Wheels to Meals: Measuring the Impact of Micromobility on Restaurant Demand

Abstract

Dockless shared micromobility services have grown substantially in recent years, but their impact upon consumer demand has remained largely unstudied. The authors estimate how the largest and fastest growing segment of this market – the dockless electric scooter ('e-scooter') sharing industry – impacts spending in one of the largest segments of the local economy, the restaurant industry. Using data covering 391 companies in 98 U.S. cities, the authors find that the introduction of e-scooters in a city significantly impacts restaurant spending, increasing treated individuals' spending by approximately 4.4%, driving incremental spending of at least $10.2 million annually across all cities that first allowed e-scooters to operate over summer 2018. Impact varies by restaurant subcategory, with a strong positive effect upon fast food restaurant spending, and an insignificant effect upon sit-down restaurant spending. E-scooter entry has a larger impact upon companies with higher revenues selling at lower prices. It facilitates discovery of new restaurants from prospective customers and repeat business from already-acquired customers. We infer an insignificant effect for non-restaurant in-store spending, implying the positive effects we observe in the restaurant industry are not offset by negative effects at local businesses outside of it.

Keywords: micromobility, sharing economy, electric scooters

Dockless shared micromobility has expanded substantially since its inception in September 2017. It is a shared transportation service in which companies make low-speed vehicles – electric scooters ('e-scooters') and electric bicycles – available to consumers in any approved location, and enable consumers to find and rent them on a short-term basis through mobile apps. We focus upon the e-scooter industry, which is the largest and fastest growing subsegment of the dockless shared mobility industry. Over 100 thousand e-scooters were deployed in the United States in 2019[1] and many more in the rest of the world.[2] E-scooters have become a popular form of last-mile transportation due to their convenience and ease of use for consumers, and their ability to help cities meet mobility, climate/emissions and equity goals with a limited need for dedicated infrastructure (City of Portland 2019, City of Washington D.C. 2018).

The research questions we study in this paper revolve around the impact of e-scooters on consumer demand in the restaurant category and what this implies for the local economy more generally:

1. When a city allows e-scooters to operate in it, what effect does this have upon spending in the restaurant category?

2. What types of cities, and what types of businesses, are most affected?

3. Are these effects offset by changes in local non-restaurant spending, or do they carry through to overall spending in the local economy?

If these effects are positive, then the beneficial impact of e-scooter programs on local GDP growth (and thus job creation and sales tax revenue) may make city legislators more amenable, on the margin, to e-scooter programs, and restaurants more active proponents of them as well. This would also have implications for e-scooter companies, who may look to leverage this information through partnerships and promotion opportunities.

Our intended contribution in this article is to more formally evaluate these research questions through an individual-level analysis of consumer purchase behavior, leveraging a unique credit card panel data set from Earnest Research, a leading data analytics company. Through this data set, we observe individual-level purchase behavior at all 391 companies in their coverage universe within the restaurant sector – fast food and sitdown restaurants – from January 2017 through February 2020.

It is not obvious, a priori, that we would observe significant effects. E-scooters have not become popular for supporting local economies. Indeed, a meta-analysis of publicly disclosed reports produced by cities about their e-scooter programs shows that the primary purposes for launching these programs were reducing traffic congestion and reducing carbon emissions. This implies that cities do not expect meaningful restaurant spending effects, and in turn, that e-scooter operators have not been marketing financial spillovers to cities as part of their proposals. As such, uncovering that e-scooter program introduction does have significant effects, showing that those effects flow through into changes in overall spending in the local economy, but vary significantly across cities and businesses, would have unexpected yet meaningful consequences for city legislators and e-scooter operators.

We analyze transaction data for 44,618 individuals making purchases in 98 cities, 49 of which we designate as “treatment cities,” with the remainder representing “control cities.” Treatment cities collectively represent every city that first allowed e-scooters to operate over the five-month period between June 2018 and October 2018, roughly corresponding to summer 2018, which was the first full summer after the inception of the e-scooter industry. Over 50% of all cities that ever allowed e-scooters to operate, from their inception in September 2017 through February 2020, did so over this one summer period. Control cities are similar to the treatment cities in terms of observable sociodemographic variables, but did not allow e-scooters to operate over the study period.

Leveraging the quasi-randomness of regulation-driven e-scooter program adoption over the summer 2018 period, our main analysis compares individual-level spending in treatment and control cities using a fixed effects regression model and coarsened exact matching to account for observed and unobserved individual-level confoundedness. We also run a number of placebo tests, robustness checks and additional analyses that support the exogeneity of our causal variable, ensure that our results are not sensitive to the exact specification that we choose, and rule out alternative explanations. Lastly, to evaluate whether or not the effects we observe in the restaurant sector are offset by spending patterns at non-restaurant local businesses, we run a supplementary analysis upon total non-restaurant in-store spending.

Our main result is that introducing an e-scooter program into a city does indeed generate significant positive economic spillover for the restaurant sector as a whole, increasing overall restaurant spending for treated individuals by 4.4%. However, this effect is heterogeneous by subsector, with a significantly positive effect upon spending at fast food restaurants such as McDonald's and Chick-fil-A, and a positive but statistically insignificant effect upon sitdown restaurants such as Olive Garden and Cracker Barrel. As such, our results suggest that e-scooter entry does not benefit all restaurants to the same extent. Importantly, we also do not find evidence that the increase in spending in the restaurant category is coming at the expense of non-restaurant local businesses – the impact upon non-restaurant in-store spending is positive but statistically insignificant. This suggests that the introduction of an e-scooter program increases spending in the local economy, and any compensatory decline in spending due to budget constraints arises in "non-local" spending categories (e.g., e-commerce).

Our results have other implications for the types of businesses and cities that would benefit most from e-scooter program operation. For example, spending lift is significant for businesses with high historical revenues, selling at lower average tickets. Spending is significantly lifted for both businesses that consumers have not purchased at before (i.e., aiding in the discovery of new restaurants), as well as those that consumers have visited before. Lastly, spending lift is significant for cities with medium-to-high young-to-middle aged populations, with no significant differences as a function of local climate or public transit infrastructure.

An examination of the reasons why individuals ride e-scooters supports these findings. We collected all publicly accessible reports, to the best of our knowledge, that were conducted by cities about their e-scooter pilot programs. 20 cities produced survey reports analyzing e-scooter activity within their city. Of these 20 reports, 15 surveyed e-scooter riders about trip purpose. While there was large variation in the framing of survey questions, they consistently reported that individuals ride e-scooters for hedonic reasons and more specifically, they often use e-scooters to ride to or from restaurants.

Figure 1 shows e-scooter trip usage survey results for seven major cities. All cities include trip purpose categories specific to the restaurant sector, and suggest that going to or from a restaurant is a common purpose for e-scooter riding. For example, Chicago notes that 42% of all e-scooter trips were to or from a restaurant (City of Chicago 2020), while in Seattle, Santa Monica and Milwaukee, 15%, 15.5% and 14.4% of respondents stated that the primary purpose of their last trip was dining (City of Milwaukee 2019, City of Santa Monica 2019, City of Seattle 2022). These figures may understate the total number of trips that entailed going to or from a restaurant if going to or from a restaurant was a secondary purpose. While we would expect that many of these restaurant visits would still have occurred even if e-scooters were not available, these results are nevertheless suggestive that the introduction of an e-scooter program affects restaurant spending.

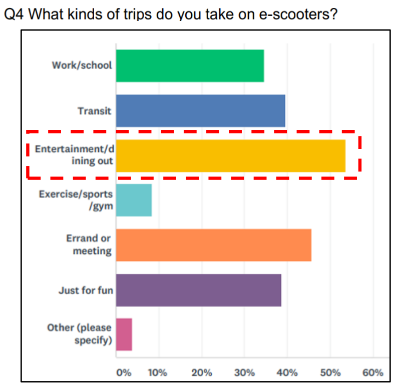
These reports also suggest that unlike other forms of shared mobility such as ridehailing, individuals typically ride e-scooters for hedonic purposes. A survey conducted in the city of Portland showed that 58% and 53% of respondents listed fun / recreation and social / entertainment as one of their three most common uses, respectively (City of Portland 2020). Respondents to a similar survey conducted by the city of Atlanta showed that social recreation was the most popular trip purpose, with 61% of all respondents listing it as one of the top two destinations when riding e-scooters (City of Atlanta 2019). 96% of e-scooter riders in the city of Austin included recreation as one of their trip purposes, with results in most other cities conveying the same substantive message. These findings are consistent with the heterogeneity in our results across restaurant subcategories, and may be suggestive of a mechanism akin to individuals enjoying an unplanned trip to grab a quick meal or drink with their friends, which would imply a larger effect at fast food restaurants than sitdown restaurants, at lower average order values.

The results are economically meaningful. We estimate that the average uplift in spend over a one-year period due to the introduction of an e-scooter program was approximately \$163.8 per e-scooter allowed across the subset of cities we study, a 4.94% increase relative to what would have been spent if e-scooters were not adopted.

Our paper joins a growing literature studying the effects of growth in the sharing econ-

Figure 1: E-scooter trip usage survey results for selected cities

Alexandria VA



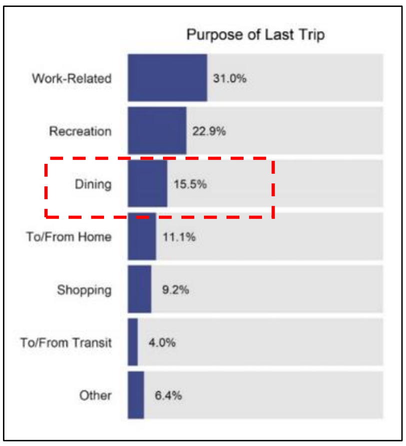
Seattle WA

Chicago IL

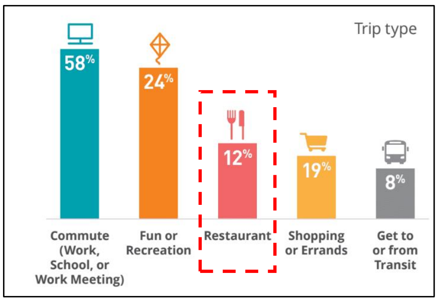
Table 7: Trip Purpose for E-scooter Use*

	Commute	Go To or From Transit	Go To or From School	Social/ Entertainment	Go To or From Restaurant
More than 1x per day	75%	49%	10%	32%	30%
Daily	70%	46%	11%	35%	31%
3-6x per week	60%	50%	6%	41%	40%
1-3x per week	38%	43%	7%	50%	47%
Occasionally, but < once per week	23%	32%	3%	58%	48%
I've only ridden once	10%	12%	1%	39%	21%
Overall	30%	34%	4%	50%	42%
Visitors	N/A	36%	N/A	41%	33%

Santa Monica CA



Portland OR



Milwaukee WI

What is the most frequent reason you've ridden a dockless scooter?

	1 trip	2+ trips	All
Connecting to transit (bus/streetcar)	2.1%	1.7%	1.6%
Riding for fun or recreation	50.1%	24.4%	28.6%
Running errands or shopping	2.5%	5.0%	4.6%
Traveling to/from a restaurant	11.2%	15.0%	14.4%
Traveling to/from a work-related meeting or appointment	4.4%	6.6%	6.2%
Traveling to/from entertainment	19.3%	26.4%	25.2%
Traveling to/from school or campus	1.7%	3.4%	3.6%
Traveling to/from work	2.9%	15.4%	13.4%
Other	5.8%	1.6%	2.3%

San Antonio TX

How did you use e-scooters/e-bikes? (Select all that apply)

ANSWER CHOICES	RESPONSES
To get to a shopping destination	40.73%
To get to a restaurant	56.95%
For sightseeing	70.20%
For recreation	58.61%
For transportation	69.21%
Total Respondents: 302	

Note: Figures and tables are obtained from the following survey reports: City of Alexandria (2019), City of Seattle (2022), City of Chicago (2020), City of Santa Monica (2019), City of Portland (2020), City of Milwaukee (2019), City of San Antonio (2019). Red boxes overlaid by the authors highlight data specific to the restaurant category.

omy, with Airbnb being the company most studied. Zervas, Proserpio, and Byers (2017) study Airbnb's entry into the state of Texas and quantify its economic impact on the hotel industry. Basuroy, Kim, and Proserpio (2020) study Airbnb's effect upon the restaurant industry, while Barron, Kung, and Proserpio (2021) analyze its effect upon housing affordability. These papers are similar to ours in that they seek to quantify Airbnb's impact upon a financially oriented measure of interest (e.g., hotel or restaurant revenue, home prices, and housing rental rates). Our paper is different and complementary to these papers in three important ways: we study the impact of (1) the e-scooter category (2) upon individual-level consumer spending (3) across the restaurant category, and its underlying subcategories (as well as its complement, local non-restaurant spending, as a supplementary analysis). Outside of these papers, there are no extant policy evaluation studies of the sharing economy, let alone micromobility, to the best of our knowledge.

The rest of the paper is organized as follows. In the next section, we summarize the data we use to perform the analysis. We then develop our model and discuss its identification, provide the main empirical results, and outline the robustness checks that we performed before concluding with a discussion of the results and future work.

DATA

Our main data source is individual-level transaction data from Earnest Research, a leading data analytics firm that has access to de-identified credit and debit card transactions at the daily level. We obtain purchase data for all 391 companies tracked by Earnest Research in the restaurant category for our main analysis. Each company is further subcategorized as a fast food restaurant (222 companies) or a sitdown restaurant (169). Table 1 contains summary statistics of this data. It shows, for example, that 76.5% of the individuals within the panel visited a fast food restaurant at least once over the observation window, that there are an average of 2.05 purchase-weeks per panel member in a given month in the fast food restaurant category, and that individuals spend $23.75 in the fast food restaurant category each purchase-week, on average.

We also obtain transaction data at companies in four other categories – restaurant deliv-

Table 1: Summary Statistics for Food Spending per Customer by Category

	Active Panel Members		Average Purchase-Week Frequency per Month		Spend per Purchase-Week ($)	
Category	Count	Share of Panel	Mean	St. Dev	Mean	St. Dev
Total Restaurants	76,463	78.8%	2.167	0.429	30.95	26.2
Fast food	74,232	76.5%	2.047	0.410	23.75	22.61
Sitdown	30,567	31.5%	0.453	0.09	40.12	21.9

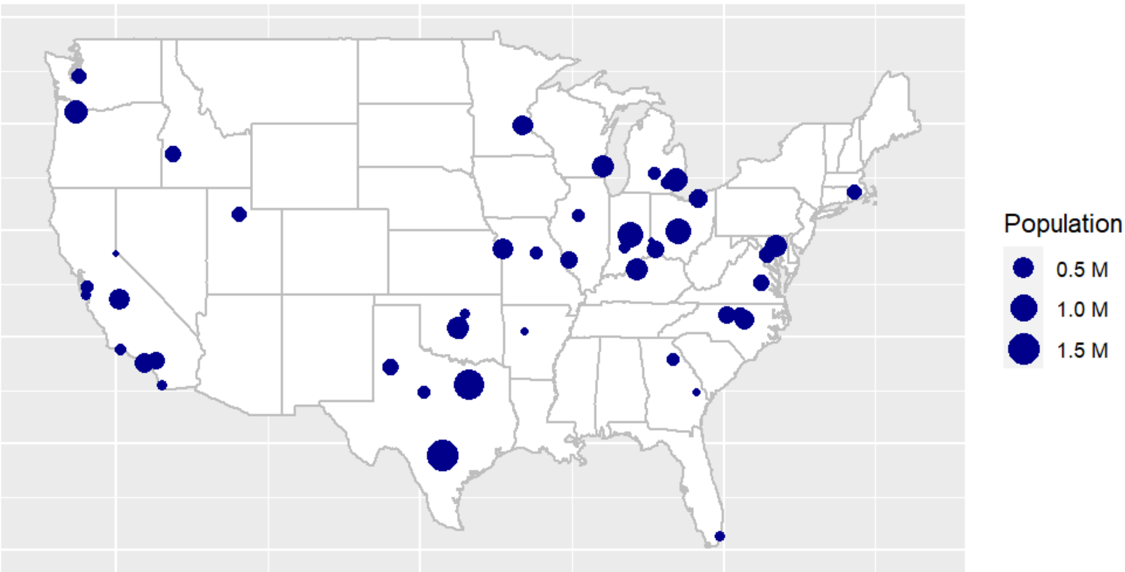
Note: Active panel members indicates people who made a purchase at least once during the observation period. Count is the number of active panel members, and share of panel represents the proportion of active panel members out of the full panel. Average purchase-week frequency per month is the total number of purchase-weeks per member-month, averaging across all member-months. Spend per purchase-week is spend amount per week, averaging across all purchase-weeks. All statistics are computed over the full observation period.

ery (31 companies), online grocers (35), air travel (13), and mass transit (16) – which we use to conduct a variety of supplementary analyses. In Web Appendix A, we list all companies in the restaurant category, as well as all companies in these four non-restaurant categories.

Through this dataset, we also observe the location at the city-level associated with most in-store transactions. We use this location data to subset down to individuals making purchases primarily in 98 cities from January 1 2017 through February 29 2020. As mentioned in the previous section, these 98 cities consist of 49 "treatment cities," representing all cities that launched e-scooter programs over summer 2018,[3] which we match against 49 "control cities" that are similar to our treatment cities in terms of sociodemographics, but did not launch e-scooter programs over the observation period. A table containing all treatment and control cities is available in Web Appendix B.

Cities launching e-scooter programs over this period vary significantly in terms of observable 2018 demographics, including population (e.g., South Lake Tahoe CA and San Antonio TX have populations of 22 thousand and 1.5 million, respectively), income (e.g., Oxford OH and Arlington VA have median household incomes of $27 thousand and $120 thousand), and climate (e.g., St. Paul MN and Coral Gables FL have average temperatures of 44 and 76, respectively). These cities also vary significantly in terms of their geographical location. Through Figure 2, which plots the location of all cities launching e-scooter programs over summer 2018, it is visually evident that these cities (and their population sizes) are broadly distributed across the country. This variability enables us to identify heterogeneous treatment effects as a function of city characteristics, expanding the relevance of our analysis to policymakers in other cities looking to understand what effect launching an e-scooter program may have in their city, given their city's observable demographics.

Figure 2: Map of Cities Launching E-scooter Programs



Note: Dots represent 49 cities launching e-scooter programs from June 2018 to October 2018. The area of each dot is proportional to the population of the associated city.

Our unit of analysis is an individual over a weekly unit of time. Although we could in theory marginalize our data across customers and perform an aggregate city-level analysis as in Zervas, Proserpio, and Byers (2017), we proceed at the individual-level instead for two reasons. First, even though e-scooters have become more popular in recent years, they are still a small proportion of overall transportation. As such, the effects that we would expect to see at the aggregate-level would be obfuscated by other sources of variation that are unrelated to the phenomenon that we are studying. Second, an individual-level analysis provides us with greater visibility into the effects that an e-scooter program has, and how those effects vary across customers.

The observation period for our main analysis is comprised of a pre-treatment period, over which both treatment and control cities do not have e-scooter programs, and a post-treatment period, over which treatment cities have e-scooter programs but control cities do not. Our study period is 38 months long, from January 2017 to February 2020 (before the onset of the COVID-19 pandemic).

We pre-process the data, filtering down to individuals living within the target cities of interest, dropping transactions generated outside of modal home cities (e.g., vacation-related spending), and removing inactive accounts. For preprocessing details, see Web Appendix C. After pre-processing, there were 95,479 individuals in treated cities and 76,889 individuals in control cities.

Our goal is to infer the population-level impact of introducing an e-scooter program on restaurant spending. However, our transaction data is a subsample – we do not have data for every individual in the cities we study. As such, while the Earnest Research panel is very large (approximately 3% of all credit and debit transactions in the U.S.), there may still be concerns regarding the representativeness of the patterns in restaurant spending of our panel members relative to those in the broader population. We empirically assess the external validity of our panel data by obtaining population-level data for all publicly-traded, US-listed restaurant brands that disclose sales measures. 36 restaurant brands met these criteria, including Burger King, Domino's Pizza, KFC, McDonald's, Starbucks, and Shake Shack. These 36 brands collectively represent 55.4% of total panel sales in our 391-company coverage universe over the observation period.

These public disclosures provide us with "gold standard" company-specific population-level panel spending data which we can compare our panel data to. We obtain the empirical correlation between the population-level sales disclosures and the corresponding panel-level sales figures over all available quarterly observations from the first calendar quarter of 2018 through the third calendar quarter of 2021. The sample average and median of these 36 correlations were 91.8% and 98.6%, respectively, and the interquartile range was 92.3% to 99.1%. While not perfect, these statistics are nevertheless quite high, supporting the notion that restaurant spending patterns as observed through our panel are reflective of population-level trends. For a list of all 36 companies and their population-panel correlations, as well as a more detailed discussion of the representativeness analysis, see Web Appendix D.

MODEL DEVELOPMENT

Our goal is to study the causal relationship between e-scooter program introduction and individual-level restaurant spending. As mentioned in the introduction, our main analysis encodes e-scooter program introduction as a binary measure, controlling for unobserved individual-level heterogeneity and common demand shocks that might affect both restaurant spending and e-scooter entry through a two-way fixed effects regression model.

Letting $\log(Spend_{it} + 1)$ represent the log of the total amount spent (plus one) for each consumer i in week t, and letting $ScooterEntry_{c(i)t}$ denote a binary variable equal to 1 if an e-scooter program had already begun in city c by week t and 0 otherwise, we assume the following:

$$\text{(1)} \qquad \log(Spend_{it} + 1) = \beta_0 + \beta_1 ScooterEntry_{c(i)t} + \phi_i + \xi_t + \theta_c t + \epsilon_{it},$$

where β_1 is the coefficient of interest, measuring how e-scooter program introduction affects (log) spending in the restaurant category, ϕ_i and ξ_t represent individual-specific fixed effects and week fixed effects respectively, $\theta_c t$ is a city-specific linear time trend, and ϵ_{it} is an error term.

ϕ_i control for time-invariant individual-specific characteristics such as static but unobserved preferences, typical transit patterns, and sociodemographic factors such as gender, age, and educational background, as well as city-specific characteristics such as population density, walkability, public transit availability, the size of the downtown area, and other factors that effectively do not change over the observation period. ξ_t control for common macroeconomic events, seasonal trends, and other time-based factors that affect all cities. $\theta_c t$ control for city-specific baseline growth trends (Angrist and Pischke 2008, Goodman-Bacon 2021). For example, local GDP steadily grew and fell from 2012 to 2017 in San Antonio TX and Peoria IL, at annual rates of 6.7% and -3.3%, respectively U.S. Bureau of Economic Analysis 2019). These time-varying city-specific differences in baseline growth rates would not otherwise be controlled for by the aforementioned individual and time fixed effects, so omitting them could bias estimates of β_1. As we show after the main results, our results are robust to the functional form used to model these trends – we obtain the same substantive results assuming a quadratic or cubic trend in lieu of a linear one.

The causal impact of e-scooter program introduction is inherently an intention-to-treat (ITT) effect (Hernán and Hernández-Dıaz, 2012, Hernán and Robins, 2020). The launch of an e-scooter program, on its own, does not cause individuals to spend in the restaurant category – it is the riding of e-scooters after an e-scooter program is launched that does. In this sense, e-scooter riding is the treatment, while e-scooter program introduction is an encouragement to receive the treatment. Encouraging treatment and receiving treatment are not the same – individuals do not ride e-scooters every day after a new e-scooter program is launched – but launching an e-scooter program encourages individuals to ride by making riding possible.

In this article, we focus upon the ITT analysis and not on the impact of the treatment itself for relevance, actionability, interpretability, and concision. The ITT analysis is the most important aspect of our analysis from a public policy standpoint – local governments want to know what the economic implications are to introducing an e-scooter program in their city, as this has direct repercussions for local economic growth and sales tax revenue. Local governments can decide whether to launch an e-scooter program through legislation, making the ITT analysis more actionable (e-scooter riding cannot be directly manipulated). Finally, while the causal estimand of the ITT analysis is interpretable, the causal estimand of an IV regression of e-scooter riding on restaurant spending is a local average treatment effect that would be more difficult for policymakers to interpret.

Our geographical unit is the city, and not areas within each city, for similar reasons. In addition to lack of data availability – our purchase data is only available at the city-level – a city-level analysis is most relevant to policymakers. Policymakers decide whether or not to launch an e-scooter program at the city-level, and overall employment and tax revenue are driven off of city-level spending. Finally, there are additional endogeneity concerns stemming from possible dynamics within subregions of a city. As in Bertrand, Duflo, and Mullainathan (2004), collapsing the data to a higher level of aggregation allays such concerns.

Coarsened Exact Matching

A complicating factor in our empirical setting is that relatively few consumers ride e-scooters, given the fast-growing yet low absolute level of e-scooter adoption to date (rides occur in approximately 0.34% of panel member-weeks). Adjustment for pre-treatment variables can further improve the credibility of causal inferences (Imbens 2014) by lowering model dependence and improving estimation efficiency and stability. Therefore, in addition to matching treated cities to control cities, we match treated individuals to control individuals within treated and control cities, respectively, through coarsened exact matching (CEM; Iacus, King, and Porro 2012). Using CEM, we pair consumers with similar pre-treatment covariates but different local e-scooter availabilities. Our matching covariates are parameters summarizing each individual's baseline purchase propensity in the restaurant category over the pre-treatment period (McCarthy and Oblander 2021). After coarsening, we match and weight individuals who are similar as measured through these covariates and through the characteristics of the cities they live in, but different in terms of whether e-scooters operate or not in the city. After removing unpaired individuals, panel members with positive CEM weights consist of 14,908 ever-riders (i.e., individuals who rode an e-scooter at least one time) in 49 treatment cities and 29,710 individuals in 49 control cities. Hereafter, when we refer to individuals in treatment and control cities, we are referring to these individuals. Performing this procedure significantly improved pre-treatment covariate balance. For details of this procedure, see Web Appendix E. Our conclusions are the same in magnitude and direction when we do not use CEM – analogous results using unmatched data are provided in Table W 5 in Web Appendix H.

Identifiability of the Causal Effect of E-scooter Programs

We assume the decision to introduce and exact timing of e-scooter program introduction are exogenous after controlling for fixed effects and other covariates. We provide three arguments in this section for why we believe this is likely to hold – institutional context, parallel pre-trends, and placebo tests. We then provide a series of other tests and checks in the next section that further support the validity of our results.

Institutional context. If cities chose to launch e-scooter programs because they believed, using information unknown to us, that these programs would increase spending in the restaurant category, our estimate of β_1 from Equation 1 could overstate the true causal effect of e-scooter program introduction. One way of assessing this potential source of bias is by analyzing the reasons why cities launch e-scooter programs. To this end, we performed a study of all publicly disclosed announcements in cities launching e-scooter programs (see, for example, City of Atlanta 2019, City of Austin 2018, City of Minneapolis 2019, Lime & City of San Francisco 2018, City of Portland 2019, City of Santa Monica 2019). In these announcements, a variety of purposes were mentioned, including reducing traffic congestion and reducing carbon emissions, neither of which are directly related to restaurant category revenue. This data suggests that cities do not choose whether and when to launch e-scooter programs to increase restaurant sales.

Over 50% of all e-scooter entries in the United States over the 30-month period from the e-scooter industry's inception in September 2017 through February 2020 occurred during summer 2018. The large number of cities choosing to launch e-scooter programs over this time period is a byproduct of regulation, which act as another helpful source of identification. Summer 2018 was the first summer after e-scooter companies began commercial operations, and as such, e-scooter companies were primarily focused upon growing penetration and revenue. This made e-scooter companies not selective in terms of the cities they were willing to enter. The primary constraint facing the industry was regulatory in nature (i.e., whether cities would allow e-scooter companies to enter). While the first e-scooters were deployed in September 2017, e-scooter companies did so without approval from local governments, and regulatory frameworks governing e-scooter use did not exist. As such, many of these cities subsequently banned e-scooters from operating in their cities (Irfan 2018). By summer 2018, however, regulatory frameworks were successfully put in place, as was an industry-wide data recording standard enabling cities to systematically track e-scooter usage and deployment (e.g., vis a vis maximum e-scooter deployment quotas set by cities) known as the "Mobility Data Specification" (MDS). Summer temperatures also made it seasonally attractive to allow e-scooter companies into new cities. These factors drove an upward shock to e-scooter program introduction over summer 2018. Just 15 cities allowed e-scooters to operate over the prior 9 months, and another 29 cities did so over the subsequent 16 months. For additional details, see Web Appendix B.

In summary, institutional context makes strategic forward-looking behavior on the part of cities and e-scooter companies with respect to the restaurant category second-order in nature, if it exists at all. As we will show next, a series of statistical tests further support the causal validity of our specification.

Parallel pre-trends. The canonical parallel trends assumption is that the average outcome among the treated and control groups would have followed parallel trends if the treatment had not occurred. As in prior literature, we first evaluate this through a model-free visualization (Fisher, Gallino, and Xu 2019, Goldfarb, Tucker, and Wang 2022). In Figure 3, we plot average restaurant spending for treatment and control individuals prior to e-scooter entry. It is visually evident that average restaurant spending for both groups is roughly parallel over the pre-treatment period, even before we perform matching.

Figure 3: Average Restaurant Spend Prior to E-scooter Entry

Note: The dotted dark blue line represents the average restaurant spend per panel member ($) for treated individuals, while the solid light blue line represents the corresponding figure for control individuals. The x-axis range is January 2017 to May 2018, over which period both treated and control cities did not have active e-scooter programs.

Next, we perform a regression test to assess differences in trends between two groups over the pre-treatment period, as in Bronnenberg, Dubé, and Sanders 2020 and Fisher, Gallino, and Xu 2019. Consider the following regression equation:

$$\text{(2)} \qquad \log(Spend_{it} + 1) = \delta_0 + \delta_1 t ScooterAdopted_{c(i)} + \phi_i + \xi_t + \epsilon_{it}$$

$ScooterAdopted_{c(i)}$ is a binary variable equal to 1 if city c allows e-scooters to operate during the observation period and 0 otherwise; δ_1 represents the deviation from a common time trend for individuals in treatment cities relative to control cities, making δ_1 the coefficient of interest for this test. We run this regression over a pre-treatment window from January 2017 to May 2018, before e-scooters were introduced in any of the cities we study. As in Equation 1, $Spend_{it}$ represents total restaurant spending for customer i in week t. We infer that δ_1 is not significantly different from 0 at the 10% level (p= .19), so we cannot reject the null hypothesis of parallel pretrends.

Lastly, we test for pre-treatment trends by regressing our outcome variable upon dummy variables for the time relative to the treatment event, in addition to individual and time fixed effects (Roth et al. 2022, Goldfarb, Tucker, and Wang 2022):

$$\text{(3)} \qquad \log(Spend_{it} + 1) = \sum_{s=s^{\star}}^{0} \lambda_s Treat_i \mathbb{1}(Lag_{c(i)t} = s) + \phi_i + \xi_t + \epsilon_{it},$$

$Treat_i$ is an indicator variable equal to 1 if individual i is a part of the treatment group and 0 otherwise. $\mathbb{1}(Lag_{c(i)t} = s)$ is another indicator variable equal to 1 if at time t, the time until treatment ('lag') for the city c that individual i resides in is equal to s, and 0 otherwise, over the $s^{\star}$ weeks prior to treatment.

Our parameters of interest are λ_s, which measure differences in spending between treatment and control individuals at distinct lags s, residual of individual and time fixed effects. Estimates of λ_s that are significantly different from 0 imply violations of parallel trends at associated lags s. We set $s^{\star} = 32$, corresponding to an 8-month period prior to e-scooter entry. Regression results show that all but one of the resulting 33 λ_s coefficient estimates are not significantly different from 0 at the 95% level, which is almost exactly in line with the expected rate of Type 1 error. See Web Appendix F for more details.

Placebo tests. We conduct falsification checks with four placebo categories. Had our results been spuriously driven by unobserved local demand shocks that caused (a) cities and e-scooter companies to proactively launch e-scooter programs in advance of the shocks and (b) consumers to spend more in the restaurant sector during the shocks (e.g., a major sporting event such as Super Bowl), we would have also expected increased demand for

1. delivery services such as restaurant delivery (e.g., DoorDash) and grocery delivery (e.g., Instacart); and

2. transportation services such as air travel (e.g., Delta Airlines) and mass transit (e.g., Greyhound Bus)

As such, we can evaluate whether these alternative explanations are spuriously driving our results by re-running the regression from Equation 1 with dependent variables corresponding to these placebo categories.

Table 2: Placebo Tests

	Restaurant Delivery	Online Grocers	Air Travel	Mass Transit
	(1)	(2)	(3)	(4)
Scooter Entry	0.00088	0.01318	0.0041	-0.00099
	(0.01965)	(0.01488)	(0.00864)	(0.00623)
Individual FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Mean of Dep. Var.	0.247	0.230	0.154	0.064
R^2	0.186	0.213	0.059	0.065
No. of Obs.	4,389,825	1,437,645	4,590,795	1,479,060

Note: Results are obtained by estimating Equation 1, with dependent variables corresponding to the placebo categories shown in the top-most row of the table. Robust standard errors (in parentheses) are clustered by city; * p<0.05, ** p<0.01, *** p<0.001

The results in Table 2 show that all four placebo categories do not have statistically significant relationships with e-scooter entry, supportive of the hypothesis that our results are not driven by anticipatory behavior on the part of cities and e-scooter companies in advance of unobserved local demand shocks.

EMPIRICAL RESULTS

In this section, we report the results of our main empirical model and analyze the heterogeneity in these effects. We then provide a series of robustness checks before concluding with a discussion of future work.

Main Results

In Table 3, we present the results of estimating Equation 1, as well as the corresponding results when our dependent variable is equal to total spending in the fast food and sitdown subcategories. Standard errors in all of the following results are clustered by city, to allow for correlation of errors between individuals within a city.

Table 3: Estimates of the Effect of E-Scooter Entry on Restaurant Spending

	Dependent variable:		
	Total	Fast Food	Sitdown
	(1)	(2)	(3)
Scooter Entry	0.04424^{**}	0.03942^{**}	0.00905
	(0.01403)	(0.01385)	(0.00589)
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Mean of Dep. Var.	1.726	1.547	0.421
R^2	0.337	0.338	0.143
No. of Obs.	7,361,970	7,355,535	7,023,390

Note: Results are obtained from estimating Equation 1, with dependent variables corresponding to total restaurant spending (‘Total’) and spending within each restaurant subcategory. Robust standard errors (in parentheses) are clustered by city; * p<0.05, ** p<0.01, *** p<0.001

Table 3 shows that the positive impact of an e-scooter program on overall restaurant spending is both statistically ($p < .01$) and economically significant, but that this impact differs by category. The estimated impact on spending is positive and significant for the fast food category and positive but not statistically significant for the sitdown category. In the spirit of the aforementioned placebo tests, these results are also supportive of the hypothesis that our results are not an artifact of proactively launching an e-scooter program in advance of local demand shocks – the sort of shocks that naturally come to mind as drivers of such bias (e.g., sporting and music events) would also drive sitdown restaurant spending up, not just fast food spending.

Subgroup Analysis for Heterogeneous Treatment Effects

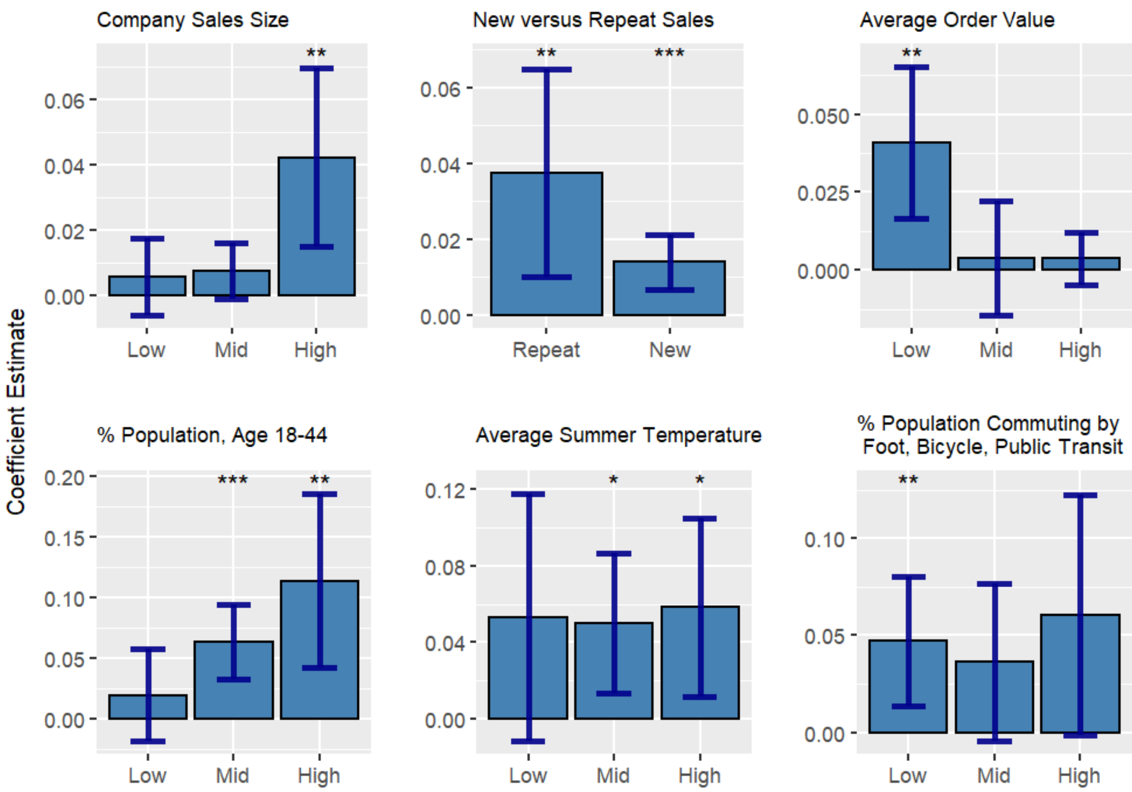
The impact of e-scooter deployment may be different for companies as a function of company-specific factors other than restaurant subcategory, and for cities as a function of city-specific factors. These effects are important to understand, especially for policymakers who may be contemplating the launch of an e-scooter program in their city, given their city's characteristics, so we study them next.

Specifically, we estimate how the causal impact of e-scooter program introduction varies as a function of (1) company sales over the pre-treatment period, (2) company average order value (AOV), (3) sales from new customers versus repeat customers, (4) share of the city's population aged 18 to 44, (5) average summer temperature in the city, and (6) share of the city's population commuting by foot, bicycle, and public transit. Figure 4 visualizes the results, which we describe in more detail next.

Company sales size. The effect of e-scooter entry may be different for large companies relative to small companies, as measured through historical sales. We assess this by categorizing companies into tercile-based groups per category based upon sales over the period prior to e-scooter entry, from January 2017 to May 2018. We then estimate Equation 1 when our dependent variable is equal to total spending in each of the resulting subgroups separately. The leftmost plot in the upper panel of Figure 4 shows that the impact of e-scooter entry is significant for large companies. In contrast, the effect of e-scooter entry is positive but insignificant at small and mid-sized companies, which could be suggestive of a form of double jeopardy (Sharp 2016).

New versus repeat sales. A related distinguishing factor is whether e-scooter program introduction lifts spending at businesses that consumers have not previously bought from, versus expanding spending from repeat customers. We measure "new sales" by summing spending each week across individuals at all companies that have not purchased at in any previous week. Conversely, we measure "repeat sales" by summing spending each week

Figure 4: Heterogeneous Effect Analysis



Note: Each bar represents the causal effect of e-scooter program upon (log transformed) restaurant spending. Dark blue vertical lines represent 95% confidence intervals for each coefficient estimate. 'High', 'Mid', and 'Low' along the x-axis represent the top 0-33%, 33%-66%, 66%-100% percentiles (i.e., tercile splits) by company sales size, average order value, share of age 18-44 population, average summer temperature, and share of commuting by walk, bicycle, and public transit. 'New Versus Repeat Sales' represents coefficient estimates when our dependent variable is equal to total new customer spending (i.e., the sum of all spending made by customers at restaurants they had not previously purchased at before) versus repeat customer spending (i.e., total restaurant spending minus new customer spending). * $p<0.05$, ** $p<0.01$, *** $p<0.001$

across individuals at all companies that have made at least one purchase at in any previous week. The results, depicted in the middle plot of the upper panel in Figure 4, show that the impact of e-scooter program introduction is positive and statistically significant for both new and repeat spending. While the difference between these groups is not statistically significant, our point estimate for the effect upon repeat sales is more than twice as large as the effect upon new sales. This implies that e-scooter program introduction drives both consumer discovery of new restaurants, and expansion of business with repeat customers, with the latter effect seeming to be the larger of the two.

Average order value. We assess heterogeneity in the treatment effect as a function of AOV by stratifying companies into tercile-based groups based upon historically observed average spend given purchase for purchases occurring before e-scooter entry. We then estimate Equation 1 when our dependent variable is equal to the sum of spending at all companies within each group separately. The third plot in the upper panel of Figure 4 shows that the impact of e-scooter program is largest for companies selling at lower price points, and is statistically insignificant for companies selling at middle or high price points. This is supportive of the behavioral narrative suggested in the Introduction section, as individuals enjoying a quick meal or drink with friends would likely spend less than the unconditional average spend at a restaurant.

Age. We next categorize cities into tercile-based groups based on the share of the population whose age is between 18 and 44, as survey reports summarizing e-scooter activity across the U.S. show that more than half (50% to 73%, depending upon the city) of e-scooter riders are under the age of 40 (NACTO 2019). We then estimate Equation 1 on all individuals whose home city is in each age group separately. The first plot in the lower panel of Figure 4 shows that the impact of e-scooter entry is statistically significant in cities with a middle-to-high proportion of young-to-middle aged individuals. In contrast, the effect of e-scooter entry is small and statistically insignificant in cities with a low proportion of young-to-middle aged individuals. This implies that cities with elderly populations may not see significant financial spillovers from the launch of a new e-scooter program, likely due to more limited adoption by the local population.

Summer temperature. We categorize cities into tercile-based groups based on the summer temperature. As with age demographics, we then assign individuals to groups based on their modal home city location, and run separate regressions on each of the subgroups. The second plot in the lower panel of Figure 4 shows no clear directional pattern in estimated effects as we move from low to high temperatures, and the differences across tercile groups are not statistically significant. Differences in absolute statistical significance appear to be driven primarily by variation in statistical power. As such, our results suggest that lift is significant in all climates, and that it is difficult to draw more precise conclusions about climate effects because of low statistical power.

Public transit infrastructure. We may expect the impact of e-scooter program introduction to vary with the amount of public transit infrastructure that exists in a city. On the one hand, micromobility has a high degree of complementarity with public transit – e-scooter survey reports note that individuals often ride e-scooters to or from other forms of public transit (e.g., using an e-scooter to get to a subway station) – which may lead to larger effects in cities with high public transit infrastructure. On the other hand, e-scooters are also a substitute for public transit in areas where public transit infrastructure does not exist, which could imply larger effects in cities with low existing public transit infrastructure.

We measure public transit infrastructure through the proportion of the population commuting by foot, bicycle, and public transit. As with age demographics and summer temperature, we segment cities into tercile-based groups based upon this measure, use this segmentation to in turn segment individuals, then estimate Equation 1 separately for each group of individuals. While we only infer a significant positive impact for the low public transit infrastructure tercile group, the rightmost plot in the lower panel of Figure 4 shows no clear trend in estimates across tercile groups, and statistically insignificant differences between them. As with temperature, differences in absolute statistical significance once again appear to arise largely from variation in statistical power. This suggests that the introduction of an e-scooter program benefits cities with all levels of public transit infrastructure, and that low statistical power makes it infeasible to draw more fine-grained inferences than this.

In summary, e-scooter program introduction primarily drives sales lift for large companies selling at low price points, and aid both discovery of new restaurants and expansion of business at existing ones. From a city perspective, the launch of an e-scooter program has a larger effect in cities with a medium-to-high proportion of young-to-middle age citizens, and appears to benefit cities of all climates and levels of public transit infrastructure. These results suggest that restaurants more likely to be beneficiaries of the launch of new e-scooter programs have a financial incentive to lobby in support of these programs. Conversely, city legislators looking to support their local economies may be more inclined, on the margin, to consider e-scooter programs, but only if their cities' demographics imply that doing so will bring about that support.

All Spending on Local Businesses Except Restaurant Categories

One concern is that consumers may increase their spending at restaurants but decrease it in other segments of the local economy because of budget constraints. If this were the case, then the lift figures we estimate may overstate the actual overall increase in spending on the local economy after an e-scooter program is introduced. To evaluate this, we obtain all spending tagged by our data provider as being in-store, regardless of whether that spending was at a restaurant or not. We then subtract total restaurant spending from total in-store spending to derive a measure of total local non-restaurant spending. Finally, we estimate Equation 1 using total local non-restaurant spending as the dependent variable. The results in Table 4 show that the impact of e-scooter program entry on total local non-restaurant spending is positive but not significantly different from 0. This implies that the lift in restaurant sales due to e-scooter program entry is not offset by a compensatory decline in local non-restaurant sales, even if the latter effect is, in aggregate, not significant. To the extent that budget constraints drive a decrease in spending, then, our results would suggest that this decline is in a "non-local" spending category, such as e-commerce spending.

Robustness Checks

We now turn to additional robustness checks that further support the validity of our substantive conclusions. We first use a permutation test, then discuss variations of our focal model

Table 4: Spending on Local Businesses across All Categories

	Dependent variable:	
	All but Restaurants	Restaurants
	(1)	(2)
Scooter Entry	0.03220	0.04424^{**}
	(0.01759)	(0.01403)
Individual FE	✓	✓
Time FE	✓	✓
Mean of Dep. Var.	2.588	1.726
R^2	0.387	0.337
No. of Obs.	7,370,550	7,370,220

Note: Robust standard errors (in parentheses) are clustered by city; * p<0.05, ** p<0.01, *** p<0.001

specification.

Permutation tests. As in Christian and Barrett (2017) and Barron, Kung, and Proserpio (2021), we carry out a permutation test to evaluate whether our results are a byproduct of spurious common trends. In the original data set, each individual is associated with a binary vector representing whether e-scooters had already been deployed in her city. We randomly permute this vector across individuals, while leaving her restaurant spending and other data unpermuted. We then re-run Equation 1 on the permuted data, obtain the coefficient estimates and corresponding t-statistic values associated with e-scooter program introduction, β_1, and repeat this procedure 200 times.

Permutation breaks the association between restaurant spending and e-scooter program introduction, while preserving aggregate-level time trends in spending across customers. Through this procedure, we obtain a distribution of the significance of their association under the null hypothesis that there is no association between the two. If e-scooter program introduction significantly impacts restaurant spending, the t-statistic for β_1 in the original unpermuted data would be significantly different from its associated null distribution.

As expected, the t-statistic for β_1 using the permuted data is statistically insignificant, implying that our original estimate is indeed significantly different from what we would have expected under the null hypothesis that there is no relationship between restaurant spending and e-scooter program introduction. For further details, see Web Appendix G.

Variation in model specification. To ensure that our results are not an artifact of the exact specification of our model, we consider the following alternative specifications:

1. Different matching method: instead of CEM, we also applied one-to-one Propensity Score Matching (PSM) and obtain the results on matched data using PSM. We also run our analysis using unmatched data to allay concerns that results are sensitive to the matching procedure.

2. Different city pairs: to address concerns that our results are sensitive to which control cities are matched with our treatment cities, we considered several alternative city pairs. We varied our matching covariates being used for city pairing in the main analysis (population, population density, average annual temperature, average annual precipitation, and median household income) to obtain different control cities for our treatment cities.

3. Different individual matching: to show robustness of our results to the exact individual-level matching specification we use, we replace the individual-specific spend parameters that serve as our matching variables in our main model specification (Equation 1 in Web Appendix E) with a vector of matching variables equal to the sum of restaurant spend prior to e-scooter entry each month.

4. Different observation period: we consider a robustness check in which we use one-year windows before and after e-scooter program entry as our observation period.

5. Different city-specific time trends: to assess whether our results are robust to variation in the specification of our control variables, we allow more flexibility in a functional form of how city-specific spending trends may evolve in Equation 1, allowing for a quadratic city-specific time trend instead of a linear one.

6. Different unit of time: we consider a model in which we use a daily unit of time or monthly unit of time instead of a weekly unit of time, modeling purchase-days or purchase-months instead of purchase-weeks.

Details of these alternative specifications, and their results, are provided in Web Appendix H. As we show, our results across all of these specifications are qualitatively the same as those of our main analysis in terms of magnitude and direction, implying our results are not sensitive to the exact specification we have chosen.

Lastly, we run an event study specification to show dynamic treatment effects. We do so by taking Equation 3 from the Model Development section (which we had used to assess parallel pre-trends), and extending the event study window to include the post-treatment period in addition to the pre-treatment period. Recall that there was one week in which a lag coefficient was significantly different from 0 over the period before e-scooter entry. In contrast, most weeks were significantly different from 0 ($p < .05$) after e-scooter entry. Moreover, we observe significantly positive effects consistently over the duration post-treatment period, supporting the notion that the impact of e-scooter program introduction is not transitory in nature. For details, see Web Appendix F.

POLICY IMPLICATIONS

To further illustrate the economic impact of the results, we provide a conservative estimate of the population-level dollar amount of spending lift created by the introduction of these e-scooter programs.

We can estimate incremental restaurant sales for our 49 treated cities over the universe of restaurant companies tagged by Earnest Research in the post-treatment period after e-scooter program introduction, all else equal, through the coefficient estimates from our main model in Equation 1 (summarized in Table 3). For each treated individual over each post-treatment time period, we use Equation 1 to estimate total sales with versus without e-scooter entry by setting the treatment variable equal to 1 versus 0, respectively, while holding all parameters equal to their coefficient estimates. We then sum expected spending with versus without e-scooter program introduction across treated individuals over one-year post-treatment periods, then take the difference. We restrict our analysis to retrospective and not predictive estimates because we include time fixed effects in our model.

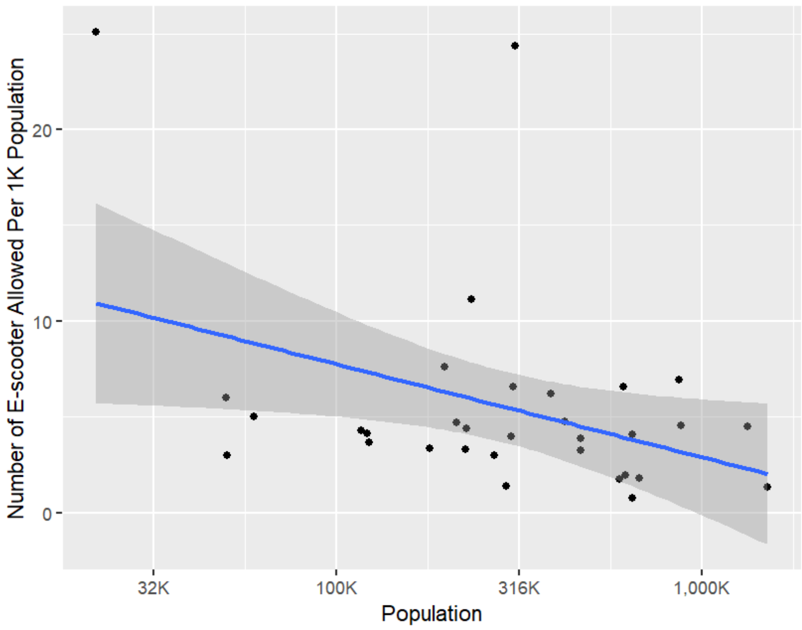
We then re-scale the dollar lift figures obtained above to account for how many individuals are in our panel data set in each city, relative to the working age and older (15+) population in those cities. For example, our panel represents approximately 4.1% of the working age and older population across all treatment cities, so we gross up our panel-level all-city dollar lift estimate by a factor of 24.58 to obtain our estimate of population-level all-city dollar lift for the 391 restaurant companies tagged by Earnest Research. This implies that all else equal, approximately $10.15 million in incremental spending over a one-year period was created for these restaurant companies in our 49 treated cities because of the introduction of e-scooter programs.

We also estimate of how much spending was lifted per e-scooter allowed to operate by the cities. We first identified every city for which citywide e-scooter allowed data was available, either through a government website or a local news source. Of the 49 cities we study, we were able to obtain citywide e-scooter allowed data for 32 of them. Table W 10 in Web Appendix I lists each city and the corresponding number of e-scooters allowed for that city, when available. For cities that changed the number of e-scooters allowed during the observation period, we conservatively report the maximum number of e-scooters allowed. 58,380 e-scooters in 32 cities were permitted to operate after e-scooter entry across these treatment cities. Taking the ratio of total population-level spending lift to e-scooters allowed across all cities we have permitted e-scooter fleet size data for, we estimate that at least $163.9 in restaurant sales was created per e-scooter allowed to operate in the city.

Of course, while city legislators can, for all intents and purposes, set the number of e-scooters allowed to any level they would like, we would not expect this lift estimate to hold if legislators set e-scooters allowed to an arbitrarily high level. As alluded to in the Model Development section, the number of e-scooters allowed is not what causes individuals in treated cities to spend more in the restaurant category – it is the riding of those e-scooters, which is a function of the number of e-scooters deployed and the propensity of individuals to ride deployed e-scooters. The number of e-scooters allowed is a ceiling for e-scooters deployed, and will not impact e-scooters deployed (and thus e-scooter rides) if increased beyond a certain point.

A natural question, then, is what a "normal" level of e-scooters allowed is. We would expect that this level would depend upon the city's population, motivating a focus upon the number of e-scooters allowed per capita. The average and interquartile range of e-scooters allowed per 1,000 people across the 32 cities we have e-scooter allowed data are 5.54 and 3.17 to 6.06, respectively. However, there is a strong negative correlation between the number of e-scooters allowed per capita and population size. We visualize this relationship through Figure 5, which shows the number of e-scooters allowed per 1,000 people as a function of (log) population across the cities we study, with a regression fit (and 95% confidence interval) overlaid. For smaller cities, e-scooters allowed per 1,000 people of between 3.78 and 5.76 (the IQR for cities in the first tercile by population size) may be more appropriate, while for larger cities, between 1.77 and 4.54 (the corresponding IQR for cities in the third tercile by population size) may be more appropriate. While other factors will certainly be at play, we hope that this provides policymakers with a reference point as they contemplate the size of an e-scooter program in their city.

Figure 5: Distribution of Cities along Population and E-scooter Allowed



Note: 32 treated cities whose number of e-scooters allowed are collected are plotted.

Finally, we can compare how the benefits of these programs – to the city directly and to the restaurant sector more broadly – compare to the cost to the city of rolling out the programs. Currently, cities pass on the cost of roll-out onto e-scooter companies through fees (e.g., for permitting and monitoring), making total fees a reasonable initial estimate for the roll-out cost to the city. While fee data is sparse, the city of Dallas reported that it had collected $67,848 in e-scooter-related fees in 2019, so we carry out these calculations for Dallas[4]. Of the aforementioned total lift across all cities of $10.15 million, $415,640 of this accrued to Dallas. Consider benefits to the city directly, versus to the restaurant sector:

1. The local sales tax rate in Dallas is 8.25%, implying that the city generated at least $34,290 in incremental sales tax revenue due to e-scooter rollout, or 50.5% of the cost of rollout to the city.

2. We may consider the benefit to the restaurant industry in terms of the incremental revenue and profitability that is created through e-scooter rollout. As mentioned above, e-scooter rollout created $415,640 in incremental revenue for the restaurant sector annually. When we multiply this dollar lift estimate by the contribution margin in the restaurant sector, as estimated through filings with the Securities and Exchange Commission by major publicly-traded companies in these categories, we estimate $92,740 in incremental profits to the restaurant sector annually, or approximately 137% of the cost of rollout to the city. For details, see Web Appendix J.

In sum, we infer that the lift created by e-scooter deployments to the city, and to the restaurant sector, are meaningful relative to the cost of rolling out the e-scooter program, which is currently absorbed by the e-scooter companies themselves. These figures may prove to be conservative, as the cost of an e-scooter program may be higher in the first year a city rolls out e-scooters than in subsequent years, depressing first-year profitability. Additionally, as mentioned earlier, we only measure spending lift at restaurant companies tagged by Earnest Research, implying that lift across all firms in the restaurant sector (i.e., tagged by Earnest Research or not) are likely to be larger than the figures provided here. As such, we believe these figures serve as a lower bound on the economic benefits that may accrue to policymakers in cities such as Dallas.

DISCUSSION

Our study suggests that micromobility creates positive economic externalities for the local economy they operate in – introducing an e-scooter program lifts sales in the restaurant sector, and this lift is not offset by a corresponding decline in non-restaurant local sales. The findings represent an opportunity for restaurant operators and e-scooter companies to capitalize on the economic surplus that they create. For example, these spillovers could be further facilitated through location-based advertising opportunities (Molitor et al. 2020), which could be a welcome source of incremental revenue for e-scooter companies, who have historically struggled with weak unit economics in their core business.

Likewise, these results suggest that city governments looking to stimulate the local economy may be incentivized to take a more lenient stance towards e-scooter usage, allowing e-scooters into cities in which e-scooter companies have not been allowed in, and/or raising the maximum number of e-scooters allowed to be on the street. We readily acknowledge that other factors are at play in these decisions, and some city governments may even view an increase in restaurant spending as a negative spillover from a societal wellbeing standpoint (which could then be viewed as another relevant yet unexpected impact of our results). Our broader point is that these impacts – financial and non-financial – should be understood and properly accounted for when regulators decide whether or not to allow e-scooters to operate in their cities.

We readily acknowledge that this research comes with limitations. Given that the level of geographical aggregation of our main data source is at the city-level, we can provide the average overall impact of introducing an e-scooter program into a city, but are not able to measure effects at a finer geographical unit. While a finer grained analysis may be harder to act upon as e-scooter introduction is inherently a city-level decision, such an analysis may still be of interest to policymakers. We also reiterate that the target of our analysis is an intention-to-treat effect – our goal is to measure the impact of introducing a new e-scooter program into a city, and not of e-scooter riding (or even of e-scooter deployment). While we believe the benefits of focusing this article upon the former outweigh the costs (e.g., valid causal identification, policy relevance and interpretability of results), uncovering the causal effect of usage and deployment behavior would enrich our understanding of how consumers and e-scooter companies drive impact, making this an interesting area for future work. Finally, we acknowledge the potential limitations associated with performing our analysis upon a credit/debit card panel data set. While we have shown that our panel data correlates very strongly with population-level sales measures disclosed by 36 restaurant brands representing most of the spending in our panel data set, and over 80% of all U.S. transactions are non-cash, we nevertheless acknowledge that there may still be measurement error stemming from the non-observability of cash transactions.

There are several other questions regarding the effect of e-scooter-enabled mobility on company sales and consumer behavior that we also leave for future work. For example, while our analysis only covers non-restaurant companies through an aggregated “total non-restaurant in-store spending” category, decomposing this further into subcategories such as entertainment (e.g., movie theaters) and health/fitness (e.g., gyms), could provide deeper insight into heterogeneity in the effects that e-scooter programs have across a wider range of categories. It could also be valuable to understand if our results generalize to non-US geographies. While we believe our results should extend to other geographies, empirical proof that they do could provide further confidence to international policymakers.

In conclusion, we believe that micromobility is an understudied area within marketing, and that studying its economic impact is important for a variety of stakeholders, including city policymakers, consumers, companies, and e-scooter companies themselves. We hope that this work represents a first step towards better understanding these important topics.

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Notes

[1]https://nacto.org/2020/08/27/136-million-trips-taken-on-shared-bikes-and-scooters-across-the-u-s-in-2019/; https://nabsa.net/about/industry/

[2]https://mobilityforesights.com/product/scooter-sharing-market-report/

[3]There were a total of 54 cities that launched e-scooter programs from June to October 2018, but five were dropped due to our matching procedure.

[4]https://dallascityhall.com/departments/transportation/traffic-calming/DCH%20Documents/ Dockless%20Vehicle%20Permit%20Application%20July%202018.pdf

The Web Appendix is available at the following link: https://bit.ly/3JNK277