The Early Effects of Coronavirus-Related Social Distancing Restrictions on Brands

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Abstract

This paper presents some of the first evidence on the effect of the spread of coronavirus (Covid-19) in the US on retail footfall traffic. The paper uses granular visit data from cell-phone tracking to estimate the shift in visits to different types of restaurants as coronavirus spread in the USA across the first three weeks of March 2020. The descriptive empirical work provides three useful insights. First, the precise level of coronavirus spread in the state or the timing of any in-person dining ban in the state has had far smaller effects than the pronounced nationwide overall collapse in demand. Second, there is little evidence of substitution towards restaurants focused on delivery as a result of the bans. Though dine-in restaurants suffered the largest drop in customers as a result of state-imposed restaurant bans, quick-service restaurants experienced a steep decline. Last, the biggest individual effects of these state-specific bans appears to have been top-ranked brands focusing on full service dining. Both top and non top-ranked brands suffered drops for restaurants not focused on dining in, with top brands suffering a slightly smaller decline.

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1 Introduction

The spread of the coronavirus (Covid-19) poses unimaginable challenges for all nations' health systems. As a result many countries have taken steps to ensure that people avoid being in close physical proximity to others to prevent the spread of the virus. One of the more salient steps in the USA has been a series of executive orders from state governors to restrict in-person dining at restaurants, and limit restaurant service to takeout and delivery. There have also been federal guidelines issued suggesting that people avoid eating in restaurants. Given the fact that Americans spend more money on "food away from home" than "food at home", [1] these dining bans may significantly alter people's daily life and business operation. This paper evaluates how this series of executive orders has affected visits to these restaurants. As such, it aims to give some early indications about the likely consequences of coronavirus-related bans on business and brands.

To do this, the paper uses data on visits to restaurants provided by Safegraph. Safegraph partners with mobile applications to obtain opt-in consent from 45 million users to collect anonymous location data. The longitude and latitude of these devices is used to determine visits to points of interest - in the case of this study, restaurants. We combine this data with data on the brand status and footprint of each restaurant brand, and also data on the spread of coronavirus at the state level, as well as the dates that various states imposed restaurant bans.

The empirical analysis in this paper is descriptive in nature. However, due to the fact that the paper uses three weeks of daily data from the beginning of March 2020, have a rich series of restaurant-level fixed effects, state-level data by the day about the spread of coronavirus, and variation provided by the precise date that states adopted restaurant bans, it seems reasonable to consider this paper is approaching a regression discontinuity approach

[1] `https://www.theatlantic.com/business/archive/2017/06/its-the-golden-age-of-restaurants-in-america/530955/`

which measures the causal effects of each state's ban on in-restaurant dining.

We find three empirical regularities. First, though the decline in physical visits to restaurants are correlated with the spread of coronavirus within states, the ban on dining within restaurants had a sharp and significant separate effect on physical visits. Second, there is no evidence of substitution towards restaurants which focus on offering takeout. Third, there is some evidence that the largest brands of restaurant chains suffered the smallest decrease in visits due to the spread of coronavirus and the various restrictions on dining imposed by government, though this advantage is very small relative to the general steep decline in demand experienced by all restaurants.

This paper contributes to three strands of the academic literature:

The first strand is a literature that studies the effects of the spread of coronavirus, and the associated steps taken by governments to restrict in-person interactions, on economic outcomes and consumer behavior. A search on the Social Science Research Network in late March revealed no other papers written on this topic. However, this is likely to change swiftly.

The second strand is the literature studying the effect of bans on consumer behavior in marketing. Typically, these papers document the possibility of spillovers and substitution resulting from well-intended regulations intended to stop consumers purchasing products that are not good for them. For example, Nelson (2003) shows that there is substitution towards other venues selling alcohol in the face of alcohol sales bans. Goldfarb and Tucker (2011) study the effect of bans on advertising certain products on certain types of media, and show that these bans effectively make other types of advertising for that product more effective. Mullick et al. (2017); Seiler et al. (2019) study the effect of soda taxes and show that they lead to displacement of retail sales to places where taxes are not imposed. Given this wealth of evidence of displacement and substitution due to bans, the finding of this paper - that there is little evidence of substitution as a result of the restaurant bans towards people directly visiting (and potentially congregating at) takeout-oriented restaurants, is useful for policy makers.

The third is the literature on consumer-demand for restaurants and the role of brands. One notable example of this is the work by Thomadsen (2005, 2007) who studies the effect of branded fast food restaurant ownership structure on competition and price. Kalnins and Lafontaine (2004) studies in a similar spirit the effect of the ownership of multiple stores. In general, this literature has emphasized the role of branding for fast-food restaurants in helping resolve uncertainty. This paper contributes to this literature, by documenting how the strength of the brand actually appears to have almost negligible effects at protecting the business at a time of national crisis.

The paper aims to be useful to policy makers. As of yet there is little evidence about the effect of social distancing measures on people's behavior. One worry is that a ban on something such as dine-in restaurants will simply lead to substitution to other forms of physical interaction with restaurants, such as congregating at a restaurant to pick up takeout. However, in this paper, we find little evidence of substitution. This suggests that in general bans motivated by enhancing social distancing are effective, and do not lead to unforeseen spillovers. There is also evidence that such bans are necessary if the aim is to avoid visitation of places where in-person contact makes the spread of disease likely. Though we do see evidence that the spread of positive cases of coronavirus does depress demand for restaurants of all kinds, it is clear that the bans themselves had a separate effect relative to the small measured effects of the actual spread of coronavirus in the locality. Furthermore, it is also clear that though the bans and data on the spread of coronavirus within a state have had a negative effect on visitation of restaurants, the effect is small relative to the large nationwide decline in visits. This suggests that these bans did have the intended effect of reducing visitation, but that visitation to restaurants had already declined substantially in the absence of direct bans on restaurants providing in-restaurant dining.

Though the intended audience for this paper is not predominantly managers, there are some insights about potential variation in likely consequences of wide-scale shutdowns associated with spread of coronavirus. Our results suggest that there are no winners among restaurants due to the restrictions implied by the spread of coronavirus. Instead they suggest that both large brands and smaller brands suffer, though smaller-scale brands suffer slightly more when focused on delivery, and top-ranked brands suffered more if they were focused on in restaurant dining. This contradicts earlier analysis on selected cities which suggested that brands with a large national footprint and a focus on quick service such as McDonalds would experience an uptick due to coronavirus.[2] We emphasize that though this might be taken as justification for stimulus measures to help both big and small businesses, we simply measure suppression of demand, not underlying economic ability to withstand that drop in visits.

2 Data

We use data provided by Safegraph for the purposes of studying the spread of coronavirus for the first three weeks of March 2020. This data is built on a panel of 45 million devices that collect anonymous location data.[3] Each of the users of these devices has opted into an app and given permission for their location to be tracked. Safegraph matches the location of these devices to a variety of locations of branded physical retail locations within the US. It then records the number of visits to that location within one day. The key measure of a visit is not the same as a purchase, but it is a useful estimate of visits to a physical location. And given that many of the policies focused on stemming the spread of coronavirus are focused on reducing foot traffic and physical proximity this seems a useful measure. To focus on the effect of dining bans, we only include visit data for restaurants (including full-service

[2] https://www.nrn.com/quick-service/visits-quick-service-restaurants-11-nationally-covid-19-outbreak

[3] https://www.safegraph.com/blog/what-about-bias-in-the-safegraph-dataset

restaurants and limited-service restaurants) in our dataset.

This data is focused on the US. There are two avenues in which the data is potentially not representative - first of course - it does not represent behavior of people who do not have smartphones. However, this is likely to be a smaller bias. There are estimated to be 272.6 million users of smartphones in the US in a population of 327.2 million.[4] A more serious potential source of bias is that the people who optin to having their location tracked may not be representative of the population. Goldfarb and Tucker (2012) show for example that younger people are more likely than older people to divulge personal information. Though some selection must evidently be present, Safegraph themselves have a variety of checks which suggest that their data does line up with census data such as expected household income in a census block.[5] We also cross-checked the data against aggregate measures of the number of retail locations of branded restaurant chains and found a 99.1% correlation in the data, which is reassuring that selection is not leading to gaps in coverage of restaurants.

We extended the Safegraph data with a variety of external data. First is data on the spread of coronavirus across states. This was collated by the New York Times, and they made it available to researchers.[6] This includes data on the number of confirmed cases of coronavirus and deaths in each state by day. Of course, this data is unlikely to be accurate about the true spread of coronavirus in the US, given the general lack of testing capacity (Cohen and Kupferschmidt, 2020). Therefore, this data should be thought of at best as an indicative moving measure about the likely shifts in community spread. Or, more conservatively, it should be interpreted as simply representing the information that people living in that state had access to about the extent of spread in their community at the time they decided to visit a restaurant. Though this data includes data on deaths from

[4] https://www.statista.com/statistics/201182/forecast-of-smartphone-users-in-the-us/ This estimate seems rather high to us given the idea that young children are unlikely to possess devices.

[5] https://colab.research.google.com/drive/1u15afRytJMsizySFqA2EPlXSh3KTmNTQ#offline=true&sandboxMode=true

[6] https://raw.githubusercontent.com/nytimes/covid-19-data/master/us-states.csv

the coronavirus, this is not the focus of our specifications - partly because it is a lagging indicator, partly because it is collinear with the number of cases, and partly because in the first three weeks of March 2020, there was not yet substantial mortality in the US. We include specifications including the effect of deaths in the appendix.

The second type of data was data on the precise timing that states enacted bans on in-restaurant dining. The primary source of this data is provided by the National Governors Association,[7] which keeps records on state actions designed to limit the spread of coronavirus. In each case, we verified the data on the ban, with local media. In some cases, for example in Utah, counties imposed bans on dining prior to state-wide bans being imposed.[8] In such cases, we focus on the dates when the state-wide ban was imposed given that we do not have county-level data on key variables such as the spread of the coronavirus. Though this may introduce some measurement error to our estimate, it does mean that our estimates of the effects of in-person dining bans on restaurants are therefore likely to be conservative.

Our dataset ends on March 22nd. As such it predates the majority of 'stay at home' orders that have been issued by states to combat coronavirus. One exception is California, whose stay at home order started on March 19th, and also New Jersey and Illinois whose stay at home order started on March 21st, and New York and Ohio on the final day of our data on March 22nd. We check the robustness of our results to excluding these states, and the results were similar.

In addition, on March 16, the federal government released advisory guidelines to tackle the coronavirus crisis. These stated that people should 'avoid eating or drinking at bars, restaurants and food courts - use drive-thru, pickup or delivery options.[9] Since these were

[7]https://www.nga.org/coronavirus/

[8]Salt Lake County imposed a ban first https://www.deseret.com/utah/2020/3/16/21181755/coronavirus-covid19-utah-salt-lake-city-bars-restaurants-closed-shutdown, but was swiftly followed by a day later the rest of the state https://www.sltrib.com/news/2020/03/18/utah-orders-restaurants/

[9]https://www.whitehouse.gov/wp-content/uploads/2020/03/03.16.20_coronavirus-guidance_8.5x11_315PM.pdf

nationwide in scale, we do not have an easy way of separately identifying their effect from a general national decline. In addition, these guidelines were announced simultaneously with a variety of top-branded chains such as Starbucks and McDonald's ceasing in-venue dining operations. Though we include some specifications which estimate the magnitude of a coefficient associated with this national ban, we highlight that this should not interpreted as being the effect of the national ban - instead it should be interpreted as a simple measure of nationwide decline in demand for restaurant services after March 16.

The last type of data was data on the ranking, footprint and brand-power of the restaurant chains we study. We obtained this data from the Foodservice Database Company.[10] This data covers the top 250 restaurant brands in the country and covered 83.23% of observations in the safegraph data.[11] The brands that were not matched covered 1,177 different smaller restaurant brands.

Table 1 provides summary statistics of the key variables. In total we have observations for 193,635 restaurants over 21 days.

The large maximum number for the number of reported coronavirus cases is driven by New York state at the end of the period. In our main specifications, we focus on a per-capita measure of the coronavirus spread. However, we also measure in the appendix the effect of different ways of measuring the spread of coronavirus, including the log of cases. $DiningBanInPlace$ is an indicator variable for whether the state had imposed bans on in-person dining. $FullServiceDining$ is an indicator for whether the restaurant was focused on providing in-person dining. 25% of the restaurants in our data are full-service restaurants. $2018U.S.Units$ measures how many branches each of the chain restaurants in our dataset had. The top 20 brand indicator, is an indicator for whether the restaurant belongs to a top

[10] `https://www.fsdbco.com/top-250-restaurant-chains-us-2019/`

[11] There were 11 brands in the top 250 database that are not included in our study as their focus was not dining. These include brands like 'Brass Tap' which is described as an upscale beer bar, and venues such as 'Dave and Busters' which are primarily entertainment venues.

20 brand (defined by 2019 US sales). From these statistics, it makes it clear that what we are studying are largely chain restaurants, rather than independent restaurants with a single outlet. In general, chain restaurants account for around 47% of restaurants in the US.[12]

Table 1: Summary Statistics

	Mean	Std Dev	Min	Max
Visits	14.5	19.8	0	971
Coronavirus Cases Reported	204.9	817.4	0	15,168
Cases Per Capita ($\times$10000)	0.14	0.45	0	7.80
Deaths	3.17	9.99	0	122
Dining Ban in Place	0.20	0.40	0	1
National Guidelines Issued	0.29	0.45	0	1
Full Service Dining	0.25	0.43	0	1
2018 U.S. Units	6,571.0	7,887.7	14	24,798
Top 20 Brand	0.48	0.50	0	1

N= 4,066,335, except for 2018$U.S.Units$, where the number of observations is 3,461,325.

Typically, the number of visits is around 14.5 a day for each restaurant venue. However, there was a skewed distribution with some venues reporting a very high number of daily visits. In each of these cases, the number of visits were for a very successful fast food restaurant and appeared plausible given its location. However, just in case the Safegraph data was tracking, for example, highway traffic rather than visits to the restaurant, we excluded restaurants where it was claimed there were over 1,000 visits in a day in the data we analyze (this was less than 0.1% of our sample). Given the coverage of roughly 10% of the population by Safegraph, more than 1,000 visits in a day seemed implausible. We also use log of visits as our main dependent variable to help compress the effect of outliers.

[12] `https://foodondemandnews.com/0317/restaurant-analysts-react-to-covid-19-closures/`

3 Empirical Analysis

This paper uses a familiar base econometric specification, where for restaurant i located in state s on date t, visits are a function of:

$$LogVisits_{it} = \beta_1 DiningBan_{st} + \beta_2 CoronavirusCasesPerCapita_{st} + \alpha_i + \delta_t + \epsilon_{it}. \quad (1)$$

The dependent variable is log-transformed, not only for ensuring our results are not driven by outliers but also for better interpretability. We report the robustness of our results to using a linear measure of visits in Table A2 in the appendix. The number of coronavirus cases per capital is added here to control how reports on the severity of the local outbreak itself affects restaurant visits. In appendix Table A1 we explore the results of using different specifications of the way the spread of coronavirus is reported. That table indicates that cases per capita appears to have the most explanatory power.

To control for the large degree of heterogeneity of restaurants in our sample, we include fixed effect α_i for each of the 193,629 restaurants in our sample. We also include in some specifications a vector of date fixed effects δ_t. When we are looking at the correlation of visits with the nationwide guidelines, we use day of week fixed effects, as the date fixed effects are collinear with time trends at the national level.

In subsequent specifications, we use subgroup analysis regarding restaurant characteristics to understand the variation in size and direction of these variables.

3.1 Base Effects of Dining Bans on Visits to Restaurants

Figure 1 suggests that physical visits to restaurants dropped steeply after in-restaurant dining bans – the average number of visits dropped by more than half after the dining bans took effect. In full service restaurants, this drop was larger than a half - which is to be expected given the dining directly affected their main function. However, restaurants that did not

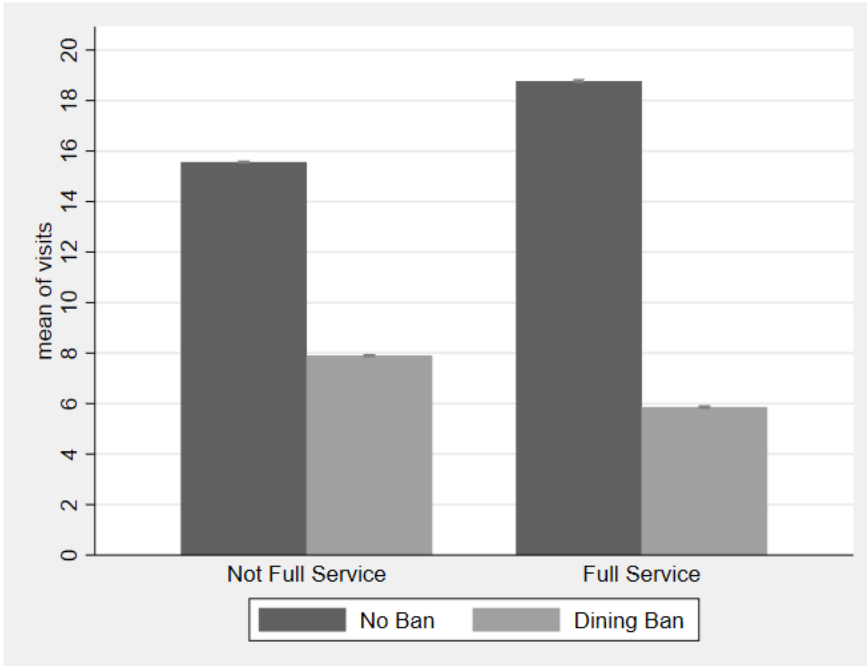

Figure 1: How Visits To Restaurants Vary By Type and By Presence of State-Level Ban

focus on full service, and instead focused on takeout options, still experienced a large drop.

Table 2 reports ordinary least squares (OLS) estimates of the base specification presented in Equation (1). Column (1) in the table shows the result of a regression that explores the correlation between coronavirus cases in a state, the presence of a dining ban, the issuing of national guidance to not eat at restaurants and visits to restaurants. The result suggests that people tend to visit restaurants less when the coronavirus outbreak is a more serious issue: Visits drop by 8%[13] if the number of cases per ten thousand people increases by one. However, of course cases of coronavirus were increasing during our period and it is likely that this is picking up a general step up of protective measures rather than the causal effect of reported cases on restaurant visitation. We measure that after March 16, visits to restaurants dropped on a nationwide basis by 37% on average. The presence of a state dining ban was correlated with a 18% incremental decrease in visits.

[13]To interpret the coefficients of these regressions given the log form, one takes (1-exp(-Coeff)) or in this case (1-exp(-0.078)) of the coefficient.

Though the magnitude of these effects is striking and useful from a policy point of view, we emphasize that they are correlational rather than causal. There are many things that shifted in the nation as coronavirus cases increased, and after the national guidelines were issued. To more closely approximate causal effects, we use the full specification in equation (1), and use date fixed effects, to more sharply measure the discontinuity in visits before and after a particular state imposes a ban on in-place dining. Column (2) reports the results of this specification. What is striking is that there is no measured effect of local state-level coronavirus cases being reported. Instead, the only negative and significant effect is that of a state enacting a dining ban. Though highly significant, the effect of the ban itself is an 8% decrease in visits. Given the large drop in restaurant visitation observed in the raw data, it seems that much of this drop was not related to the imposition of dining bans themselves but instead general shifts in population behavior due to the spread of the coronavirus.

In Columns (3)-(6) of Table 2, we investigate the variation in the impact of dine-in eating bans across different types of restaurant. We first divide the restaurants into two categories based on their NAICS code: Full-service restaurants (722511) and not full-service restaurants (composed of limited-service restaurants, 722513). The major difference between the two categories is that customers will be served by a server and then pay after eating if they choose to consume food on premises in a full-service restaurant. In non full-service restaurants they will pay before eating and won't be served even when dining in. Therefore, the gap in customer experience between dine-in visits and take-out visits is smaller for the restaurants from the latter category. As shown in Figure 1, this gap has been reflected in the degree to which people cut their restaurant visits after the dine-in eating bans went into effect: On average, visits to full-service restaurants drop by more than two-thirds after the enforcement of dining-in bans while we are seeing a smaller drop for not full-service restaurants.

Table 2: Baseline Effects of the State-Level Ban

	(1)	(2)	(3)	(4)	(5)	(6)
Type	All		Full Service		Not Full Service	
	Logged Visits	Logged Visits	Logged Visits	Logged Visits	Logged Visits	Logged Visits
Dining Ban in Place	-0.202***	-0.080***	-0.354***	-0.175***	-0.161***	-0.056***
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
Cases Per Capita (×10000)	-0.078***	0.001	-0.107***	0.014***	-0.076***	-0.009***
	(0.001)	(0.001)	(0.004)	(0.004)	(0.001)	(0.001)
National Guidelines Issued	-0.468***		-0.708***		-0.381***	
	(0.001)		(0.003)		(0.001)	
Restaurant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	No	Yes	No	Yes	No
Date Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	4,066,335	4,066,335	1,013,082	1,013,082	3,053,253	3,053,253
R-Squared	0.78	0.80	0.76	0.79	0.80	0.82

Notes: Dependent variable is the logged value of visits+1 on day t in restaurant i. Since the log of zero does not exist, to include observations where there are zero visits we use the log of visits + 1. Robust standard errors clustered at restaurant-level are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

To quantify and compare the effect sizes, we conduct a subgroup analysis based on the base specification presented in Equation (1). Focusing on the estimates in Columns (4) and (6) which have date fixed effects and therefore can be, with some caution interpreted, as causal, there is a 16% drop in restaurant visits due to a state-level prohibition on in-restaurant dining for full-service restaurants while the change is smaller but still significant for not full-service restaurants (5%). To ensure our results were accurate we reran our specification using a finer segmentation obtained from the Foodservice database, where the restaurant brands are divided into four segments: Fine dining, casual dining, fast casual, and quick service restaurants. Consistent with the previous results, these unreported results suggest that restaurants that provide more service suffer a more severe drop in visits. Though the large decline in estimates of visits to full service restaurants is to be expected, the still significant decline in visits to non-full service restaurants is important, as it suggests that there is no aggregate-level substitution taking place. Estimates suggest that in a typical quick-service restaurant, approximately 70% of sales are generated through the drive-thru with a low-to-mid-single digit mix from delivery, which should have in theory insulated such restaurants from the decline in visits observed.[14] Our findings also goes against earlier optimistic media reporting which suggested that restaurants that focused on take out could be a 'winner' from the coronavirus pandemic.[15]

3.2 Variation in Effects by Brand

We then explore the variation in effects by brand. We rank the brands by its sales in 2018[16] and divide the restaurants into two, by distinguishing between brands in the top 20 and brands, that are not in the top 20. Given that quick service restaurants dominate in sales (all top 10 brands are not full-service restaurants), we look at the variation in full-service

[14] https://foodondemandnews.com/0317/restaurant-analysts-react-to-covid-19-closures/

[15] https://www.fool.com/investing/2020/03/20/quick-service-eateries-see-an-11-traffic-boost-fro.aspx

[16] This information is available for the top 250 restaurant brands in the country

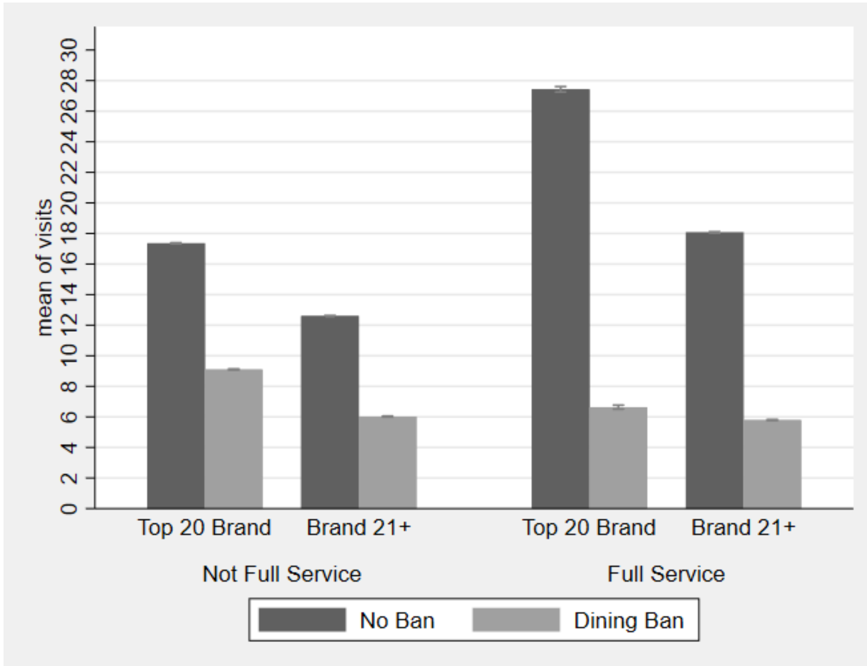

Figure 2: How Visits To Restaurants Vary By Brand × Type and By Presence of Ban

restaurants and not full-service restaurants separately. Figure 2 suggests three things. First, full-service restaurants have more visits before the regulation and smaller brands have fewer visits per store, which is consistent with how the rankings are calculated. Second, the drop is smaller for top non full-service brands in terms of percentage change. It suggests that larger non full-service brands may provide more diversified service and are more resistant to the regulation. Finally, a reverse trend has been seen in the full-service restaurants and the number of visits dropped to almost the same level for all restaurants in this category when dine-in eating is banned. Therefore top full-service brands may suffer a larger loss compared to other restaurants due to the nature of their business.

The corresponding estimation results based on the basic model are presented in Table 3. We report the results first in Columns (1)-(4) for a specification that looks at general aggregate trends, but does not try and measure a causal effect. What is striking in these results are the very large declines in visits since the issuing of the national guidelines on

March 16. Our estimates imply that for top brands, there has been a 63% decline in visits, if they are a full-service offering. For restaurants not focused on full service, there has been a 30% decline in visits for top brands and a 35% decline in visits for non-top brands. Therefore, though top brands may have seen a smaller percentage decline, they still have seen a substantial drop after March 16th.

In Columns (4)-(8) of Table 3, we look at the measured effect of the direct timing of the imposition of a state-wide ban on dining within restaurants. These estimates suggest that the effect of the dining ban, was almost double that for full service top brands than non-top brands (26% vs 15%). For restaurants focused on takeout, top brands saw a decline of 5% in visits, and non-top brands saw a decline of 7%. These results suggest that top brands focused on quick service only saw modest advantages relative to the direct effects of the dining ban, relative to non-top brands.

Table 3: Effect of Dining Ban on Restaurants by Type and By Brand Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Type	Full Service		Not Full Service		Full Service		Not Full Service	
	Top 20 Brand	Brand 21+	Top 20 Brand	Brand 21+	Top 20 Brand	Brand 21+	Top 20 Brand	Brand 21+
Dining Ban in Place	-0.501***	-0.339***	-0.148***	-0.181***	-0.302***	-0.161***	-0.047***	-0.070***
	(0.013)	(0.004)	(0.002)	(0.003)	(0.012)	(0.004)	(0.002)	(0.003)
Cases Per Capita ($\times$10000)	-0.116***	-0.101***	-0.071***	-0.085***	0.013	0.021***	-0.007***	-0.014***
	(0.010)	(0.004)	(0.002)	(0.003)	(0.009)	(0.004)	(0.002)	(0.003)
National Guidelines Issued	-0.990***	-0.688***	-0.355***	-0.424***				
	(0.010)	(0.003)	(0.002)	(0.003)				
Restaurant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	Yes	Yes	Yes	No	No	No	No
Date Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes
Observations	76,944	936,138	1,893,108	1,160,145	76,944	936,138	1,893,108	1,160,145
R-Squared	0.76	0.76	0.82	0.77	0.82	0.79	0.83	0.78

Notes: Dependent variable is the logged value of visits+1 on day t in restaurant i. Since the log of zero does not exist, to include observations where there are zero visits we use the log of visits + 1. Robust standard errors clustered at restaurant-level are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4 Conclusions

This paper explores how recent in-dining bans have affected visits to restaurants. We find three patterns. First, the spread of the coronavirus has led to a nationwide decline in visits to restaurants. Dining bans at the state level have had a small complementary negative effect on visits relative to this general decline. Second, there is no evidence that the shuttering of dining rooms has led to substitution towards drive-through or takeout venues. Third, there is little evidence that possessing a strong brand is helpful in navigating this general decline in demand.

There are of course limitations of our study. First, this is early work looking at early patterns of retail visits that have shifted due to the spread of coronavirus in the US. Second, though we believe that due to our use of a rich set of restaurant and date fixed effects we are able to examine the effect of the timing of an in-restaurant ban within a state, we acknowledge that if other measures were imposed by the state at the same time, we cannot tease these apart. Instead, we are measuring the bundle of measures that the average state imposed at the same time as its dining ban. Third, our measure of visits focuses on actual traffic to restaurants and not other sources of revenues such as delivery. Notwithstanding these limitations, we believe our paper is a useful first step in starting to calibrate the likely effects of shutdowns on retail outlets implied by the coronavirus pandemic.

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A.1 Appendix

We try different transformations of the number of confirmed cases in Table A1. The number of total infected cases, the number of infected cases per 10,000 people, and the log-transformed infected cases are considered in Column (1) - (3), respectively. All of them suggest that visits decrease with a growing outbreak. In Column (4) the same table, we consider a model where the number of deaths in the state is controlled.[17] We add all those variables into one model and the estimates reported in Column (5) indicate that cases per capita appears to have the most explanatory power.

We adopt a different specification of the dependent variable by replacing the logged visits by its original metric in Table A2. We report the estimation results for different segments separately to shed light on how the effect size changes with the daily visits. The fact that the effect size of the regulation is almost proportional to the daily visits supports the use of the log-transformed number of visits in our main model, which produces the coefficients that can be easily interpreted in terms of percentage change. In contrast, the coefficients can be read as raw effects on the average number of visits for that particular strata of visitation by restaurant. For example, the estimates in Column (3) suggest that for a restaurant which on average exhibited a maximum of over 100 visits, there were 42 fewer visits after the March 16.

[17]We add all of those three forms of the number of confirmed cases in the model to better control it.

Table A1: Effect of Spread of Coronavirus Cases: Different Specifications

	(1) Logged Visits	(2) Logged Visits	(3) Logged Visits	(4) Logged Visits	(5) Logged Visits
Coronavirus Cases Reported	-0.000***				-0.000***
	(0.000)				(0.000)
Cases Per Capita (×10000))		-0.005**			0.089***
		(0.001)			(0.005)
Logged Coronavirus Cases			-0.003**		-0.009***
			(0.001)		(0.001)
Deaths				-0.001***	-0.001***
				(0.000)	(0.000)
Restaurant Fixed Effects	Yes	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	4,066,335	4,066,335	4,066,335	4,066,335	4,066,335
R-Squared	0.80	0.80	0.80	0.80	0.80

Notes: Dependent variable is the logged value of visits+1 on day t in restaurant i. Since the log of zero does not exist, to include observations where there are zero coronavirus cases (visits) we use the log of cases (visits)+ 1. Robust standard errors clustered at restaurant-level are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A2: Effect of Dining Ban on Restaurants: Linear Model

	(1) < 50 Visits	(2) 50-100 Visits	(3) 100+ Visits	(4) < 50 Visits	(5) 50-100 Visits	(6) 100+ Visits
Dining Ban in Place	-1.366***	-6.978***	-12.541***	-0.169***	-1.345***	-2.797***
	(0.017)	(0.098)	(0.679)	(0.018)	(0.094)	(0.721)
Cases Per Capita (×0000)	-0.589***	-2.905***	-8.431***	0.074***	0.275***	-2.278**
	(0.014)	(0.100)	(0.889)	(0.014)	(0.064)	(0.833)
National Guidelines Issued	-4.065***	-15.627***	-42.245***			
	(0.015)	(0.081)	(0.642)			
Restaurant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week Fixed Effects	Yes	Yes	Yes	No	No	No
Date Fixed Effects	No	No	No	Yes	Yes	Yes
Observations	3,456,432	482,202	124,593	3,456,432	482,202	124,593
R-Squared	0.70	0.58	0.50	0.72	0.66	0.53

Notes: Dependent variable is the number of visits on day t in restaurant i. Robust standard errors clustered at restaurant-level are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Restaurants with an unusually high level of visits on a single day are excluded (5% of the sample).