be classified as recently exposed when visiting the neighboring restaurant, even though the review induced the consumer's initial exposure to the area. Thus, the recently exposed group may include consumers whose exposure to the area was itself induced by the review.

6.3 Do Reviews Draw Consumers Across Demographic Boundaries?

Sections 6.1.2 and 6.2.2 show that the effects of reviews vary with consumers' geographic distance from the restaurant and their prior exposure to the area. We now ask whether reviews also draw consumers across demographic boundaries.

As described in Section 4, we classify each CBG as majority-W when its non-Hispanic White population share exceeds 50% and as majority-NW otherwise. A visit is cross-demographic when the visitor's home CBG and the restaurant's CBG are of different types, and same-group when they are of the same type. Restaurant visits are demographically sorted as discussed in Section 4.3: same-group visits account for 68% of visits to restaurants in majority-W CBGs and 58% of visits to restaurants in majority-NW CBGs.

We estimate the direct and spillover effects separately for each of the four combinations of home and restaurant CBG type. For each combination, the treated restaurants and their potential controls are located in CBGs of the same type, and the SDID weights are re-estimated for the origin-specific outcome. To compare demographically distant and close visitors, we compare the two flows to the same destination type, which are measured at the same reviewed restaurants.[12] Since we classify visits by the composition of home and restaurant CBGs and do not observe the race of individual visitors, we interpret the estimates as suggestive evidence on movement between neighborhoods with different demographic composition.

6.3.1 Direct Effects

Table IV reports the direct effects averaged over the first eight post-publication weeks, and Figure V plots the event studies. Reviews increase visits from majority-W home CBGs to reviewed restaurants in majority-NW CBGs by 7.4% ($p = 0.002$). The increase begins in

[12] We do not compare flows to different destination types because they involve different reviewed restaurants, and a difference could reflect which restaurants critics choose to review in each type of neighborhood.

the publication week (Figure V, panel (b)). Visits in the opposite direction, from majority-NW home CBGs to reviewed restaurants in majority-W CBGs, increase by 2.1%, but the estimate is imprecise ($p = 0.104$). Same-group visits also increase. Visits from majority-NW home CBGs to reviewed restaurants in majority-NW CBGs rise by 4.7% ($p = 0.018$), and visits from majority-W home CBGs to reviewed restaurants in majority-W CBGs rise by 2.6% ($p = 0.096$). These results are consistent with part (a) of Empirical Prediction 3 at majority-NW destinations, where visits from both origin types increase significantly. At majority-W destinations, both estimates are positive but only marginally significant.

Within each destination type, the differences between demographically distant and close visitors are imprecisely estimated and differ in sign. At reviewed restaurants in majority-NW CBGs, the increase is 2.6 percentage points larger for visits from majority-W home CBGs than for visits from majority-NW home CBGs ($p = 0.240$). At reviewed restaurants in majority-W CBGs, the increase is 0.4 percentage points smaller for visits from majority-NW home CBGs than for visits from majority-W home CBGs ($p = 0.807$). The direct effects therefore do not support part (b) of Empirical Prediction 3, which predicts a larger proportional increase among demographically distant consumers.

Together, these results show that reviews encourage consumers to cross demographic boundaries, most clearly from majority-W home CBGs to restaurants in majority-NW CBGs. This result contrasts with Aneja et al. (2025). They find that labeling restaurants as Black-owned increases visits mainly from White customers and mainly at restaurants in majority-White neighborhoods, but they find no evidence that the information leads White consumers to visit demographically different neighborhoods. The information differs across the two settings: their label concerns the owner's race, whereas a professional review concerns the restaurant's quality.

6.3.2 Spillover Effects

Table V reports the spillover effects for nearby non-reviewed restaurants, and Figure VI plots the event studies. Visits from majority-W home CBGs to nearby restaurants in majority-NW CBGs increase by 4.0% ($p < 0.001$), and the weekly estimates remain positive through the eighth post-publication week (Figure VI, panel (b)). Visits from majority-NW home CBGs to nearby restaurants in majority-W CBGs increase by 0.8%, but the estimate is imprecise ($p = 0.217$). Same-group spillovers are also positive: 1.9% for visits from majority-NW home CBGs to nearby restaurants in majority-NW CBGs ($p = 0.023$), and 1.6% for visits from majority-W home CBGs to nearby restaurants in majority-W CBGs ($p = 0.087$).

Within destination types, the spillover is 2.1 percentage points larger for demographically distant visitors at nearby restaurants in majority-NW CBGs ($p = 0.046$), consistent with part (c) of Empirical Prediction 3. At nearby restaurants in majority-W CBGs, the spillover is 0.9 percentage points smaller for demographically distant visitors, and the difference is not statistically significant ($p = 0.183$).

Part (d) of Empirical Prediction 3 predicts a positive spillover among demographically distant consumers. The estimate supports this prediction for visits from majority-W home CBGs. For visits from majority-NW home CBGs, the estimate is positive but not statistically significant.

In summary, reviews draw consumers across demographic boundaries. Visits from majority-W home CBGs to restaurants in majority-NW CBGs increase at both the reviewed restaurant and nearby restaurants. Visits in the opposite direction also increase, but these estimates are imprecise. At a given destination, demographically distant visitors do not respond consistently more than close visitors: the only statistically significant difference is the larger spillover among visitors from majority-W home CBGs at nearby restaurants in majority-NW CBGs. These differences do not isolate the effect of demographic distance: because of residential segregation, demographically distant visitors also tend to live farther from the restaurant, and visitors from majority-W and majority-NW CBGs may differ in other ways,

such as their exposure to newspaper reviews.

6.4 Robustness Checks

Placebo test of publication timing. We assess whether outcomes change before the review becomes public. We estimate the SDID weights using weeks -9 to -2 relative to the true publication date, so the weights do not use the placebo week. Unlike the pre-publication event-study estimates, the placebo estimate is therefore not fitted by construction. Online Appendix Table D.2 reports the results. The placebo estimate for the reviewed restaurant is 2.3% and is not statistically significant ($p = 0.27$). The effect in the actual publication week, estimated on the same week, is 5.3% ($p = 0.002$). For nearby non-reviewed restaurants, the placebo estimate is 0.9% ($p = 0.29$), compared with 1.9% ($p = 0.03$) in the actual publication week. Thus, we find no significant change in the week before publication, and we find a discrete increase when the review becomes public. This pattern supports the timing and conditional parallel trends assumptions, i.e., Assumptions 2, 3 and 4.

Longer pre-treatment windows. We test whether the results depend on the length of the pre-treatment window used to construct the synthetic control weights. Online Appendix Table D.1 extends this window from 8 to 12 weeks, in one-week increments. An extended pre-treatment window can improve match quality (Arkhangelsky et al., 2021). The direct effect ranges from 4.7% to 5.5%, close to our baseline estimate of 4.7%. The spillover effect ranges from 1.85% to 1.95%, close to our baseline estimate of 1.9%. Both effects stay statistically significant at the 1% level across all five window lengths. These results show that our findings do not depend on the specific choice of pre-treatment window.

Two-stage difference-in-differences estimator. As a robustness check, we re-estimate the main direct and spillover effects using the two-stage difference-in-differences estimator of Gardner et al. (2024). Unlike the SDID estimator, in this approach we do not rely on matching or cohort-level weighting. Instead, we include all treated and control units

and control for restaurant and week fixed effects. By using only untreated observations to estimate these fixed effects, the model addresses the concerns associated with conventional two-way fixed effects under staggered treatment. The estimator also allows us to incorporate additional control variables. Following Reimers and Waldfogel (2021), who study the effect of professional book reviews on Amazon sales rank, we include the lagged outcome and Yelp review controls capturing cumulative ratings and review volume.

We provide details on the estimation procedure in Online Appendix Section C.2. Online Appendix Table C.2 shows that the estimated direct and spillover effects remain positive and statistically significant across specifications with and without these controls. The specification with the lagged outcome yields a direct estimate of 5.0% and a spillover estimate of 1.4%, close to our main SDID estimates of 4.7% and 1.9%. The main two-stage specification, which adds the Yelp controls, yields 5.6% for the direct effect ($p < 0.001$) and 1.3% for the spillover effect ($p = 0.018$). The direct estimates are larger when we do not control for the lagged outcome (7.7% and 8.7%). This difference is consistent with the growth in visits of reviewed restaurants before publication shown in Online Appendix Figure D.1. Overall, these results indicate that our findings are robust to an alternative difference-in-differences estimator and to controlling for lagged outcomes and Yelp review information.

7 Discussion

Our results indicate that a food critic's review generates demand not only for the reviewed restaurant but also for the area around it. We interpret this pattern as neighborhood discovery: the review makes the neighborhood more salient as a dining destination and brings in consumers who would not otherwise consider it. Three findings support this interpretation. First, the spillover is concentrated among consumers who live outside nearby restaurants' trade areas and among consumers who had not recently been in the area. Second, the trade areas of nearby restaurants expand by about 5%, or roughly one kilometer at the median

(Section 6.2.2). Third, the direct effect is larger among consumers without recent exposure to the area (Section 6.1.2). As the model predicts, the review has the largest effect on consumers who are least likely to visit the neighborhood absent the review.

Three alternative explanations could also produce positive spillovers. The first is overflow: diners who cannot get a table at a newly reviewed restaurant may visit a nearby restaurant instead. Overflow should be concentrated in the weeks when the reviewed restaurant is busiest, but the spillover remains positive through the eighth week, after the direct effect has returned toward baseline. The second is that reviews coincide with other changes in the neighborhood that independently raise visits. However, the spillover begins in the publication week, and the placebo test finds no significant spillover in the week before publication (Section 6.4). The third is that a review raises consumers' beliefs about the quality of the area as a whole (Glaeser et al., 2001). We cannot separate this mechanism from the review increasing the probability that the neighborhood will be considered for dining by the consumers. We therefore view our results as evidence that information generates demand for neighborhoods, without distinguishing whether it operates through consideration of the area or beliefs about it.

Neighborhood discovery may also extend beyond the initial visit. Dean et al. (2026) show that even brief exposure to an unfamiliar location can increase individuals' willingness to return. Consistent with this mechanism, the trade areas of nearby restaurants continue to expand over the eight post-publication weeks, and the spillover remains positive after the direct effect fades. Because our data do not follow consumers across establishments, we cannot confirm that the same consumers return, and we view this interpretation as suggestive.

One prediction of the model is not borne out. Part (b) of Empirical Prediction 2 predicts a negative spillover among local consumers who consider the neighborhood with certainty, because the reviewed restaurant draws them away from its neighbors. We instead find a local spillover close to zero. This estimate suggests either that local consumers do not consider

the neighborhood with certainty, so that discovery offsets substitution, or that substitution toward the reviewed restaurant is small. In either case, nearby restaurants do not appear to lose local customers when a neighbor is reviewed.

The spillover is smaller than the direct effect in percentage terms, but it accrues to many more restaurants. The 547 reviewed restaurants in the two-stage sample have 6,454 non-reviewed restaurants within 300 meters, or about 12 per reviewed restaurant (Online Appendix Section C.2). At pre-publication means (Table II), the direct effect of 4.7% corresponds to about 0.8 additional weekly visits to the reviewed restaurant. The spillover of 1.9% corresponds to about 0.5 additional weekly visits to each nearby restaurant, or about 5.6 per reviewed restaurant. The total increase in visits to nearby restaurants is therefore nearly seven times the increase at the reviewed restaurant, or about four times using the two-stage estimates. Because our data cover only a sample of devices, the additional visits in levels are larger than these figures, but the ratio between nearby and reviewed restaurants is unaffected as long as the sampling rate is similar across restaurants. These calculations are approximate: they apply proportional effects to sample means, and the mean for nearby restaurants is computed over all non-reviewed restaurants in census tracts with a reviewed restaurant.

These magnitudes imply that most of the additional visits a review brings to an area go to restaurants that were not reviewed. The value of a review to the neighborhood therefore exceeds its value to the reviewed restaurant, consistent with demand externalities among nearby restaurants (Leonardi and Moretti, 2023). Neighboring businesses, and the organizations that represent them, share an interest in attracting critics' attention that no single restaurant fully internalizes. The magnitudes also suggest that the decline of local newspapers (Abernathy, 2018) has costs beyond the restaurants that would have been reviewed: it removes a source of information that generates demand for neighborhoods, not only for individual businesses.

The demographic results show that reviews also draw consumers across demographic

boundaries. Visits from majority-W neighborhoods rise at reviewed and nearby restaurants in majority-NW neighborhoods. The model predicts that demographically distant visitors respond more than close visitors to the same restaurant; we find this pattern only for spillovers at restaurants in majority-NW neighborhoods. Because we classify visits by neighborhood composition rather than by the race of individual visitors, we view these results as suggestive.

Our findings should be interpreted in light of the setting and data. Professional reviews provide relatively salient and credible information about experience goods, and other forms of information may generate different responses. Our sample covers reviews in the largest-circulation newspaper with an active food critic in each of 11 metropolitan areas, published between January 2019 and mid-March 2020, before the COVID-19 pandemic changed dining and urban mobility. The estimates are effects for restaurants that critics chose to review, which may differ from the effect of reviewing a randomly chosen restaurant. Our mobility data come from devices whose users opted in to location sharing, and home locations are inferred from nighttime location patterns. The data measure realized visits rather than consumers' information or consideration sets, so we cannot separately identify changes in neighborhood consideration, restaurant awareness, or beliefs about quality. Finally, our estimates compare restaurants near a reviewed restaurant with restaurants more than 300 meters from any reviewed restaurant, so they measure how much a review raises visits in the reviewed area relative to the rest of the city. If some of the additional visits replace visits to restaurants elsewhere in the city, for example because consumers dine in the reviewed area instead of at their usual restaurant, the net effect on restaurant visits citywide is smaller than our estimates.

8 Conclusion

Can information change the geographic boundaries of an urban market? We study this question using professional restaurant reviews and smartphone mobility data. We find that

reviews increase visits to the reviewed restaurant while also changing where its visitors come from. Visits increase among consumers who live inside and outside the restaurant's usual trade area, with a larger point estimate for non-local consumers, and the increase is larger among consumers with limited recent exposure to the area. Information about a restaurant can therefore broaden the set of consumers who visit it.

The effects extend beyond the reviewed restaurant. Nearby non-reviewed restaurants also gain visits, particularly from non-local consumers, and their trade areas expand. These findings show that information about one business can affect demand for other businesses nearby. Reviews also move consumers across demographic boundaries. Majority-W-origin consumers increase visits to reviewed restaurants in majority-NW neighborhoods and to nearby non-reviewed restaurants there. The estimates for majority-NW-origin consumers visiting majority-W neighborhoods are positive but small and not statistically significant.

Taken together, our findings show that information can change not only which businesses consumers choose, but also where they go within a city. By drawing consumers beyond the places they typically visit, information can expose them to new areas and generate benefits for nearby businesses. Geographic and demographic frictions, however, continue to shape these responses. The boundaries of local markets are therefore neither fixed by geography nor eliminated by information; they reflect the interaction between the two.

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Table I: Number of Reviews by Newspaper

Metropolitan Area	Newspaper Name	Num. Reviews
New York, NY	*New York Times*	104
Los Angeles, CA	*Los Angeles Times*	99
Washington, DC	*The Washington Post*	94
Philadelphia, PA	*Philadelphia Inquirer*	53
Phoenix, AZ	*Arizona Republic*	45
Boston, MA	*Boston Globe*	40
San Francisco, CA	*San Francisco Chronicle*	35
Seattle, WA	*Seattle Times*	31
Chicago, IL	*Chicago Tribune*	26
Austin, TX	*Austin American-Statesman*	22
Nashville, TN	*Tennessean*	15
Total		564

Note. Each newspaper is the largest by circulation in the major city of the corresponding metropolitan area. Metropolitan areas may contain multiple cities; for example, the Boston metropolitan area includes the City of Boston, MA, Cambridge, MA, and so on. The vast majority of reviews in each newspaper cover restaurants within the corresponding metropolitan area and its major city.

Table II: Descriptive Statistics

	Reviewed Restaurants	Non-reviewed Restaurants
Panel A: Sample		
Restaurants	564	10,415
Restaurant-week observations	34,968	645,730
Panel B: Weekly visits (pre-publication)		
Mean	17.9	25.3
SD	28.1	47.1
Panel C: Restaurant neighborhood		
Share in majority-non-White neighborhood	0.34	0.31
Panel D: Consumer characteristics (pre-publication)		
Share of visits from unfamiliar consumers	0.76	0.74
Share of visits from non-local consumers	0.40	0.42
	Maj-White visitors	Maj-non-White visitors
Panel E: Share of visits by visitor and restaurant neighborhood (all restaurants combined, pre-publication)		
Majority-White restaurants	0.68	0.32
Majority-non-White restaurants	0.42	0.58

Note. Reviewed restaurants are restaurants reviewed by critics. Non-reviewed restaurants are all other restaurants in the census tracts that contain at least one reviewed restaurant. Panel A reports the full panel. Panels B–D report statistics computed over the pre-publication period: the eight weeks before the review's publication date for reviewed restaurants, and the full observation window for non-reviewed restaurants. Panels D and E are computed as equal-weighted averages across restaurants: shares are first aggregated within each restaurant across weeks, then averaged across restaurants. Panel E combines all restaurants in the sample. Each row sums to one. Restaurant neighborhoods are classified as majority-White if the non-Hispanic White share of the census block group exceeds 50%. Visitors are classified by the racial composition of their home census block group. A consumer is unfamiliar if they did not visit the restaurant's census tract in the 30 days before their visit. A consumer is non-local if they live outside the restaurant's trade area, defined as the 60th percentile of the distribution of home-to-restaurant distances.

Table III: Direct and Spillover Effects of Critics Reviews on Restaurant Visits, Synthetic Difference-in-Differences

	Outcome: $\ln(\text{Visits}+1)$			
	Direct Effect		Spillover Effect	
Control group:	Never (1)	Both (2)	Never (3)	Both (4)
ATT	0.0494	0.0467	0.0204	0.0186
Std. Err.	(0.0127)	(0.0126)	(0.0068)	(0.0069)
P-value	(< 0.001)	(< 0.001)	(0.003)	(0.007)
Sample statistics				
Avg. treated units	9.1	9.1	107.6	107.6
Avg. control units	3750.0	3944.7	3750.0	5585.0
Avg. pre-periods	7.7	7.7	7.7	7.7
Avg. post-periods	8.4	8.4	8.4	8.4
Number of cohorts	60	60	60	60
Avg. eff. controls	1707.9	1829.2	3069.7	4620.7
Bootstrap clusters	426	426	420	420

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Never*: restaurants never treated during the sample period; *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.

Table IV: Direct Effects by Visitor Origin and Restaurant Location, SDID

	Outcome: ln(Visits + 1)	
	Restaurants in majority-W CBGs (1)	Restaurants in majority-NW CBGs (2)
(i) Visits from majority-W CBGs		
ATT	0.0256	0.0736
Std. Err.	(0.0154)	(0.0238)
P-value	(0.096)	(0.002)
(ii) Visits from majority-NW CBGs		
ATT	0.0214	0.0471
Std. Err.	(0.0132)	(0.0199)
P-value	(0.104)	(0.018)
Difference in ATTs, (i) − (ii)		
Difference	0.0042	0.0264
Std. Err.	(0.0172)	(0.0225)
P-value	(0.807)	(0.240)
Sample statistics		
Avg. treated units	6.0	3.1
Avg. control units	2792.8	1149.4
Avg. pre-periods	7.7	7.7
Avg. post-periods	8.4	8.4
Number of cohorts	60	60
Avg. eff. controls	1149.0	297.2
Bootstrap clusters	300	202

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). Visits from majority-W (majority-NW) CBGs are visits from devices whose home census block group has a non-Hispanic White share above (at or below) 0.5. This is inferred from the home block group and is not observed race. A restaurant is in a majority-W CBG if the non-Hispanic White share of its own block group is above 0.5; treated and control restaurants are of the same type. Columns (1) and (2) report effects for restaurants in majority-W and majority-NW CBGs, respectively. The difference row compares visits from majority-W and majority-NW CBGs at the same restaurants; its standard error uses a joint bootstrap (the same resampled census tracts in every draw). Sample statistics are those of visits from majority-W CBGs (i); the restaurants are the same in (ii). Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.

Table V: Spillover Effects by Visitor Origin and Restaurant Location, SDID

	Outcome: ln(Visits + 1)	
	Nearby restaurants in majority-W CBGs (1)	Nearby restaurants in majority-NW CBGs (2)
(i) Visits from majority-W CBGs		
ATT	0.0165	0.0400
Std. Err.	(0.0096)	(0.0116)
P-value	(0.087)	(<0.001)
(ii) Visits from majority-NW CBGs		
ATT	0.0076	0.0194
Std. Err.	(0.0062)	(0.0085)
P-value	(0.217)	(0.023)
Difference in ATTs, (i) − (ii)		
Difference	0.0089	0.0205
Std. Err.	(0.0067)	(0.0103)
P-value	(0.183)	(0.046)
Sample statistics		
Avg. treated units	72.4	35.7
Avg. control units	3884.9	1706.4
Avg. pre-periods	7.7	7.6
Avg. post-periods	8.4	8.4
Number of cohorts	60	59
Avg. eff. controls	3011.5	1035.8
Bootstrap clusters	301	207

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). Visits from majority-W (majority-NW) CBGs are visits from devices whose home census block group has a non-Hispanic White share above (at or below) 0.5. This is inferred from the home block group and is not observed race. A restaurant is in a majority-W CBG if the non-Hispanic White share of its own block group is above 0.5; treated and control restaurants are of the same type. Columns (1) and (2) report effects for nearby non-reviewed restaurants in majority-W and majority-NW CBGs, respectively. The difference row compares visits from majority-W and majority-NW CBGs at the same restaurants; its standard error uses a joint bootstrap (the same resampled census tracts in every draw). Sample statistics are those of visits from majority-W CBGs (i); the restaurants are the same in (ii). Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.

Table VI: Direct and Spillover Effects by Recent Exposure to the Area, SDID

	Outcome: ln(Visits + 1)		
	Familiar (recently exposed)	Unfamiliar (not recently exposed)	Diff.
	(1)	(2)	(3)
Panel A: Direct effect			
ATT	0.0089	0.0293	-0.0204
Std. Err.	(0.0080)	(0.0104)	(0.0097)
P-value	(0.266)	(0.005)	(0.035)
Sample statistics			
Avg. treated units	9.1	9.1	
Avg. control units	3944.7	3944.7	
Avg. pre-periods	7.7	7.7	
Avg. post-periods	8.4	8.4	
Number of cohorts	60	60	
Avg. eff. controls	1894.6	2009.3	
Bootstrap clusters	426	426	
Panel B: Spillover effect			
ATT	0.0063	0.0225	-0.0162
Std. Err.	(0.0032)	(0.0068)	(0.0066)
P-value	(0.050)	(< 0.001)	(0.015)
Sample statistics			
Avg. treated units	107.6	107.6	
Avg. control units	5585.0	5585.0	
Avg. pre-periods	7.7	7.7	
Avg. post-periods	8.4	8.4	
Number of cohorts	60	60	
Avg. eff. controls	4402.9	4412.7	
Bootstrap clusters	420	420	

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). A visitor is familiar if the device was observed in the restaurant's census tract during the previous 30 days. Column (3) is the difference between columns (1) and (2). Both outcomes come from the same restaurants, so its standard error uses a joint bootstrap (the same resampled census tracts in every draw). Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.

Table VII: Direct and Spillover Effects on Local and Non-Local Visits, SDID

	Outcome: $\ln(\text{Visits}+1)$		
	Local visits (1)	Non-local visits (2)	Diff. (3)
Panel A: Direct effect			
ATT	0.0211	0.0423	-0.0212
Std. Err.	(0.0137)	(0.0107)	(0.0127)
P-value	(0.124)	(< 0.001)	(0.093)
Sample statistics			
Avg. treated units	9.1	9.1	
Avg. control units	3944.7	3944.7	
Avg. pre-periods	7.7	7.7	
Avg. post-periods	8.4	8.4	
Number of cohorts	60	60	
Avg. eff. controls	1785.2	2030.9	
Bootstrap clusters	426	426	
Panel B: Spillover effect			
ATT	-0.0007	0.0256	-0.0263
Std. Err.	(0.0051)	(0.0104)	(0.0094)
P-value	(0.890)	(0.014)	(0.005)
Sample statistics			
Avg. treated units	107.6	107.6	
Avg. control units	5585.0	5585.0	
Avg. pre-periods	7.7	7.7	
Avg. post-periods	8.4	8.4	
Number of cohorts	60	60	
Avg. eff. controls	4682.8	4603.2	
Bootstrap clusters	420	420	

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). Local (non-local) visitors live inside (outside) the restaurant's fixed 60th-percentile trade area. Column (3) is the difference between columns (1) and (2). Both outcomes come from the same restaurants, so its standard error uses a joint bootstrap (the same resampled census tracts in every draw). Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.

Table VIII: Direct and Spillover Effects on the Restaurant's Trade Area, SDID

	Outcome: ln(Trade area + 1)	
	Direct Effect (1)	Spillover Effect (2)
ATT	0.0484	0.0503
Std. Err.	(0.0296)	(0.0243)
P-value	(0.102)	(0.038)
Sample statistics		
Avg. treated units	9.1	107.6
Avg. control units	3944.7	5585.0
Avg. pre-periods	7.7	7.7
Avg. post-periods	8.4	8.4
Number of cohorts	60	60
Avg. eff. controls	1847.9	4619.5
Bootstrap clusters	426	420

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). The trade area is the 60th percentile of the distance between the restaurant and its visitors' home block groups, calculated for each restaurant-week. Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.

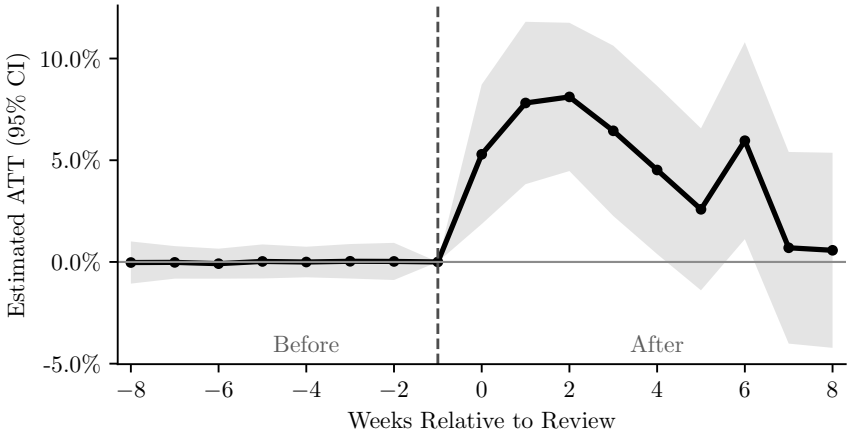
Figure I: Illustration of Spillover Zone



Note. The figure illustrates the spatial design of the spillover analysis. The example shows Nemi, reviewed by Craig LaBan of the *Philadelphia Inquirer* on May 24, 2019.[a]

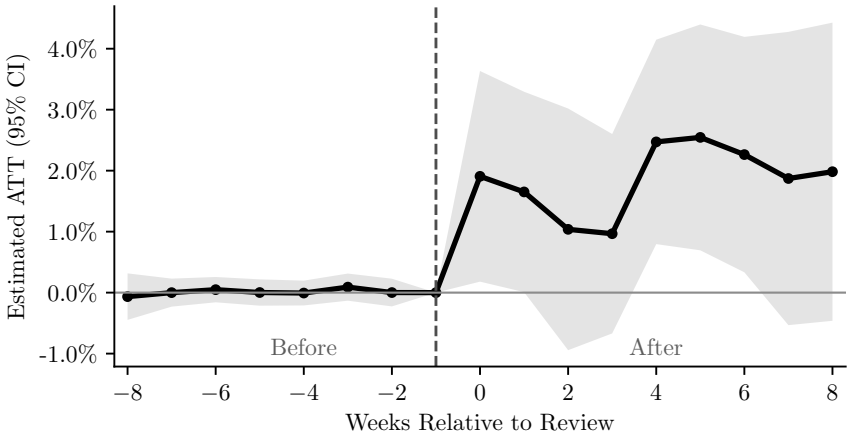
[a] https://www.inquirer.com/food/craig-laban/nemi-review-philadelphia-port-richmond-20190524.html

Figure II: Reviews and Visits to the Reviewed Restaurant Over Time



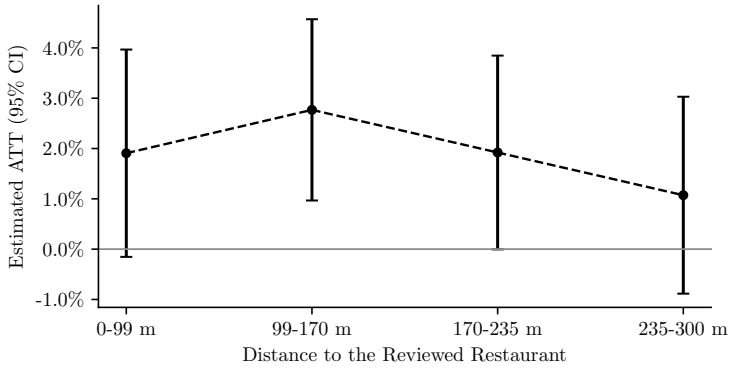
Note. Weekly ATT estimates from staggered SDID (Equation 19) with 95% confidence intervals. Outcome: $\ln(\text{Visits} + 1)$. Review published at $t = 0$; reference period $t = -1$. Control group: *Both* (never-treated and not-yet-treated restaurants). Standard errors from 100 bootstrap replications, clustered at the census tract level.

Figure III: Reviews and Visits to Nearby Non-Reviewed Restaurants Over Time



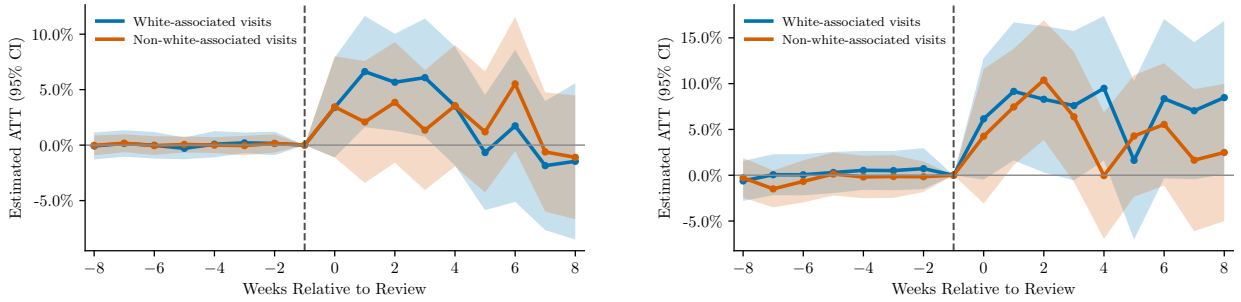
Note. Weekly SDID ATT estimates (Equation 19) with 95% confidence intervals for non-reviewed restaurants within 300 meters. Outcome: $\ln(\text{Visits} + 1)$. Review published at $t = 0$; reference period $t = -1$. Control group: *Both* (never-treated and not-yet-treated restaurants). Standard errors from 100 bootstrap replications, clustered at the census tract level.

Figure IV: Spillover Effect as a Function of Distance from the Reviewed Restaurant



Note. SDID ATT estimates aggregated over weeks 0 to +8 (Equation 19) for nearby non-reviewed restaurants by distance bin with 95% confidence intervals. The four distance groups contain approximately equal numbers of restaurants. Outcome: ln(Visits+1). Control group: *Both* (never-treated and not-yet-treated restaurants). Standard errors from 100 bootstrap replications, clustered at the census tract level.

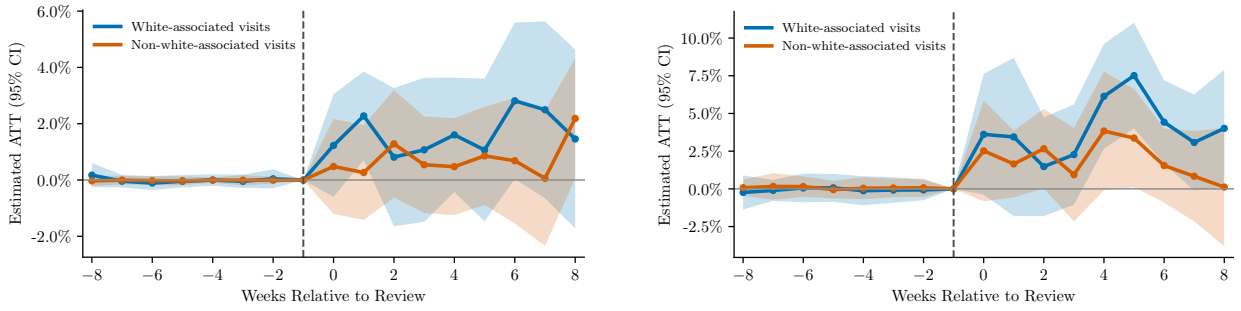
Figure V: Direct Effects by Visitor Origin and Restaurant Location



(a) Restaurants in majority-W CBGs (b) Restaurants in majority-NW CBGs

Note. Weekly ATT estimates by visitor origin, separately for restaurants in majority-W and majority-NW CBGs (Equation 19) with 95% confidence intervals. Outcome: $\ln(\text{Visits} + 1)$ from majority-W/majority-NW CBGs. Review published at $t = 0$; reference period $t = -1$. Control group: *Both* (never-treated and not-yet-treated restaurants). Standard errors from 100 bootstrap replications, clustered at the census tract level.

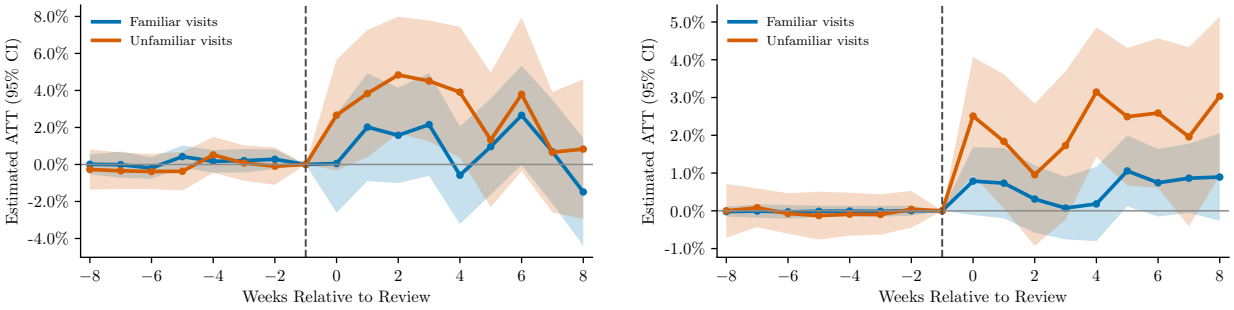
Figure VI: Spillover Effects by Visitor Origin and Restaurant Location



(a) Restaurants in majority-W CBGs (b) Restaurants in majority-NW CBGs

Note. Weekly ATT estimates for spillover visits to non-reviewed restaurants within 300 meters, by visitor origin and restaurant location (Equation 19) with 95% confidence intervals. Outcome: $\ln(\text{Visits}+1)$. Review published at $t = 0$; reference period $t = -1$. Control group: *Both* (never-treated and not-yet-treated restaurants). Standard errors from 100 bootstrap replications, clustered at the census tract level.

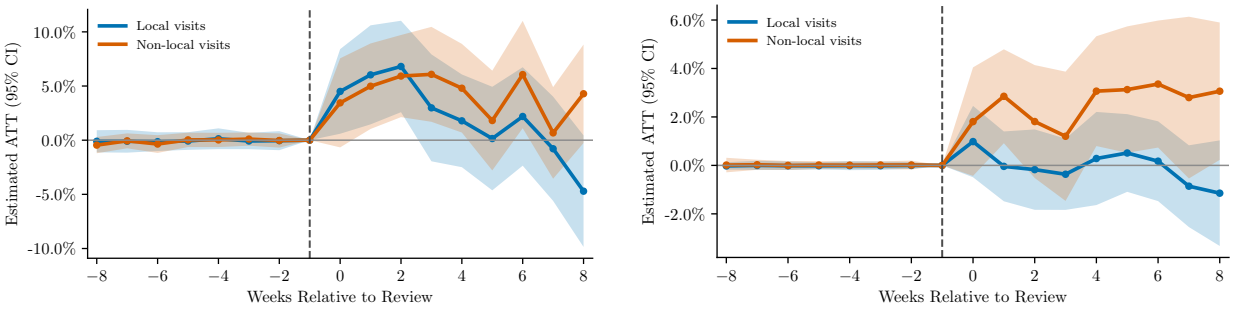
Figure VII: Direct and Spillover Effects by Recent Visitor Exposure to the Census Tract



(a) Direct Effect (b) Spillover Effect

Note. Weekly ATT estimates by visitor familiarity with the focal census tract (Equation 19) with 95% confidence intervals. A visitor is classified as familiar if the device was observed in the tract at least once in the prior 30 days; otherwise the visitor is classified as unfamiliar. Panel (a): direct effect. Panel (b): spillover effect for non-reviewed restaurants within 300 meters. Outcome: $\ln(\text{Visits}+1)$. Review published at $t = 0$; reference period $t = -1$. Control group: *Both*. Standard errors from 100 bootstrap replications, clustered at the census tract level.

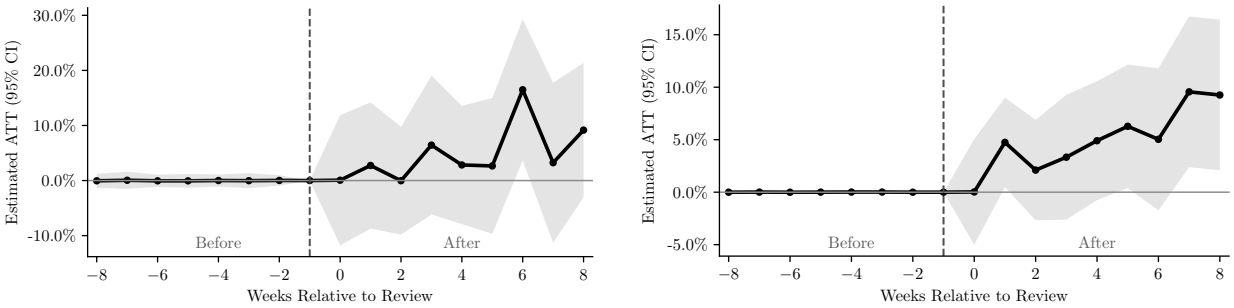
Figure VIII: Direct and Spillover Effects by Visitor Local Status



(a) Direct Effect (b) Spillover Effect

Note. Weekly ATT estimates by visitor local status (Equation 19) with 95% confidence intervals. Trade-area cutoff: 60th percentile of the home-to-restaurant distance distribution (Aversa et al., 2024; Reilly, 1953). For reviewed restaurants, the cutoff is calculated using pre-publication visits. In the current spillover analysis, the cutoff for non-reviewed restaurants is calculated over the analysis period. Panel (a): direct effect. Panel (b): spillover effect for non-reviewed restaurants within 300 meters. Outcome: $\ln(\text{Visits}+1)$. Review published at $t = 0$; reference period $t = -1$. Control group: *Both*. Standard errors from 100 bootstrap replications, clustered at the census tract level.

Figure IX: Direct and Spillover Effects on Restaurants' Trade Area



(a) Direct Effect

(b) Spillover Effect

Note. Weekly ATT estimates for trade area (Equation 19) with 95% confidence intervals. Trade area: 60th percentile of the weekly home-to-restaurant distance distribution (Aversa et al., 2024; Reilly, 1953). Panel (a): direct effect. Panel (b): spillover effect. Outcome: $\ln(\text{Trade Area} + 1)$. Review published at $t = 0$; reference period $t = -1$. Control group: *Both* (never-treated and not-yet-treated restaurants). Standard errors from 100 bootstrap replications, clustered at the census tract level.

Online Appendix

HOW INFORMATION SHAPES THE GEOGRAPHY OF URBAN CONSUMPTION

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A Proofs of Model's Results

This appendix contains the proofs of the results in Section 3.

A.1 Proof of Proposition 1

Proof. We have

$$\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}} = \left(\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}}\right) \cdot \left(\frac{p_{ij}^{t^*+1}}{p_{ij}^{t^*-1}}\right) \cdot \left(\frac{p_{ik}^{t^*+1} \dfrac{x_{ij}^{t^*+1}}{1+x_{ij}^{t^*+1}+x_{ik}^{t^*+1}} + \left(1-p_{ik}^{t^*+1}\right) \dfrac{x_{ij}^{t^*+1}}{1+x_{ij}^{t^*+1}}}{p_{ik}^{t^*-1} \dfrac{x_{ij}^{t^*-1}}{1+x_{ij}^{t^*-1}+x_{ik}^{t^*-1}} + \left(1-p_{ik}^{t^*-1}\right) \dfrac{x_{ij}^{t^*-1}}{1+x_{ij}^{t^*-1}}} \right). \tag{A.1}$$

The expression above consists of three factors. Given the assumptions of the model, the first two factors are weakly greater than 1:

$$\alpha_i^{t^*+1} \geq \alpha_i^{t^*-1} \quad \Longrightarrow \quad \frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} \geq 1, \tag{A.2}$$

$$p_{ij}^{t^*+1} \geq p_{ij}^{t^*-1} \quad \Longrightarrow \quad \frac{p_{ij}^{t^*+1}}{p_{ij}^{t^*-1}} \geq 1. \tag{A.3}$$

Next, the review has no effect on awareness of restaurant k or beliefs about its quality:

$$p_{ik}^{t^*+1} = p_{ik}^{t^*-1}, \tag{A.4}$$

$$\mu_{ik}^{t^*+1} = \mu_{ik}^{t^*-1} \quad \Longrightarrow \quad x_{ik}^{t^*+1} = x_{ik}^{t^*-1}. \tag{A.5}$$

Using these equalities, define

$$A_i(x_{ij}) \equiv p_{ik}^{t^*-1} \frac{x_{ij}}{1+x_{ij}+x_{ik}^{t^*-1}} + \left(1-p_{ik}^{t^*-1}\right) \frac{x_{ij}}{1+x_{ij}}. \tag{A.6}$$

Denote the third factor by

$$F_i = \frac{A_i\left(x_{ij}^{t^*+1}\right)}{A_i\left(x_{ij}^{t^*-1}\right)}. \tag{A.7}$$

The derivative of A_i with respect to x_{ij} is

$$\frac{\partial A_i(x_{ij})}{\partial x_{ij}} = p_{ik}^{t^*-1} \frac{1 + x_{ik}^{t^*-1}}{\left(1 + x_{ij} + x_{ik}^{t^*-1}\right)^2} + \left(1 - p_{ik}^{t^*-1}\right) \frac{1}{(1 + x_{ij})^2} > 0. \tag{A.8}$$

Since

$$\mu_{ij}^{t^*+1} > \mu_{ij}^{t^*-1} \quad \Longrightarrow \quad x_{ij}^{t^*+1} > x_{ij}^{t^*-1}, \tag{A.9}$$

it follows that

$$A_i\left(x_{ij}^{t^*+1}\right) > A_i\left(x_{ij}^{t^*-1}\right), \tag{A.10}$$

and hence $F_i > 1$. Combining these results,

$$\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} \geq 1, \tag{A.11}$$

$$\frac{p_{ij}^{t^*+1}}{p_{ij}^{t^*-1}} \geq 1, \tag{A.12}$$

$$F_i > 1. \tag{A.13}$$

Therefore,

$$\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}} > 1, \tag{A.14}$$

which implies

$$\frac{\mathbb{E}\left(R_{ij}^{t^*+1}\right)}{\mathbb{E}\left(R_{ij}^{t^*-1}\right)} > 1. \tag{A.15}$$

□

A.2 Proof of Lemma 1

Proof. We use the decomposition of $\mathbb{P}_{ij}^{t^*+1}/\mathbb{P}_{ij}^{t^*-1}$ into three factors in Equation (A.1). Each comparative static varies the indicated parameter while holding the remaining primitives fixed.

Comparative static with respect to $\alpha_i^{t^*-1}$

Note that only the first factor of Equation (A.1) depends on $\alpha_i^{t^*-1}$, and the other two factors are positive. Substituting for $\alpha_i^{t^*+1}$, the first factor is

$$\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} = \frac{\alpha_i^{t^*-1} + \left(1 - \alpha_i^{t^*-1}\right)\theta}{\alpha_i^{t^*-1}}. \tag{A.16}$$

Taking the derivative,

$$\frac{\partial}{\partial \alpha_i^{t^*-1}}\left(\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}}\right) = \frac{(1-\theta)\alpha_i^{t^*-1} - \left[\alpha_i^{t^*-1} + \left(1 - \alpha_i^{t^*-1}\right)\theta\right]}{\left(\alpha_i^{t^*-1}\right)^2} = -\frac{\theta}{\left(\alpha_i^{t^*-1}\right)^2} < 0.$$

Therefore,

$$\frac{\partial}{\partial \alpha_i^{t^*-1}}\left(\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}}\right) < 0, \tag{A.17}$$

for $\theta > 0$.

Comparative static with respect to $p_{ij}^{t^*-1}$

Similarly, only the second factor of Equation (A.1) depends on $p_{ij}^{t^*-1}$, and the other factors are positive. Substituting for $p_{ij}^{t^*+1}$, the second factor is

$$\frac{p_{ij}^{t^*+1}}{p_{ij}^{t^*-1}} = \frac{p_{ij}^{t^*-1} + \left(1 - p_{ij}^{t^*-1}\right)\theta}{p_{ij}^{t^*-1}}. \tag{A.18}$$

Taking the derivative, we have

$$\frac{\partial}{\partial p_{ij}^{t^*-1}}\left(\frac{p_{ij}^{t^*+1}}{p_{ij}^{t^*-1}}\right)=\frac{(1-\theta)p_{ij}^{t^*-1}-\left[p_{ij}^{t^*-1}+\left(1-p_{ij}^{t^*-1}\right)\theta\right]}{\left(p_{ij}^{t^*-1}\right)^2}=-\frac{\theta}{\left(p_{ij}^{t^*-1}\right)^2}<0. \tag{A.19}$$

Therefore,

$$\frac{\partial}{\partial p_{ij}^{t^*-1}}\left(\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}}\right)<0, \tag{A.20}$$

for $\theta>0$.

Comparative static with respect to c_i

Only the third factor of Equation (A.1) depends on c_i, and the other factors are positive. This factor is F_i, defined in Equation (A.7). Writing out A_i from Equation (A.6) and putting the terms in the numerator and denominator of F_i over common denominators gives

$$F_i=\left[\frac{x_{ij}^{t^*+1}}{x_{ij}^{t^*-1}}\right]\left[\frac{1+x_{ij}^{t^*-1}}{1+x_{ij}^{t^*+1}}\right]\left[\frac{1+x_{ij}^{t^*-1}+x_{ik}^{t^*-1}}{1+x_{ij}^{t^*+1}+x_{ik}^{t^*-1}}\right]\left[\frac{1+x_{ij}^{t^*+1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}{1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}\right]. \tag{A.21}$$

Define the following:

$$F_i=F_{1i}F_{2i}F_{3i}, \tag{A.22}$$

$$F_{1i}\equiv\frac{x_{ij}^{t^*+1}}{x_{ij}^{t^*-1}}=\frac{\exp\left\{\mu_{ij}^{t^*+1}V^H+\left(1-\mu_{ij}^{t^*+1}\right)V^L-c_i\right\}}{\exp\left\{\mu_{ij}^{t^*-1}V^H+\left(1-\mu_{ij}^{t^*-1}\right)V^L-c_i\right\}}=\frac{\exp\left\{\mu_{ij}^{t^*+1}V^H+\left(1-\mu_{ij}^{t^*+1}\right)V^L\right\}}{\exp\left\{\mu_{ij}^{t^*-1}V^H+\left(1-\mu_{ij}^{t^*-1}\right)V^L\right\}}, \tag{A.23}$$

$$F_{2i}\equiv\left[\frac{1+x_{ij}^{t^*-1}}{1+x_{ij}^{t^*+1}}\right]\left[\frac{1+x_{ij}^{t^*+1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}{1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}\right], \tag{A.24}$$

$$F_{3i}\equiv\frac{1+x_{ij}^{t^*-1}+x_{ik}^{t^*-1}}{1+x_{ij}^{t^*+1}+x_{ik}^{t^*-1}}. \tag{A.25}$$

Note that F_{1i} is not a function of c_i. We next show that $\frac{\partial F_{2i}}{\partial c_i}\geq 0$ and $\frac{\partial F_{3i}}{\partial c_i}>0$. This,

along with the fact that $F_{1i}, F_{2i}, F_{3i} > 0$, implies that

$$\frac{\partial F_i}{\partial c_i} = F_{1i}\frac{\partial F_{2i}}{\partial c_i}F_{3i} + F_{1i}F_{2i}\frac{\partial F_{3i}}{\partial c_i} > 0. \tag{A.26}$$

We have

$$x_{ir}^t = \exp\left\{\mu_{ir}^t V^H + \left(1-\mu_{ir}^t\right)V^L - c_i\right\}, \tag{A.27}$$

$$\frac{\partial x_{ir}^t}{\partial c_i} = -x_{ir}^t. \tag{A.28}$$

First, consider F_{2i}. Differentiating its first ratio gives

$$\frac{\partial}{\partial c_i}\left(\frac{1+x_{ij}^{t^*-1}}{1+x_{ij}^{t^*+1}}\right) = \frac{-x_{ij}^{t^*-1}\left(1+x_{ij}^{t^*+1}\right)+\left(1+x_{ij}^{t^*-1}\right)x_{ij}^{t^*+1}}{\left(1+x_{ij}^{t^*+1}\right)^2} = \frac{x_{ij}^{t^*+1}-x_{ij}^{t^*-1}}{\left(1+x_{ij}^{t^*+1}\right)^2}. \tag{A.29}$$

Differentiating the second ratio gives

$$\begin{aligned}
&\frac{\partial}{\partial c_i}\left(\frac{1+x_{ij}^{t^*+1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}{1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}\right)\\
&= \frac{-\left[x_{ij}^{t^*+1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]\left[1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]}{\left[1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]^2}\\
&\quad+\frac{\left[1+x_{ij}^{t^*+1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]\left[x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]}{\left[1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]^2}\\
&= \frac{x_{ij}^{t^*-1}-x_{ij}^{t^*+1}}{\left[1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]^2}.
\end{aligned} \tag{A.30}$$

Applying the product rule,

$$\begin{aligned}
\frac{\partial F_{2i}}{\partial c_i} &= \frac{x_{ij}^{t^*+1}-x_{ij}^{t^*-1}}{\left(1+x_{ij}^{t^*+1}\right)^2}\frac{1+x_{ij}^{t^*+1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}{1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}}\\
&\quad+\frac{1+x_{ij}^{t^*-1}}{1+x_{ij}^{t^*+1}}\frac{x_{ij}^{t^*-1}-x_{ij}^{t^*+1}}{\left[1+x_{ij}^{t^*-1}+\left(1-p_{ik}^{t^*-1}\right)x_{ik}^{t^*-1}\right]^2}.
\end{aligned} \tag{A.31}$$

Factoring out $x_{ij}^{t^*+1} - x_{ij}^{t^*-1}$ and putting the two terms over a common denominator gives

$$\begin{aligned}\frac{\partial F_{2i}}{\partial c_i} = & \frac{x_{ij}^{t^*+1} - x_{ij}^{t^*-1}}{\left(1 + x_{ij}^{t^*+1}\right)^2 \left[1 + x_{ij}^{t^*-1} + \left(1 - p_{ik}^{t^*-1}\right) x_{ik}^{t^*-1}\right]^2} \\ & \times \Bigg\{ \left[1 + x_{ij}^{t^*+1} + \left(1 - p_{ik}^{t^*-1}\right) x_{ik}^{t^*-1}\right] \left[1 + x_{ij}^{t^*-1} + \left(1 - p_{ik}^{t^*-1}\right) x_{ik}^{t^*-1}\right] \\ & \quad - \left(1 + x_{ij}^{t^*-1}\right) \left(1 + x_{ij}^{t^*+1}\right) \Bigg\}. \end{aligned} \tag{A.32}$$

After some additional algebra, and since $1 - p_{ik}^{t^*-1} \geq 0$ and $x_{ij}^{t^*+1} - x_{ij}^{t^*-1} > 0$, we have

$$\begin{aligned}\frac{\partial F_{2i}}{\partial c_i} = & \frac{\left(1 - p_{ik}^{t^*-1}\right) x_{ik}^{t^*-1} \left(x_{ij}^{t^*+1} - x_{ij}^{t^*-1}\right)}{\left(1 + x_{ij}^{t^*+1}\right)^2 \left[1 + x_{ij}^{t^*-1} + \left(1 - p_{ik}^{t^*-1}\right) x_{ik}^{t^*-1}\right]^2} \\ & \times \left[2 + x_{ij}^{t^*+1} + x_{ij}^{t^*-1} + \left(1 - p_{ik}^{t^*-1}\right) x_{ik}^{t^*-1}\right] \geq 0. \end{aligned} \tag{A.33}$$

Finally, consider

$$F_{3i} = \frac{1 + x_{ij}^{t^*-1} + x_{ik}^{t^*-1}}{1 + x_{ij}^{t^*+1} + x_{ik}^{t^*-1}}. \tag{A.34}$$

Taking the derivative yields

$$\begin{aligned}\frac{\partial F_{3i}}{\partial c_i} &= \frac{-\left(x_{ij}^{t^*-1} + x_{ik}^{t^*-1}\right)\left(1 + x_{ij}^{t^*+1} + x_{ik}^{t^*-1}\right)}{\left(1 + x_{ij}^{t^*+1} + x_{ik}^{t^*-1}\right)^2} + \frac{\left(1 + x_{ij}^{t^*-1} + x_{ik}^{t^*-1}\right)\left(x_{ij}^{t^*+1} + x_{ik}^{t^*-1}\right)}{\left(1 + x_{ij}^{t^*+1} + x_{ik}^{t^*-1}\right)^2} \\ &= \frac{x_{ij}^{t^*+1} - x_{ij}^{t^*-1}}{\left(1 + x_{ij}^{t^*+1} + x_{ik}^{t^*-1}\right)^2} > 0. \end{aligned} \tag{A.35}$$

Therefore, since we showed that $\frac{\partial F_{2i}}{\partial c_i} \geq 0$ and $\frac{\partial F_{3i}}{\partial c_i} > 0$, as discussed before, we have shown that

$$\frac{\partial}{\partial c_i}\left(\frac{\mathbb{P}_{ij}^{t^*+1}}{\mathbb{P}_{ij}^{t^*-1}}\right) > 0. \tag{A.36}$$

The derivative of the change in beliefs

By Bayes' rule,

$$\mu_{ij}^{t^*+1} = \frac{\Pr(+ \mid V^H)\mu_{ij}^{t^*-1}}{\Pr(+ \mid V^H)\mu_{ij}^{t^*-1} + \Pr(+ \mid V^L)(1-\mu_{ij}^{t^*-1})}. \tag{A.37}$$

The change in belief is:

$$\begin{aligned}
&\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1} \\
&= \frac{\Pr(+ \mid V^H)\mu_{ij}^{t^*-1}}{\Pr(+ \mid V^H)\mu_{ij}^{t^*-1} + \Pr(+ \mid V^L)(1-\mu_{ij}^{t^*-1})} - \mu_{ij}^{t^*-1} \\
&= \frac{\left[\Pr(+ \mid V^H) - \Pr(+ \mid V^L)\right]\mu_{ij}^{t^*-1}(1-\mu_{ij}^{t^*-1})}{\Pr(+ \mid V^H)\mu_{ij}^{t^*-1} + \Pr(+ \mid V^L)(1-\mu_{ij}^{t^*-1})}.
\end{aligned} \tag{A.38}$$

Differentiating the change in beliefs with respect to the prior gives

$$\begin{aligned}
&\frac{\partial\left(\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1}\right)}{\partial \mu_{ij}^{t^*-1}} \\
&= \frac{\left[\Pr(+ \mid V^H) - \Pr(+ \mid V^L)\right]\left[\Pr(+ \mid V^L)(1-2\mu_{ij}^{t^*-1}) - \left(\Pr(+ \mid V^H) - \Pr(+ \mid V^L)\right)(\mu_{ij}^{t^*-1})^2\right]}{\left[\Pr(+ \mid V^H)\mu_{ij}^{t^*-1} + \Pr(+ \mid V^L)(1-\mu_{ij}^{t^*-1})\right]^2}.
\end{aligned} \tag{A.39}$$

Since $\Pr(+ \mid V^H) > \Pr(+ \mid V^L) > 0$ and $\mu_{ij}^{t^*-1} \geq \frac{1}{2}$, the first term in the brackets is weakly negative, while the second term is strictly negative. Therefore, the expression in the brackets is strictly negative, and

$$\frac{\partial\left(\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1}\right)}{\partial \mu_{ij}^{t^*-1}} < 0. \tag{A.40}$$

□

A.3 Proof of Proposition 2

Proof. Let

$$\frac{\mathbb{P}_{Lj}^{t^*+1}}{\mathbb{P}_{Lj}^{t^*-1}} = \frac{\alpha_L^{t^*+1}}{\alpha_L^{t^*-1}} \frac{p_{Lj}^{t^*+1}}{p_{Lj}^{t^*-1}} F_L, \tag{A.41}$$

$$\frac{\mathbb{P}_{Nj}^{t^*+1}}{\mathbb{P}_{Nj}^{t^*-1}} = \frac{\alpha_N^{t^*+1}}{\alpha_N^{t^*-1}} \frac{p_{Nj}^{t^*+1}}{p_{Nj}^{t^*-1}} F_N, \tag{A.42}$$

where F_L and F_N are given by Equation (A.21) with $i = L$ and $i = N$, respectively.

Using Assumption 1, we have $\alpha_L^{t^*-1} > \alpha_N^{t^*-1}$ and $p_{Lj}^{t^*-1} > p_{Nj}^{t^*-1}$. Using Lemma 1, this implies that

$$\frac{\alpha_N^{t^*+1}}{\alpha_N^{t^*-1}} > \frac{\alpha_L^{t^*+1}}{\alpha_L^{t^*-1}}, \tag{A.43}$$

$$\frac{p_{Nj}^{t^*+1}}{p_{Nj}^{t^*-1}} > \frac{p_{Lj}^{t^*+1}}{p_{Lj}^{t^*-1}}. \tag{A.44}$$

It remains to compare F_N and F_L. Based on the proof of Lemma 1, $\frac{\partial F_i}{\partial c_i} > 0$: F_i is strictly increasing in c_i.

As defined before,

$$x_{ir}^t = \exp\left\{\mu_{ir}^t V^H + (1 - \mu_{ir}^t) V^L - c_i\right\}. \tag{A.45}$$

As $c_i \to \infty$,

$$x_{ij}^{t^*-1} \to 0, \qquad x_{ij}^{t^*+1} \to 0, \qquad x_{ik}^{t^*-1} \to 0. \tag{A.46}$$

The first factor of F_i is not a function of c_i since

$$\frac{x_{ij}^{t^*+1}}{x_{ij}^{t^*-1}} = \frac{\exp\left\{\mu_{ij}^{t^*+1} V^H + (1 - \mu_{ij}^{t^*+1}) V^L - c_i\right\}}{\exp\left\{\mu_{ij}^{t^*-1} V^H + (1 - \mu_{ij}^{t^*-1}) V^L - c_i\right\}} = \exp\left\{(\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1})(V^H - V^L)\right\}. \tag{A.47}$$

Hence, as $c_i \to \infty$, the remaining three factors in Equation (A.21) converge to one. Hence, we have

$$\lim_{c_i \to \infty} F_i = \exp\left\{(\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1})(V^H - V^L)\right\}. \tag{A.48}$$

Based on Assumption 1, $\mu_{Nj}^{t^*-1} = \frac{1}{2} < \mu_{Lj}^{t^*-1} < 1$. One of the results of Lemma 1 is

$$\frac{\partial\left(\mu_{ij}^{t^*+1} - \mu_{ij}^{t^*-1}\right)}{\partial \mu_{ij}^{t^*-1}} < 0 \quad \text{for} \quad \mu_{ij}^{t^*-1} \geq \frac{1}{2}. \tag{A.49}$$

Combining this result with the assumption that $V^H - V^L > 0$, yields

$$\exp\left\{(\mu_{Nj}^{t^*+1} - \mu_{Nj}^{t^*-1})(V^H - V^L)\right\} > \exp\left\{(\mu_{Lj}^{t^*+1} - \mu_{Lj}^{t^*-1})(V^H - V^L)\right\}. \tag{A.50}$$

Therefore,

$$\lim_{c \to \infty} F_N(c) > \lim_{c \to \infty} F_L(c). \tag{A.51}$$

Combining these inequalities,

$$F_L(c_L) < \lim_{c \to \infty} F_L(c) < \lim_{c_N \to \infty} F_N(c_N). \tag{A.52}$$

Therefore,

$$\lim_{c_N \to \infty} F_N(c_N) > F_L(c_L). \tag{A.53}$$

Since the limit is strictly greater than $F_L(c_L)$, there exists a finite c^* such that, for all $c_N > c^*$,

$$F_N(c_N) > F_L(c_L). \tag{A.54}$$

For every $c_N > c^*$, we therefore have

$$\frac{\alpha_N^{t^*+1}}{\alpha_N^{t^*-1}} > \frac{\alpha_L^{t^*+1}}{\alpha_L^{t^*-1}}, \tag{A.55}$$

$$\frac{p_{Nj}^{t^*+1}}{p_{Nj}^{t^*-1}} > \frac{p_{Lj}^{t^*+1}}{p_{Lj}^{t^*-1}}, \tag{A.56}$$

$$F_N(c_N) > F_L(c_L). \tag{A.57}$$

Multiplying these three inequalities yields

$$\frac{\mathbb{P}_{Nj}^{t^*+1}}{\mathbb{P}_{Nj}^{t^*-1}} > \frac{\mathbb{P}_{Lj}^{t^*+1}}{\mathbb{P}_{Lj}^{t^*-1}}. \tag{A.58}$$

Since $M_N, M_L > 0$, cross-multiplication and cancellation of common terms imply

$$\frac{M_N \mathbb{P}_{Nj}^{t^*+1}}{M_L \mathbb{P}_{Lj}^{t^*+1} + M_N \mathbb{P}_{Nj}^{t^*+1}} > \frac{M_{Nj} \mathbb{P}_{Nj}^{t^*-1}}{M_L \mathbb{P}_{Lj}^{t^*-1} + M_N \mathbb{P}_{Nj}^{t^*-1}}. \tag{A.59}$$

Thus, the share of visits to the reviewed restaurant from non-local consumers increases following the review.

□

A.4 Proof of Proposition 3

Proof. The ratio of the post-review and pre-review probabilities is:

$$\frac{\mathbb{P}_{ik}^{t^*+1}}{\mathbb{P}_{ik}^{t^*-1}} = \left(\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}}\right)\left(\frac{p_{ik}^{t^*+1}}{p_{ik}^{t^*-1}}\right)\left(\frac{x_{ik}^{t^*+1}}{x_{ik}^{t^*-1}}\right) \times \frac{\dfrac{p_{ij}^{t^*+1}}{1+x_{ij}^{t^*+1}+x_{ik}^{t^*+1}} + \dfrac{1-p_{ij}^{t^*+1}}{1+x_{ik}^{t^*+1}}}{\dfrac{p_{ij}^{t^*-1}}{1+x_{ij}^{t^*-1}+x_{ik}^{t^*-1}} + \dfrac{1-p_{ij}^{t^*-1}}{1+x_{ik}^{t^*-1}}}. \tag{A.60}$$

Since we assume that the review does not affect p_{ik} or μ_{ik}, we have $p_{ik}^{t^*+1} = p_{ik}^{t^*-1}$ and $x_{ik}^{t^*+1} = x_{ik}^{t^*-1}$. Therefore,

$$\frac{\mathbb{P}_{ik}^{t^*+1}}{\mathbb{P}_{ik}^{t^*-1}} = \frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} G_i, \tag{A.61}$$

where

$$G_i \equiv \frac{\dfrac{p_{ij}^{t^*+1}}{1+x_{ij}^{t^*+1}+x_{ik}^{t^*+1}} + \dfrac{1-p_{ij}^{t^*+1}}{1+x_{ik}^{t^*+1}}}{\dfrac{p_{ij}^{t^*-1}}{1+x_{ij}^{t^*-1}+x_{ik}^{t^*-1}} + \dfrac{1-p_{ij}^{t^*-1}}{1+x_{ik}^{t^*-1}}}. \tag{A.62}$$

Using $x_{ik}^{t^*+1} = x_{ik}^{t^*-1}$, this is equivalently

$$G_i = \frac{\dfrac{p_{ij}^{t^*+1}}{1+x_{ij}^{t^*+1}+x_{ik}^{t^*-1}} + \dfrac{1-p_{ij}^{t^*+1}}{1+x_{ik}^{t^*-1}}}{\dfrac{p_{ij}^{t^*-1}}{1+x_{ij}^{t^*-1}+x_{ik}^{t^*-1}} + \dfrac{1-p_{ij}^{t^*-1}}{1+x_{ik}^{t^*-1}}}. \tag{A.63}$$

The first factor in the decomposition captures the neighborhood expansion effect. Since $\theta > 0$,

$$\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} \geq 1, \tag{A.64}$$

with strict inequality if $\alpha_i^{t^*-1} < 1$ and equality if $\alpha_i^{t^*-1} = 1$.

We next show that $G_i < 1$. This term captures the substitution effect toward the reviewed restaurant. Define

$$H_i(p,x) \equiv \frac{p}{1+x+x_{ik}^{t^*-1}} + \frac{1-p}{1+x_{ik}^{t^*-1}}. \tag{A.65}$$

Because $x_{ik}^{t^*+1} = x_{ik}^{t^*-1}$,

$$G_i = \frac{H_i\left(p_{ij}^{t^*+1}, x_{ij}^{t^*+1}\right)}{H_i\left(p_{ij}^{t^*-1}, x_{ij}^{t^*-1}\right)}. \tag{A.66}$$

Differentiating $H_i(p,x)$ with respect to p gives

$$\frac{\partial H_i(p,x)}{\partial p} = \frac{1}{1+x+x_{ik}^{t^*-1}} - \frac{1}{1+x_{ik}^{t^*-1}} \tag{A.67}$$

$$= -\frac{x}{\left(1+x+x_{ik}^{t^*-1}\right)\left(1+x_{ik}^{t^*-1}\right)} < 0. \tag{A.68}$$

Similarly,

$$\frac{\partial H_i(p,x)}{\partial x} = -\frac{p}{\left(1+x+x_{ik}^{t^*-1}\right)^2} < 0. \tag{A.69}$$

Since $p_{ij}^{t^*+1} \geq p_{ij}^{t^*-1}$ and $x_{ij}^{t^*+1} > x_{ij}^{t^*-1}$ and $H_i(p,x)$ is decreasing in both arguments,

$$H_i\left(p_{ij}^{t^*+1}, x_{ij}^{t^*+1}\right) \leq H_i\left(p_{ij}^{t^*-1}, x_{ij}^{t^*+1}\right) < H_i\left(p_{ij}^{t^*-1}, x_{ij}^{t^*-1}\right). \tag{A.70}$$

This implies that $G_i < 1$.

The spillover effect on visits is positive if and only if

$$\frac{\mathbb{P}_{ik}^{t^*+1}}{\mathbb{P}_{ik}^{t^*-1}} > 1 \iff \frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} G_i > 1 \tag{A.71}$$

$$\iff \frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} > \frac{1}{G_i}. \tag{A.72}$$

1. For part (a), using (7),

$$\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}} = 1 + \theta\left(\frac{1}{\alpha_i^{t^*-1}} - 1\right), \tag{A.73}$$

and therefore

$$\frac{\partial}{\partial \alpha_i^{t^*-1}}\left(\frac{\alpha_i^{t^*+1}}{\alpha_i^{t^*-1}}\right) = -\frac{\theta}{(\alpha_i^{t^*-1})^2} < 0. \tag{A.74}$$

Since the second factor in the spillover ratio is positive and does not depend on $\alpha_i^{t^*-1}$,

$$\frac{\partial}{\partial \alpha_i^{t^*-1}}\left(\frac{\mathbb{P}_{ik}^{t^*+1}}{\mathbb{P}_{ik}^{t^*-1}}\right) < 0. \tag{A.75}$$

2. Consider the special case of local consumer where $a_L^{t^*-1} = 1$. Therefore,

$$\frac{\alpha_L^{t^*+1}}{\alpha_L^{t^*-1}} = 1. \tag{A.76}$$

It follows that

$$\frac{\mathbb{P}_{Lk}^{t^*+1}}{\mathbb{P}_{Lk}^{t^*-1}} = G_L < 1. \tag{A.77}$$

3. Next, consider non-local consumers. The proportional spillover effect for non-local consumers can be written as

$$\frac{\mathbb{P}_{Nk}^{t^*+1}}{\mathbb{P}_{Nk}^{t^*-1}} = \frac{\alpha_N^{t^*+1}}{\alpha_N^{t^*-1}} G_N, \tag{A.78}$$

where

$$G_N = \frac{\dfrac{p_{Nj}^{t^*+1}}{1+x_{Nj}^{t^*+1}+x_{Nk}^{t^*-1}} + \dfrac{1-p_{Nj}^{t^*+1}}{1+x_{Nk}^{t^*-1}}}{\dfrac{p_{Nj}^{t^*-1}}{1+x_{Nj}^{t^*-1}+x_{Nk}^{t^*-1}} + \dfrac{1-p_{Nj}^{t^*-1}}{1+x_{Nk}^{t^*-1}}}. \tag{A.79}$$

From the definition of x_{ir}^t, as $c_N \to \infty$,

$$x_{Nj}^{t^*-1} \to 0, \qquad x_{Nj}^{t^*+1} \to 0, \qquad x_{Nk}^{t^*-1} \to 0. \tag{A.80}$$

This implies that

$$\lim_{c_N \to \infty} G_N = \frac{p_{Nj}^{t^*+1} + \left(1 - p_{Nj}^{t^*+1}\right)}{p_{Nj}^{t^*-1} + \left(1 - p_{Nj}^{t^*-1}\right)} = 1. \tag{A.81}$$

Hence, and since $\theta > 0$ and $\alpha_N^{t^*-1} < 1$,

$$\lim_{c_N \to \infty} \frac{\mathbb{P}_{Nk}^{t^*+1}}{\mathbb{P}_{Nk}^{t^*-1}} = \frac{\alpha_N^{t^*+1}}{\alpha_N^{t^*-1}} > 1. \tag{A.82}$$

Since this limit is greater than 1, there exists a finite c^* such that, for all $c_N > c^*$,

$$\frac{\mathbb{P}_{Nk}^{t^*+1}}{\mathbb{P}_{Nk}^{t^*-1}} > 1. \tag{A.83}$$

4. By parts (a) and (b),

$$\frac{\mathbb{P}_{Lk}^{t^*+1}}{\mathbb{P}_{Lk}^{t^*-1}} < 1 < \frac{\mathbb{P}_{Nk}^{t^*+1}}{\mathbb{P}_{Nk}^{t^*-1}}. \tag{A.84}$$

Therefore,

$$\frac{\mathbb{P}_{Nk}^{t^*+1}}{\mathbb{P}_{Nk}^{t^*-1}} > \frac{\mathbb{P}_{Lk}^{t^*+1}}{\mathbb{P}_{Lk}^{t^*-1}}, \tag{A.85}$$

which is equivalent to

$$\frac{M_N \mathbb{P}_{Nk}^{t^*+1}}{M_L \mathbb{P}_{Lk}^{t^*+1} + M_N \mathbb{P}_{Nk}^{t^*+1}} > \frac{M_N \mathbb{P}_{Nk}^{t^*-1}}{M_L \mathbb{P}_{Lk}^{t^*-1} + M_N \mathbb{P}_{Nk}^{t^*-1}}. \tag{A.86}$$

Thus, the non-local share of visits to the neighboring restaurant increases following the review.

□

B Data

B.1 Critics' reviews

We use the 23 U.S. markets covered by Eater.com in 2019 as our initial set of candidate metropolitan areas with active and diverse restaurant scenes.[13]

Newspaper reviews. For each Eater market, we identify the largest local newspaper by circulation using data from the Local News Initiative at Northwestern University's Medill School of Journalism.[14] We then identify active food critics at each newspaper during the analysis period through manual research, drawing on website archives, Google Search, and press coverage of staff changes. For example, we identified Bill Addison and Patricia Escarcéga as the critics of the *Los Angeles Times* based on contemporary press coverage,[15] and identified *New York Times* critics via the structured restaurant reviews archive.[16] This yields 11 newspapers across 11 metropolitan areas (see Table I). That is, in 12 of the markets we cannot identify an active food critic at the largest newspaper during our analysis period.

Our procedure for collecting reviews in these 11 newspapers is the following. We first attempt using the outlet's online archive, which is only feasible when the archive is organized in a way that allows systematic retrieval of restaurant reviews.[17] When an outlet's archive is not organized in a way that allows automated retrieval, we query ProQuest[18] by author name using the format `Author(`*`name-of-critic`*`)`. Finally, if ProQuest yields no results, we use Google News,[19] searching the outlet's domain for articles mentioning the critic's name for

[13] Eater.com (https://www.eater.com) is a food and restaurant news platform owned by Vox Media that covers restaurant openings, closings, and industry developments across major US cities. It employed restaurant critics at various points in its history but had moved away from traditional restaurant reviews by the start of our analysis period. Its city selection therefore reflects markets where restaurant activity and food media are sufficiently developed.

[14] https://localnewsinitiative.northwestern.edu/

[15] https://www.latimes.com/newsletters/la-fo-food-newsletter-new-restaurant-critics-20190110-story.html

[16] https://www.nytimes.com/reviews/dining

[17] Some outlets maintain structured review sections organized by author or topic. For example, the *New York Times* restaurant reviews archive at https://www.nytimes.com/reviews/dining).

[18] https://www.proquest.com

[19] https://news.google.com

each week of the analysis period.[20] A research assistant manually filtered retrieved articles to retain only restaurant reviews.

B.1.1 Reviewed Restaurants Are Primarily Independent

We verify that the reviewed restaurants sample primarily consists of independent restaurants using SafeGraph Places, which provides information on physical points of interest, including restaurants. We classify a restaurant as independent if its name appears in only one location within the same county in the SafeGraph universe. We first match our sample of reviewed restaurants with the SafeGraph Places data. We follow the sequential procedure. We first attempt an exact match on phone number or website, both of which we collected from Yelp.com for each restaurant in our sample. If neither yields a match, we attempt an exact name match within the same ZIP code. If that also fails, we apply a fuzzy match on name and address within the same ZIP code using the RapidFuzz library in Python,[21] retaining a match only if both the name and address scores each exceed 80 out of 100. Among the 346 reviewed restaurants successfully matched (61.3% of the 564 reviewed restaurants), 96.2% are classified as independent under this definition (333 independent restaurants and 13 chain restaurants; Table B.1). The relatively low match rate may reflect two factors: SafeGraph's panel tends to track establishments with sufficient visitor volume to be reliably observed (Zhao et al., 2026), and our matching criteria are strict to avoid false positives.

B.1.2 Review Valence

We classify each review as positive, neutral, or negative using the OpenAI API with GPT-4o-mini. Each review is submitted independently with a prompt requesting a single classification label. Only 2% are classified as negative, consistent with the norm that critics predominantly publish endorsements (Burry et al., 1985).

[20] Example for Bill Addison at the *Los Angeles Times*, second week of 2019: `intext:''Bill Addison'' site:latimes.com after:2019-01-09 before:2019-01-16`.

[21] https://pypi.org/project/rapidfuzz/

Table B.1: Match to SafeGraph and Share of Independent Restaurants Among Reviewed Restaurants

	Restaurants	Share
Reviewed restaurants	564	
Matched to SafeGraph	346	61.3%
Matched restaurants	346	
Independent	333	96.2%
Chain	13	3.8%

Note. Reviewed restaurants are restaurants reviewed by critics. A restaurant is matched in this order: exact phone number, exact website, exact name within the same ZIP code, and a fuzzy match on name and address within the same ZIP code that requires both scores to exceed 80 out of 100. The share matched is relative to all reviewed restaurants. The independent and chain shares are relative to matched restaurants. A matched restaurant is classified as independent if its name appears at only one location in its county in the SafeGraph universe. The county is the most common county of the restaurant's ZIP code.

B.2 Restaurant Foot Traffic

We measure foot traffic using stop-level geolocation data from Cuebiq. Each stop record includes an anonymized device identifier, centroid GPS coordinates, dwell time, and start timestamp. We infer restaurant visits from stops as follows.

Step 1: Spatial filter. We retain stops whose centroid falls within 25 meters of any restaurant in our sample. Each retained stop is associated with at least one restaurant.

Step 2: Employee and resident exclusion. We drop all observations for a given device-restaurant pair if the device dwelled at that restaurant for more than 6 hours at any point during the analysis period, or was observed there three or more times within any 5-day rolling window. These thresholds target employees and nearby residents.

Step 3: Operating hours filter. We retain only stops that fall within the restaurant's operating hours, as collected from Yelp. For restaurants with missing hours, we assume 8 AM to 11 PM on all days.

Step 4: Co-location resolution. When a stop falls within 25 meters of more than one

restaurant—as happens when restaurants share a building or plaza—we assign it to the nearest restaurant. Ties are broken randomly.

Step 5: Dwell time filter. We define a visit as a stop with dwell time between 4 and 180 minutes, following Athey et al. (2018) and Shoshani et al. (2024).[22]

All visits are aggregated to the restaurant-week level by agreement with our data provider.

C Estimation

C.1 Synthetic Difference-in-Differences: Full Details

This appendix provides the optimization problems for the unit and time weights described in Section 5.2, formalizes the control pool eligibility condition for the *both* specification, and defines the effective number of potential controls. We use the notation established in Section 5.2: $\mathcal{I}_c$ is the set of treated restaurants in cohort c, $\mathcal{D}_c$ is the set of potential control restaurants, and $n_c = |\mathcal{I}_c|$.

Control pool. Under the *never* specification, $\mathcal{D}_c$ contains restaurants never reviewed during the sample period. Under the *both* specification, $\mathcal{D}_c$ additionally contains future-reviewed restaurants whose publication week t_i satisfies

$$t_i > t_c + K^{\text{post}}, \tag{C.1}$$

where t_c is the focal cohort's publication week and K^{post} is the post-publication window length. This restriction ensures that potential controls in the *both* pool remain untreated throughout the estimation window. We set $K^{\text{post}} = 8$ for both the event studies and the tabular ATT estimates (i.e., the aggregate effect over weeks 0 to +8), so that the estimates reflect the more immediate effect. Event studies use the *both* specification, including never-

[22]The 4-minute lower bound is consistent with other location data providers; see e.g. https://docs.safegraph.com/docs/monthly-patterns#raw_visit_counts. The 180-minute upper bound excludes observations consistent with employees working a full shift.

treated and not-yet-treated restaurants as controls. For the tables, we report both control specifications (i.e., *never* and *both*).

Additional Notation. Let $N_c = |\mathcal{D}_c|$ and $T_{\text{pre}} = 8$. Define:

- $Y^{\text{co}}_{c,\text{pre}} \in \mathbb{R}^{N_c \times T_{\text{pre}}}$: matrix of outcomes for potential control restaurants during the last T_{pre} pre-publication weeks. Row i is the outcome series for restaurant $i \in \mathcal{D}_c$; column ℓ corresponds to week $t_c + \ell$ for $\ell \in \{-T_{\text{pre}}, \ldots, -1\}$.
- $\bar{y}^{\text{tr}}_{c,\text{pre}} \in \mathbb{R}^{T_{\text{pre}}}$: vector of treated-cohort average outcomes over the same weeks. The ℓ-th entry equals $\bar{Y}^{\text{tr}}_{c,t_c+\ell}$ as defined in Section 5.2.
- $\bar{y}^{\text{co}}_{c,\text{post}} \in \mathbb{R}^{N_c}$: vector of post-publication averages for potential control restaurants. Entry i is the average outcome of restaurant $i \in \mathcal{D}_c$ over the post-publication window.

The probability vector over potential controls and pre-publication periods are

$$\Delta^{N_c} = \left\{\omega \in \mathbb{R}^{N_c}_+ : \mathbf{1}^\top \omega = 1\right\}, \qquad \Delta^{T_{\text{pre}}} = \left\{\lambda \in \mathbb{R}^{T_{\text{pre}}}_+ : \mathbf{1}^\top \lambda = 1\right\}. \tag{C.2}$$

Unit weights. The unit weights $\hat{\omega}_c \in \Delta^{N_c}$ minimize the following objective over $\omega \in \Delta^{N_c}$:

$$\hat{\omega}_c = \arg\min_{\omega \in \Delta^{N_c}} \left\| \left(Y^{\text{co}}_{c,\text{pre}}\right)^\top \omega - \bar{y}^{\text{tr}}_{c,\text{pre}} \right\|_2^2 + \zeta_\omega^2 \, T_{\text{pre}} \, \|\omega\|_2^2. \tag{C.3}$$

The objective has two terms. The first is the squared distance between $(Y^{\text{co}}_{c,\text{pre}})^\top \omega \in \mathbb{R}^{T_{\text{pre}}}$, which is the weighted average pre-publication outcome trajectory of potential control restaurants, with ℓ-th entry equal to the weighted average outcome across $\mathcal{D}_c$ in pre-period week ℓ, and the treated cohort's average trajectory $\bar{y}^{\text{tr}}_{c,\text{pre}}$. The second term is $\|\omega\|_2^2 = \sum_{i \in \mathcal{D}_c} \omega_i^2$, which is minimized when weights are equally distributed across potential controls and maximized when all weight falls on a single restaurant. Including it in the objective therefore discourages concentration on a small number of potential controls. The solution $\hat{\omega}_c = (\hat{\omega}_{c,i})_{i \in \mathcal{D}_c}$ corresponds to $\hat{\omega}_{c,i}$ in Section 5.2.

Time weights. The time weights $\hat{\lambda}_c \in \Delta^{T_{\text{pre}}}$ minimize the following objective over $\lambda \in \Delta^{T_{\text{pre}}}$:

$$\hat{\lambda}_c = \arg\min_{\lambda \in \Delta^{T_{\text{pre}}}} \left\| Y^{\text{co}}_{c,\text{pre}} \lambda - \bar{y}^{\text{co}}_{c,\text{post}} \right\|_2^2 + \zeta_\lambda^2 \, N_c \, \|\lambda\|_2^2. \tag{C.4}$$

The objective has two terms. The first is the squared distance between $Y^{\text{co}}_{c,\text{pre}} \lambda \in \mathbb{R}^{N_c}$, which is the time-weighted pre-publication outcome vector for potential control restaurants, with i-th entry $\sum_\ell Y^{\text{co}}_{c,t_c+\ell,i} \, \lambda_\ell$ equal to the time-weighted pre-publication outcome for control restaurant i, and $\bar{y}^{\text{co}}_{c,\text{post}}$, the vector of post-publication averages for potential control restaurants.[23] The second term is $\|\lambda\|_2^2 = \sum_\ell \lambda_\ell^2$, which is minimized when weights are equally distributed across pre-publication periods and maximized when all weight falls on a single week. Including it in the objective therefore discourages concentration on a single pre-publication period. The solution $\hat{\lambda}_c = (\hat{\lambda}_{c,\ell})_\ell$ corresponds to $\hat{\lambda}_{c,t}$ in Section 5.2.[24]

Effective number of potential controls. To assess the concentration of the unit weights, we compute the effective number of potential controls for each cohort:

$$N_{\text{eff},c} = \frac{1}{\|\hat{\omega}_c\|_2^2} = \frac{1}{\sum_{i \in \mathcal{D}_c} \hat{\omega}_{c,i}^2}. \tag{C.5}$$

Under uniform weighting, $N_{\text{eff},c} = N_c$. As the weights become more concentrated, $N_{\text{eff},c}$ approaches one. We report $N_{\text{eff},c}$ in each table in Section 6.

[23] The time weights are estimated using only the outcomes of potential control restaurants because the control group is untreated throughout the estimation window, so its post-publication outcomes identify the counterfactual time trend. The same weights are then applied to the treated cohort's pre-publication outcomes when computing the pre-publication baseline $\bar{Y}^{\text{tr}}_{c,\text{pre}}(\hat{\lambda}_c)$ in Equation (16).

[24] Both weight problems are solved using L-BFGS-B with nonnegativity bounds, which substantially reduces computation time relative to the Frank–Wolfe algorithm used in Arkhangelsky et al. (2021). The raw weights are normalized to sum to one after optimization. Table C.1 shows that the resulting ATT estimates and standard errors are nearly identical across the two algorithms.

Table C.1: Direct Effect ATT by Weight Solver (Random Sample, 20% of Restaurants)

	Outcome: ln(Visits + 1)	
	L-BFGS-B	Frank-Wolfe
	(1)	(2)
ATT	-0.0120	-0.0131
Std. Err.	(0.0320)	(0.0322)
P-value	(0.708)	(0.683)
Avg. treated units per cohort	2.1	2.1
Avg. control units per cohort	793.8	793.8
Avg. pre-periods per cohort	7.7	7.7
Avg. post-periods per cohort	8.5	8.5
Avg. eff. donors per cohort	141.4	136.0
Number of cohorts	50	50
Time: point estimate (min)	0.01	0.64
Time: bootstrap (min)	0.02	3.86
Time: total (min)	0.03	4.50

Note. ATT estimates from staggered SDID on a random 20% sample of restaurants (seed 42), which includes 105 reviewed and 757 never reviewed restaurants. Outcome: ln(Visits + 1). Control group: never-treated and not-yet-treated restaurants. Post-treatment window: weeks 0 to +8. Sample statistics are averages across cohorts. Both solvers minimize a ridge-penalized mean squared error subject to simplex constraints and differ only in the optimization algorithm. Standard errors come from 100 bootstrap replications with census-tract clusters, using the same draws for both solvers. The replications are parallelized across 64 cores. Timing was measured on a single machine.

C.2 Robustness Check: Two-Stage Difference-in-Differences Estimator

As a robustness check, we re-estimate our main results using the two-stage difference-in-differences (2S-DID) estimator of Gardner et al. (2024). Unlike conventional TWFE estimators under staggered treatment, the 2S-DID estimator avoids using already-treated observations as controls. Our main staggered SDID estimator likewise avoids these comparisons by constructing cohort-specific counterfactuals from not-yet-treated or never-treated restaurants. We use 2S-DID instead to assess whether our results depend on the weighting and matching procedure used to construct the SDID synthetic controls. (Gardner et al., 2024)

Let Y_{it} denote the outcome for restaurant i in week t, D_{it} the treatment indicator, and X_{it}

a vector of predetermined, time-varying controls. Treatment begins in the week of review:

$$D_{it} = \mathbf{1}\{t \geq T_i\},$$

where T_i is restaurant i's review week. In the first stage, we estimate restaurant and week fixed effects and the coefficients on X_{it} using untreated observations only:

$$Y_{it} = \alpha_i + \lambda_t + X_{it}'\gamma + \varepsilon_{it}, \qquad D_{it} = 0.$$

The fitted untreated outcome is

$$\widehat{Y}_{it}^{(0)} = \widehat{\alpha}_i + \widehat{\lambda}_t + X_{it}'\widehat{\gamma},$$

and the residualized outcome is

$$\widetilde{Y}_{it} = Y_{it} - \widehat{Y}_{it}^{(0)}.$$

In the second stage, we regress the residualized outcome on the treatment indicator:

$$\widetilde{Y}_{it} = \beta D_{it} + u_{it}.$$

Thus, β captures the ATT after removing variation associated with restaurant fixed effects, week fixed effects, and the predetermined controls X_{it}. The controls enter the first-stage residualization and are not included separately in the second stage. (Gardner et al., 2024)

We estimate the model separately for the direct and spillover effects, using the same restaurant samples as in our main specification. For the direct effect, reviewed restaurants are treated, and non-reviewed restaurants located more than 300 meters from every reviewed restaurant are controls. For the spillover effect, reviewed restaurants are excluded; non-reviewed restaurants within 300 meters of a reviewed restaurant are treated, and the same distant restaurants are controls. For eventually treated restaurants, we retain observations

from eight weeks before through eight weeks after treatment, while control restaurants are retained for the full sample period. We report the ATT pooled over post-treatment weeks 0–8, with standard errors from a cluster bootstrap with 100 replications at the census tract level.

Table C.2 reports four specifications. Columns (1)–(4) report the direct effect, and Columns (5)–(8) report the spillover effect. The first specification includes only restaurant and week fixed effects. The second additionally includes the first lag of $\ln(\text{Visits}+1)$ in the first-stage residualization. The third includes the first lag of the cumulative Yelp average rating, the first lag of the log cumulative Yelp review count, and their interaction. The fourth, which is our main two-stage specification, includes both the lagged outcome and the Yelp controls. Missing lagged Yelp values are replaced with the mean across all restaurants in the week of the lag. This specification follows Reimers and Waldfogel (2021), who study the causal effects of professional book reviews on Amazon sales-rank.

The results are qualitatively consistent with our main estimates. When we include the lagged outcome, the estimated direct effect (5.0%) is close to our main staggered SDID estimate (4.7%). Adding the Yelp controls raises it to 5.6%. The direct effects are larger without the lagged outcome (7.7% and 8.7%). The spillover effects range from 1.3% to 2.0% and are statistically significant at the 5% level in all four specifications. Table C.3 reports the placebo test that moves the review date one week earlier. The specification with only fixed effects shows a significant placebo direct effect of 5.6% ($p = 0.009$), which suggests that it picks up the growth of reviewed restaurants before publication. The placebo effects are small and not statistically significant in the specifications with the lagged outcome (2.3%, $p = 0.25$, and 1.6%, $p = 0.44$ with the Yelp controls). Overall, the results are robust to replacing the SDID weighting and matching procedure with the residualization-based 2S-DID estimator and to controlling for lagged outcomes and Yelp information.

Table C.2: Direct and Spillover Effects of Critics Reviews on Restaurant Visits, Two-Stage Difference-in-Differences

	Outcome: ln(Visits + 1)							
	Direct Effect				Spillover Effect			
Controls:	None (1)	Lag (2)	Yelp (3)	Lag + Yelp (4)	None (5)	Lag (6)	Yelp (7)	Lag + Yelp (8)
ATT	0.0771	0.0498	0.0874	0.0562	0.0199	0.0143	0.0188	0.0133
Std. Err.	(0.0132)	(0.0101)	(0.0137)	(0.0100)	(0.0072)	(0.0056)	(0.0073)	(0.0056)
P-value	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(0.006)	(0.010)	(0.010)	(0.018)
Sample statistics								
Treated restaurants	558	547	547	547	6550	6454	6454	6454
Control restaurants	3750	3750	3750	3750	3750	3750	3750	3750
Number of cohorts	61	60	60	60	61	60	60	60
First-stage observations	236689	232858	232858	232858	279667	274441	274441	274441
Treated post-publication observations	4661	4562	4562	4562	56567	55703	55703	55703
Bootstrap clusters	426	426	426	426	420	420	420	420

Note. ATT is the average treatment effect on the treated from two-stage difference-in-differences (imputation estimator of Gardner, 2022): the first stage estimates restaurant and week fixed effects on untreated observations only, and the ATT is the average difference between the observed and the imputed untreated outcome over weeks 0 to 8 after publication. Observations more than 8 weeks before or 8 weeks after publication are not used. Standard errors are based on 100 bootstrap replications that resample census tracts and repeat both stages. p-values in parentheses. Column headers: None = no control variables; Lag = lagged outcome; Yelp = lagged Yelp rating, lagged log number of Yelp reviews and their product (missing lagged values replaced by the mean over all restaurants in the week of the lag); Lag + Yelp = both (main specification). Sample statistics: restaurants with at least one treated observation, restaurants never treated, number of treatment cohorts, observations used in the first stage (untreated observations) and treated observations in weeks 0 and later.

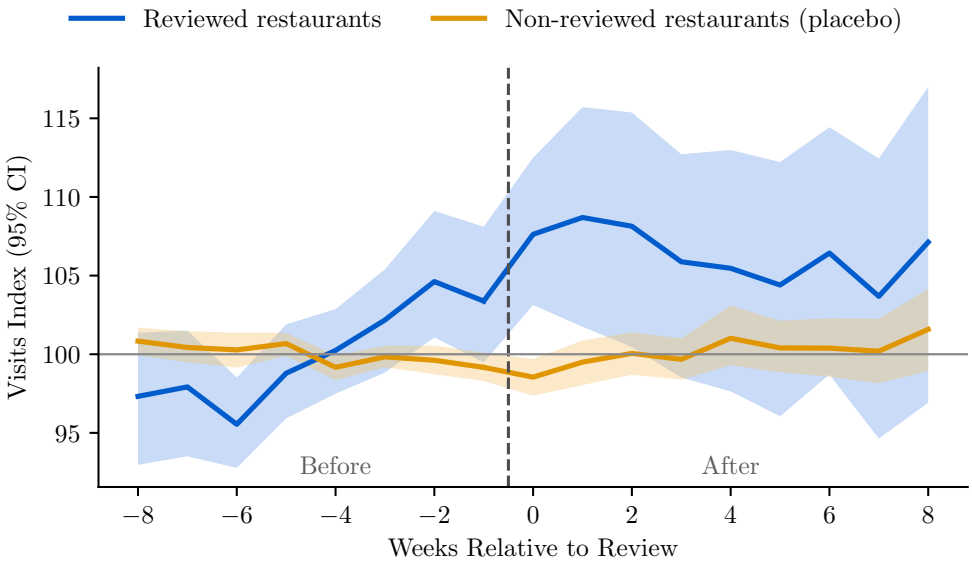
Table C.3: Placebo Test: Review Date Moved 1 Week Earlier, Two-Stage DiD

	Outcome: ln(Visits + 1)					
	Direct Effect			Spillover Effect		
	Actual (weeks 0–8) (1)	Actual (week 0) (2)	Placebo (week 0) (3)	Actual (weeks 0–8) (4)	Actual (week 0) (5)	Placebo (week 0) (6)
Panel A: controls Lagged outcome and Yelp controls						
ATT	0.0562	0.0599	0.0159	0.0133	0.0160	0.0051
Std. Err.	(0.0100)	(0.0183)	(0.0204)	(0.0056)	(0.0086)	(0.0083)
P-value	(< 0.001)	(0.001)	(0.437)	(0.018)	(0.062)	(0.541)
Post-publication weeks in the average	8	1	1	8	1	1
Panel B: controls No controls						
ATT	0.0771	0.0800	0.0559	0.0199	0.0203	0.0129
Std. Err.	(0.0132)	(0.0184)	(0.0215)	(0.0072)	(0.0088)	(0.0085)
P-value	(< 0.001)	(< 0.001)	(0.009)	(0.006)	(0.021)	(0.131)
Post-publication weeks in the average	8	1	1	8	1	1
Panel C: controls Lagged outcome						
ATT	0.0498	0.0629	0.0230	0.0143	0.0167	0.0051
Std. Err.	(0.0101)	(0.0187)	(0.0198)	(0.0056)	(0.0085)	(0.0083)
P-value	(< 0.001)	(< 0.001)	(0.246)	(0.010)	(0.050)	(0.541)
Post-publication weeks in the average	8	1	1	8	1	1
Panel D: controls Yelp controls						
ATT	0.0874	0.0756	0.0423	0.0188	0.0192	0.0124
Std. Err.	(0.0137)	(0.0184)	(0.0224)	(0.0073)	(0.0088)	(0.0085)
P-value	(< 0.001)	(< 0.001)	(0.059)	(0.010)	(0.028)	(0.142)
Post-publication weeks in the average	8	1	1	8	1	1

Note. ATT is the average treatment effect on the treated from two-stage difference-in-differences (imputation estimator of Gardner, 2022): the first stage estimates restaurant and week fixed effects on untreated observations only, and the ATT is the average difference between the observed and the imputed untreated outcome over weeks 0 to 8 after publication. Observations more than 8 weeks before or 8 weeks after publication are not used. Standard errors are based on 100 bootstrap replications that resample census tracts and repeat both stages. p-values in parentheses. In the placebo columns the publication date is moved 1 week earlier, so the post-period of the placebo is week 0 before the real publication; weeks on or after the real publication date are never used. The middle column of each effect is the actual effect measured over week 0 after the real publication, so that the actual effect and the placebo average the same number of weeks. The first column of each effect is the main estimate (weeks 0 to 8). Panel A uses the lagged outcome and the Yelp controls; the other panels use the other specifications of the first stage.

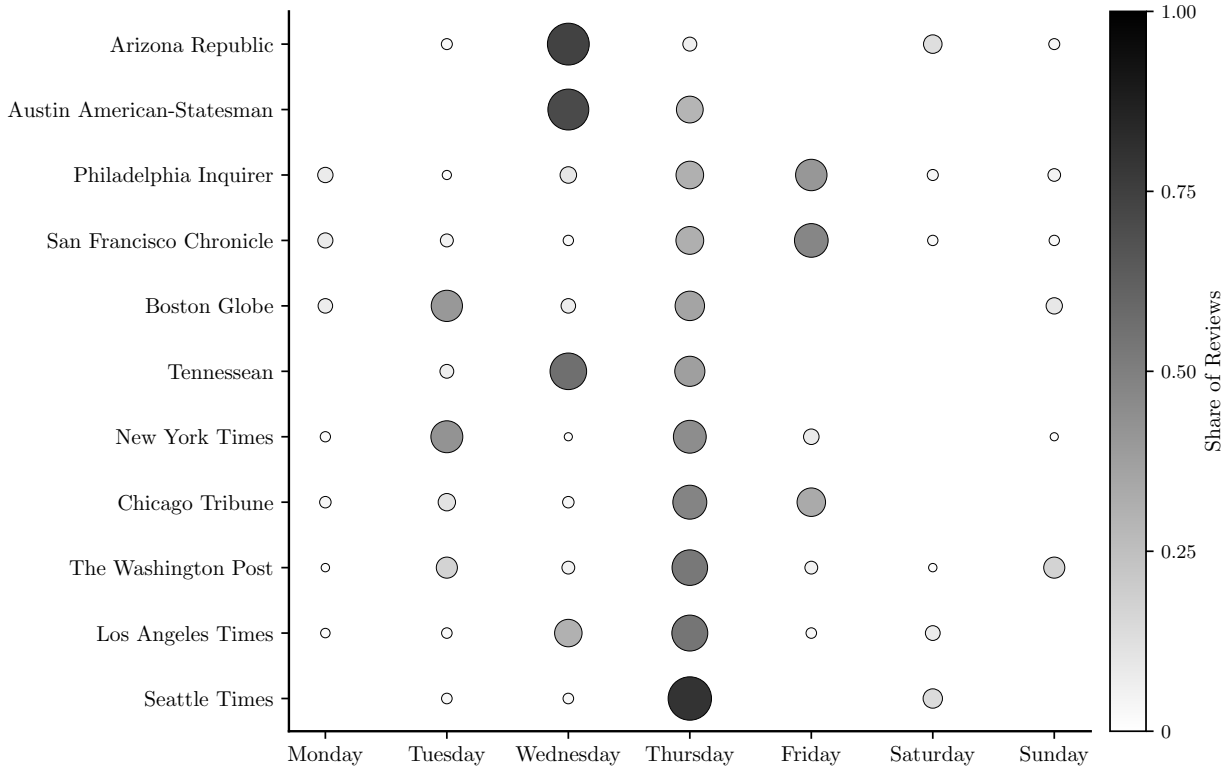
D Additional Tables and Figures

Figure D.1: Average Weekly Visits Around the Time of Review



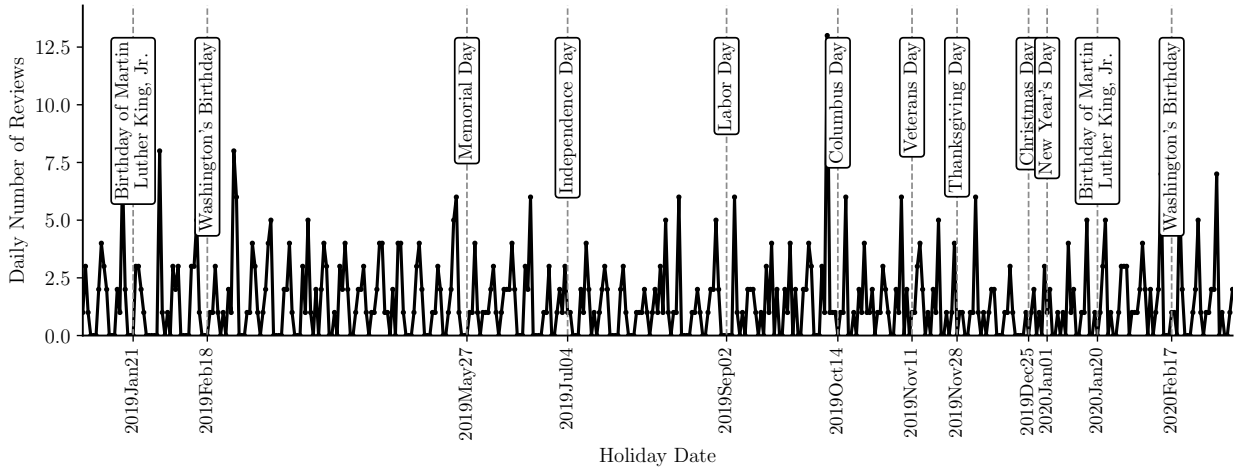
Note. The plot shows an index of average weekly visits to restaurants in the 8 weeks before and 8 weeks after receiving a review (week 0). Each line is divided by its own average over the 8 weeks before the review, which is set to 100. The first line shows reviewed restaurants. The second line shows non-reviewed restaurants in the same census tracts, assigned a placebo review date drawn from the empirical distribution of actual review dates. Shaded bands are 95% confidence intervals, computed by bootstrapping over census tracts. The figure shows that reviewed restaurants experience an increase in visits in the weeks leading up to the review, while no such pattern is observed for the placebo group.

Figure D.2: Share of Reviews by Publication and Day of Week



Note. The plot shows the share of reviews posted by different publications on each day of the week. The size and color of each dot is proportional to the share of reviews on that specific day for each publication. For example, a larger and darker dot on a specific day suggests a higher share of reviews for that publication on that day. The data includes all reviews posted during the analysis period (from January 2019 to mid-March 2020).

Figure D.3: Daily Review Volume with U.S. Federal Holiday Indicators



Note. The figure shows the total number of critic reviews published each day during the analysis period (from January 2019 to mid-March 2020), Dashed vertical lines mark U.S. federal holidays. The figure is intended to assess whether critics systematically alter their review behavior around federal holidays, and suggests no clear such pattern.

Table D.1: Robustness: Number of Pre-Treatment Weeks Used to Match, SDID

	Outcome: ln(Visits + 1)				
Matched pre-weeks:	8	9	10	11	12
	(1)	(2)	(3)	(4)	(5)
Panel A: Direct effect					
ATT	0.0467	0.0472	0.0483	0.0547	0.0532
Std. Err.	(0.0126)	(0.0125)	(0.0129)	(0.0129)	(0.0133)
P-value	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Avg. pre-periods	7.7	8.5	9.4	10.2	11.1
Avg. eff. controls	1829.2	1735.5	1619.9	1470.9	1390.7
Panel B: Spillover effect					
ATT	0.0186	0.0185	0.0195	0.0188	0.0185
Std. Err.	(0.0069)	(0.0069)	(0.0068)	(0.0067)	(0.0066)
P-value	(0.007)	(0.007)	(0.004)	(0.005)	(0.005)
Avg. pre-periods	7.7	8.5	9.4	10.2	11.1
Avg. eff. controls	4620.7	4552.4	4474.5	4428.4	4266.8

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Column headers show the number of weeks before publication used to build the weights. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort).

Table D.2: Placebo Test: Review Date Moved 1 Week Earlier, SDID

	Outcome: ln(Visits + 1)					
	Direct Effect			Spillover Effect		
	Actual (weeks 0–8) (1)	Actual (week 0) (2)	Placebo (week 0) (3)	Actual (weeks 0–8) (4)	Actual (week 0) (5)	Placebo (week 0) (6)
Panel A: control group Both						
ATT	0.0467	0.0529	0.0226	0.0186	0.0191	0.0088
Std. Err.	(0.0126)	(0.0174)	(0.0202)	(0.0069)	(0.0088)	(0.0083)
P-value	(< 0.001)	(0.002)	(0.265)	(0.007)	(0.030)	(0.286)
Post-publication weeks in the average	8	1	1	8	1	1
Panel B: control group Never						
ATT	0.0494	0.0528	0.0257	0.0204	0.0181	0.0087
Std. Err.	(0.0127)	(0.0172)	(0.0202)	(0.0068)	(0.0087)	(0.0084)
P-value	(< 0.001)	(0.002)	(0.205)	(0.003)	(0.038)	(0.301)
Post-publication weeks in the average	8	1	1	8	1	1

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. In the placebo columns the publication date is moved 1 week earlier, so the post-period of the placebo is week 0 before the real publication; weeks on or after the real publication date are never used. The middle column of each effect is the actual effect measured over week 0 after the real publication, so that the actual effect and the placebo average the same number of weeks. The first column of each effect is the main estimate (weeks 0 to 8). Control group Both: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort). Panel B uses *Never*: restaurants never treated during the sample period.

Table D.3: Spillover Effect by Distance to the Reviewed Restaurant: 4 Distance Groups, SDID

	Outcome: ln(Visits + 1)							
	Control group: Both				Control group: Never			
Distance to the reviewed restaurant:	0-99 m	99-170 m	170-235 m	235-300 m	0-99 m	99-170 m	170-235 m	235-300 m
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.0191	0.0277	0.0192	0.0107	0.0203	0.0280	0.0188	0.0115
Std. Err.	(0.0105)	(0.0092)	(0.0098)	(0.0100)	(0.0105)	(0.0094)	(0.0095)	(0.0102)
P-value	(0.070)	(0.003)	(0.051)	(0.283)	(0.052)	(0.003)	(0.048)	(0.260)
Sample statistics								
Avg. treated units	26.8	27.1	26.6	27.0	26.8	27.1	26.6	27.0
Avg. control units	4237.2	4197.0	4202.8	4197.9	3750.0	3750.0	3750.0	3750.0
Avg. pre-periods	7.7	7.7	7.7	7.7	7.7	7.7	7.7	7.7
Avg. post-periods	8.4	8.4	8.4	8.4	8.4	8.4	8.4	8.4
Number of cohorts	60	60	60	60	60	60	60	60
Avg. eff. controls	2764.8	2741.8	2791.3	2586.0	2436.5	2448.3	2453.3	2294.6
Bootstrap clusters	411	397	400	398	411	397	400	398

Note. ATT is the average treatment effect on the treated from staggered synthetic difference-in-differences, aggregated across treatment cohorts as in Equation 19 and averaged over weeks 0 to 8 after publication. Each cohort is matched on the 8 weeks before publication. Standard errors are based on 100 bootstrap replications that resample census tracts. p-values in parentheses. Each column uses the non-reviewed restaurants in one of 4 equal-count groups of the distance to the reviewed restaurant whose first review defines the treatment (within 300 meters). Control group: *Both*: restaurants never treated and restaurants not yet treated (treated more than 8 weeks after the focal cohort); *Never*: restaurants never treated during the sample period. Sample statistics are averages across cohorts: treated and control units, matched pre-periods and post-periods, and the effective number of controls, $1/\sum_j \omega_j^2$, where ω_j are the unit weights.