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FEAR, LOCKDOWN, AND DIVERSION:
COMPARING DRIVERS OF PANDEMIC ECONOMIC DECLINE 2020

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Fear, Lockdown, and Diversion: Comparing Drivers of Pandemic Economic Decline 2020
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ABSTRACT

The collapse of economic activity in 2020 from COVID-19 has been immense. An important question is how much of that resulted from government restrictions on activity versus people voluntarily choosing to stay home to avoid infection. This paper examines the drivers of the collapse using cellular phone records data on customer visits to more than 2.25 million individual businesses across 110 different industries. Comparing consumer behavior within the same commuting zones but across boundaries with different policy regimes suggests that legal shutdown orders account for only a modest share of the decline of economic activity (and that having county-level policy data is significantly more accurate than state-level data). While overall consumer traffic fell by 60 percentage points, legal restrictions explain only 7 of that. Individual choices were far more important and seem tied to fears of infection. Traffic started dropping before the legal orders were in place; was highly tied to the number of COVID deaths in the county; and showed a clear shift by consumers away from larger/busier stores toward smaller/less busy ones in the same industry. States repealing their shutdown orders saw identically modest recoveries--symmetric going down and coming back. The shutdown orders did, however, have significantly reallocate consumer activity away from “nonessential” to “essential” businesses and from restaurants and bars toward groceries and other food sellers.

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The spread of the SARS-CoV-2 virus and its associated COVID-19 disease has had unprecedented effects on economic activity around the world. In an effort to limit the spread of the disease, many governments adopted stay-at-home/shelter-in-place orders. That ignited a debate over “re-opening” and whether the health benefits from their slowing of the virus outweighs the economic damage they did.

It is not clear, however, that the economic decline actually came from the lockdown orders. By many accounts, anxious individuals engaged in physical distancing on their own accord. Understanding the size of that effect is critical policy question. If fear rather than policy drives the economics, the economic stimulus from repealing the orders may be considerably smaller than some might predict.

In this paper, we estimate the causal effect of government policy on the economy during the initial spread of COVID-19 in the U.S. using data on foot traffic at 2.25 million individual businesses. Our empirical strategy separates the effects of voluntary distancing from that of policy orders by comparing differences in foot traffic across businesses within commuting zones that span jurisdictions facing differing legal restrictions. This leverages two related types of variation: businesses in border-spanning commuting zones where jurisdictions impose of shelter-in-place orders at different times (e.g., northern Illinois when Illinois placed a sheltering order on March 20th while Wisconsin waited until the following week), and businesses in commuting zones where a jurisdiction never imposed an order (e.g., the Quad Cities area, where the Illinois towns of Moline and Rock Island faced stay-at-home orders but bordering Davenport and Bettendorf, Iowa did not). We collect data on the shutdown policy conditions at the county level, rather than relying on state-level laws as in most of the existing literature, because many of the hardest hit counties in the country imposed shutdown orders earlier than their states did.

The results indicate that legal shutdown orders account for a modest share of the massive overall changes in consumer behavior. Total foot traffic fell by more than 60 percentage points, but legal restrictions explain only around 7 percentage points of that. In other words, comparing two similar establishments within a commuting zone but on opposite sides of a shelter-in-place (S-I-P) order, both saw enormous drops in customer activity. The one on the S-I-P side saw a drop that was only about one-tenth larger. The vast majority of the decline was due to consumers choosing of their own volition to avoid commercial activity.

We find evidence tying this voluntary decline in commercial activity to fear of infection. The drop in consumer visits is strongly correlated with the number of local COVID deaths. Further, within an industry, drops in visits are disproportionately larger in establishments that were busier/larger before COVID. This is consistent with greater avoidance of and substitution away from establishments with higher potential transmission contacts.

Interestingly, and further supporting the modest size of the estimated S-I-P effects, when some states and counties repealed their shutdown orders toward the end of our sample, the recovery in economic activity due to the repeal was equal in size to the decline at imposition. Thus the recovery is limited not so much by policy per se as the reluctance of individuals to engage in economic activity that requires interacting with others.

Although the shutdown orders had a small aggregate impact, they had significant reallocation effect by driving consumer activity from “nonessential” to “essential” businesses and from restaurants and bars toward groceries and other food sellers.

There is a rapidly burgeoning empirical economics literature examining many aspects of the COVID-19 pandemic. Our study is tied most closely to two areas of this literature. One involves studies using cellular phone data to track how fear of the virus or lockdown orders have affected personal mobility and interactions. Examples include Alexander and Karger (2020), Alfaro et al.

(2020), Barrios et al. (2020), Chen et al. (2020), Cicala et al. (2020), Couture et al. (2020), Dave et al. (2020a), Fang et al. (2020), Gupta et al. (2020), and Nguyen et al. (2020). Goldfarb and Tucker (2020) tie personal mobility to retail activity by evaluating which retail industries have the most social interaction. Maloney and Taskin (2020) demonstrate connections between mobility and commercial activity in U.S. restaurants and Swedish movie theaters.

The second area of related work includes studies that focus directly on the economic impact of lockdown policy, though with different data. Kahn et al. (2020) use job postings data to show that the labor market deteriorated substantially but did so across the board, rather than more in states with shutdown orders. Rojas et al. (2020) investigate UI claims and similarly find shifts across the board. Bartik et al. (2020a, 2020b) study the impact of COVID on small businesses using survey data and employment/hours using the HOMEbase data set, documenting a significant employment decline. Aum et al. (2020) look at the labor market effects of COVID in Korea (which did not have large government imposed lockdowns) by comparing regions with larger COVID outbreaks to ones with smaller and find employment collapses even without policy. Coibon et al. (2020) do the same with survey data in the Nielsen panel so they can track aggregate spending and find that lockdowns greatly reduce spending (though their measure of lockdown is a subjective survey questions rather than an actual measure of policy). Gupta et al. (2020) look at CPS data and argue that 60% of the decrease in employment came from state social distancing policy, though they are unable to rule out the possibility that the employment drop began before the policies were in place.

Our paper differs from these studies in that we combine detailed phone record location data at the level of the business (rather than the individual) with more comprehensive data on legal restrictions than what existed in previous work. This allows us to investigate not just patterns in aggregate activity but substitution patterns across businesses as well. On the policy side, most studies

use state level information on S-I-P orders and legal restrictions aggregated by sites like the New York Times. In reality, however, a large number of counties and cities imposed lockdown orders separately from their states and before the state acted. We demonstrate below that especially in situations involving cross-border comparisons, the local policy data are important.

1. Data

Our data come from the SafeGraph panel of mobile phone usage (see SafeGraph, 2020 or Squire, 2020, for more details). SafeGraph collects information on almost 45 million cellular phone users—about 10% of devices in the U.S.—and compiles the number of visits to millions of different “points of interest” in the U.S. as specified by address. We will use this business level information. In our sample, SafeGraph reported visit numbers excluding employees of the business. We focus on business locations in industries where consumer visits are a plausible measure of economic activity (not, for example, manufacturing facilities) and we drop non-profits and other non-commercial enterprises. There are some complications and measurement errors that arise for businesses that are co-located in a space like a Starbucks inside an subway station, say, so that the phone data might record a large number of visits to the location but in reality, most of those were not to the business in question. Our sample includes more than 2.25 million business locations and includes weekly customer visitation data from March 1 to May 16 and monthly visitation data before that.

Implicitly we assume that the number of visits corresponds to the amount of economic activity. If people shop half as frequently but spend twice as much each time they go out, we would not observe that behavior. The timing of the aggregate drop in consumer visits, though, matches well the broader economic declines. To combine industries into one regression and measure aggregate effects, we weight businesses in our regressions by their average number of consumer visits in January, before COVID. The results are largely identical if we weight by the product of

January visits and the industry average revenue per visit (computed using supplemental industry revenue data from the Census Bureau).

For policy measures, most of the literature has used state-level shutdown orders. However, many lower levels of government imposed shutdown orders prior to their parent states acting. We collected more detailed policy data that includes county-level orders and use that here. We describe these data in Goolsbee et al. (2020).

2. The Problem of Confounding Lockdown with Fear

Figure 1 shows the precipitous drop and partial recovery in visits to businesses in the SafeGraph data over March, April, and early May. It shows two series, each measured using establishments' logged average visits per day across the week. The red line shows the raw data. The blue line plots the values of the week fixed effects in a regression of logged visits on establishment and week fixed effects. This latter series reflects average patterns over time controlling for any changes in the composition of establishments in the sample. We normalize both series to a value of zero in the first week of March for comparison purposes.

Both series show similar patterns. From the start of March to the trough in the week of April 12th, the aggregate number of logged visits fell by around 0.9, a 60% decline. The suddenness and the magnitude of this drop is quite similar to the credit and debit card spending data in Cox et al. (2020) or the UI claims data. In the Appendix Table, we break down the start-to-trough drop in visits for the 110 6-digit NAICS industries in our sample. It shows mostly expected patterns in terms of severity of the downturn. Businesses in almost all industries saw large declines in foot traffic, but they range from a 99% decline in the hardest hit industry, Theaters and Dinner Theaters, to slight increase at Outdoor Power Equipment Stores at the other extreme.

The question of how much of this collapse came from government regulations is not immediately obvious in the figure. A simple time series correlation would suggest the two are related, but if the spread of the virus both made people afraid to go out and induced states and counties to impose lockdowns, the correlation could be spurious. Indeed, most jurisdictions did not impose legal shutdown orders until late March or early April, but Figure 1 shows a considerable collapse of commerce before most shutdown orders were in place.

The basic problem of with estimating the impact of policy becomes clear in Table 1. Here we again combine all businesses together into a single regression, weighting each by their visits in January. The dependent variable is the establishment's log average number of visits in the week. The key explanatory variable is an indicator for the existence of a shelter-in-place (S-I-P) order for the establishment's county in that week. The regression also includes establishment fixed effects. We cluster the standard errors at the county level.

This "naïve" regression suggests a massive effect of S-I-P orders on economic activity. The coefficient in column (1) indicates S-I-P orders correspond to a more than 70 log point decline in consumer visits.

In column (2) we include both our county-level policy measure as well as an indicator for the applicable state-level policy. The results indicate that the locally detailed measure explains far more of the change in economic activity than the state-level measure, supporting our more geographically detailed metrics.

Column (3) adds to the regression the cumulative number of COVID deaths in the county. Because the death count distribution is highly skewed while still having many county-weeks with zero cases, we use the logarithmic-like inverse hyperbolic sine transformation (see Burbidge, Magee and Robb, 1988). As is apparent in the table, local deaths are strongly related to the size of the reduction in consumer visits. Further, controlling for deaths both reduces the estimated impact of

county S-I-P policy by 25%. Column (3) also thoroughly documents the importance of the county level data instead of the state. The state-level policy coefficient is economically small, statistically insignificant and of the wrong sign. We will use only the more detailed measure for the remaining results.

Finally, in column (4), we add commuting-zone-by-week fixed effects. These fixed effects control for any unobserved factors, like consumers' average current fears of infection, that operate across the geographic area in that week. It also means that the estimated effect of S-I-P orders in this specification comes from comparing differences in consumer behavior within commuting zones but across counties with different policies. Here, the estimated impact of shutdown orders falls by an order of magnitude relative to that column (1), to a bit over 7%.

The comparison of the coefficient on the S-I-P order indicator in column (4) to those in the table's other columns is important. It shows the correlation between the decline in economic activity and S-I-P policies arose mostly because the COVID crisis jointly drove both, not because S-I-Ps had a large causal effect on activity. People greatly reduced their activity regardless of the existence of S-I-P orders. The orders *per se* cut activity further in areas subject to them, but by only a modest amount, around one-tenth of the total response.

The results in column (4) also demonstrate that even as the estimated impact of lockdown policy is modest, local COVID deaths still significantly drive down consumer visits. The spread of the disease itself is strongly correlated with declines in economic activity. Because the regression includes commuting zone-week fixed effects, this indicates that even within commuting zones, more local deaths reduce local economic activity. Interestingly, though, applying this within commuting-zone coefficient at face value as an aggregate impact and multiplying by the overall increase in deaths over our entire sample, the rise in COVID deaths would correspond to a decrease in economic activity of around 30% or half the total decline observed in the data.

3. Robust Identification: The Modest Impact of Lockdowns Estimated Multiple Ways

Because there are two types of variation in the data—businesses in places where the lockdowns occurred earlier on one side of a border than the other, and businesses in places where one side of the border is in one of the eight states that never had a general lockdown—we can test whether the estimated impact of lockdown orders is consistent across these sources of variation. The results are in Table 2. Column (1) shows the estimated impact of lockdowns in the subsample of only commuting zones that share a border with a jurisdiction that never had lockdown. Column (2) looks only at businesses in the other commuting zones, where identification comes strictly from timing differences in states’ and counties’ impositions of policies. The estimated impact is almost identical in the two subsamples.

Then, in column (3), we look at potential asymmetries in S-I-P effects depending on whether they are being imposed or repealed. By the end of our sample, some states and counties had repealed their sheltering orders or let them expire, hoping this would restart economic growth. Our results above, however, suggest that repealing the S-I-P orders should not matter much as long if people still fear the spread of the virus. We examine this in more detail by allowing S-I-P repeals to have a different coefficient than S-I-P impositions. Specifically, our repeal variable equals one when a jurisdiction repeals its sheltering order, so the total effect of a repeal equals the negative of the S-I-P order coefficient (i.e., as it turns from 1 to 0) plus the repeal coefficient. As seen in the table, the repeal coefficient is small, negative, and not significantly different from zero. Thus the effect of repealing a S-I-P order is statistically the mirror image of imposing one, and certainly no larger. The point estimates imply economic activity fell 8% when governments instituted the orders and rose 5% when they repealed them.

Repealing lockdowns may not a particularly powerful tool for restarting growth. If people are otherwise concerned about potential infection, lifting legal restrictions on their activity has limited effect. Moreover, such a policy would have to be balanced against the fact that S-I-P orders may slow the spread of the disease—see, e.g., Baker et al. (2020), Chen et al. (2020), Dave et al. (2020b, 2020c), or Friedson et al. (2020). If repealing lockdowns leads to a fast enough increase in COVID infections and deaths and a concomitant withdrawal of consumers from the market place, they might ultimately end up harming business activity.

4. Shifting: Time and Geography

In Table 3 we look for evidence of shifting/gaming of S-I-P orders. High frequency data such as ours can give a misleading picture of policy impacts if, in the week prior to the policy being put into place, people rush to engage in economic activity that would have otherwise waited until later. Comparing before-and-after activity levels will overstate the effect of the policy because of this intertemporal substitution. Similarly, the estimated impact of lockdowns will overstate their true effect if consumers shift their commercial activity across borders. If customers in, say, Memphis, Tennessee simply drove to Arkansas (where there was no statewide S-I-P order) to get their hair cut when Memphis was under a sheltering order, it will look like the order causes a drop in activity even though overall commercial activity did not change.

We investigate intertemporal shifting in column (1) of Table 3. Here, we include one-week lags and leads of the policy. There is no evidence of anticipatory increases in consumer activity in the week before S-I-P enactment and there is evidence that policy effects exhibit persistence once imposed.

In column (2) we measure geographic shifting using as our dependent variable SafeGraph data on the average distance traveled to a business among its customers that week. If the sort of

cross-border shifting of activity from S-I-P jurisdictions to non-S-I-P jurisdictions is occurring, we should see the average distance traveled rise substantially when S-I-P orders go into effect. We find no such pattern; the point estimate is small, statistically insignificant, and negative.

These two pieces of evidence indicate that the effects of S-I-P orders, such as they are, do not seem to induce a lot of intertemporal or spatial shifting of economic activity. Further, it is worth noting that to the extent that any such shifting does occur, this will result in our estimates overstating the true economic effect of S-I-P policies, meaning that even their modest size is an upper bound.

5. Fear and the Choice of Big Versus Small Business

In this section we document differential patterns in the slowdown across stores of different sizes. People afraid of infection may avoid larger, busier stores in favor of smaller options with fewer visitors. Our results indicate this is what happened, further suggesting fear of the virus is an overriding determinant of people's decisions to engage in economic activity.

We divide the businesses up within their state-by-industry cell based on their size/traffic before COVID arrived (we use the total number of consumer visits to the location in January). We then classify each establishment into one of three size groupings within its state-industry: smallest 20%, middle 60% and largest 20%. For instance, we rank all Grocery Stores in Wisconsin by their traffic in January; the busiest 20% are in the top size/traffic category, and so on.

We first regress the number of weekly visits to a business on establishment fixed effects and separate week fixed effects for each of the three business size quantiles. We plot these week-by-quantile fixed effects in Figure 2. Activity falls for all businesses, but falls dramatically more for large, high-traffic businesses than for smaller, less busy ones. At the trough, traffic is down over 70% at the largest establishments, but only about 45% at the smallest ones. Consumers are

substituting away relatively more from industry businesses that pose a greater probability of contact with others.¹

In column (1) of Table 4, we measure this differential size response statistically by looking at the change in establishments' log daily visits from January 2020 to the trough week of April 12 as a function of the establishment's size quantile within its industry-state. Relative to their industry cohorts in the middle 60% of the size distribution, small businesses had considerably more traffic at the trough (they had lost traffic on average, but considerably less than the larger businesses did). The difference is about 50 log points, or over 60%. Conversely, the largest 20% of establishments saw a larger decline in traffic, about 30% more, than did the middle quantile.

Column (2) interacts the number of local COVID deaths with the business size categories. Localities where the disease is more prevalent see a more pronounced relative shift away from large businesses and toward small ones, consistent with fear of infection driving consumer behavior. Column (3) shows that S-I-P policies themselves also lead to larger shifts away from the biggest establishments.

6. Lockdowns and Business Diversion

The evidence points to a modest impact of shutdown orders on aggregate economic activity. However, the orders can still have a significant impact on the *types* of businesses that consumer visit. We see that in the size results above, but potentially even more extreme responses might be induced when shutdown orders target specific types of businesses. In this section, we use the information from Goolsbee et al. (2020) on government restrictions on activity at restaurants and bars and,

¹ We repeated this exercise using data from the same time period in 2019 to investigate if this might be just a seasonal effect. It did not show the same pattern. We also examined whether survivor bias might make it only seem that small businesses do better, because small firms that die do not get counted. Imputing zero visits for missing firms and using the inverse hyperbolic sine transformation for visits yielded the same basic patterns as in Figure 2, however.

separately, restrictions of ‘non-essential’ businesses (and which industries the policies classified as ‘non-essential’).² The results show substantial reallocations across types of businesses.

Table 5 interacts these policy measures with indicators for the type of business. The results indicate that, indeed, even though general S-I-P orders reduced consumer visits by only around 5%, orders limiting the activities of defined “non-essential” business reduce visits to those establishments by a massive amount while at the same time increasing activity by roughly the same magnitude at “essential” business. Similarly, restaurant and bar restrictions reduced consumer visits to bars and restaurants by almost 30%, but they increased visits to non-restaurant food and beverage stores by 27%, and visits to all other businesses slightly.

7. Conclusion

The COVID-19 crisis led to an enormous reduction in economic activity. We estimate that the vast majority of this drop is due to individuals’ voluntary decisions to disengage from commerce rather than government-imposed restrictions on activity. Several patterns in the data are consistent with these decisions reflecting people’s concerns that commerce may expose them to the disease. We do not find evidence of large temporal or spatial shifting in response to shelter-in-place policies. While their aggregate effect is modest, restrictions on activity that target particular types of businesses do induce large reallocations of activity away from “disallowed” businesses and toward “allowed” ones.

Our results come with caveats. While we do have data on 110 industry groupings—mostly retail, personal services, restaurants and bars, and recreational industries—where customer foot traffic is a reasonable proxy for economic activity, we do not measure policies’ effects on activity in

² We were not able to find essential business definitions systematically at the county level, so we are relying on the state definitions even in the counties that acted before their states.

other sectors. Moreover, we cannot measure the dollar volume of transactions per visit, though if we weight by the average of such volume in the pre-COVID period, our results remain.

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TABLE 1: STANDARD POLICY ESTIMATE: LN (VISITS/DAY)

	(1)	(2)	(3)	(4)
S-I-P Order	-0.714 (0.015)	-0.599 (0.016)	-0.545 (0.016)	-0.076 (0.011)
State S-I-P		-0.124 (0.014)	0.022 (0.017)	
ln(County deaths) [asinh transf]			-0.102 (0.006)	-0.030 (0.005)
N	23,865,724	23,865,724	23,865,724	23,865,721
R ²	0.853	0.853	0.858	0.880
FEs	Store	Store	Store	Store C-Zone x Week
Weights: Cluster SE:	Visits in Jan County	Visits in Jan County	Visits in Jan County	Visits in Jan County

Notes: The dependent variable is log number of average consumer visits per day to the store. S-I-P Order is the measure of shelter-in-place at the county level or at the state level as described in the text. The measure of County deaths is the log of an inverse hyperbolic sine transformation of the number of deaths in the county to account for the many zeros. The standard errors are clustered at the county level.

TABLE 2: POLICY ESTIMATES BY SOURCE OF VARIATION: LN (VISITS/DAY)

	(1) Border	(2) No Border	(3) Exit/Repeal
S-I-P Order	-0.068 (0.014)	-0.080 (0.014)	-0.082 (0.011)
Repeal Order			-0.030 (0.028)
ln(County deaths) [asinh transf]	-0.032 (0.011)	-0.042 (0.006)	-0.039 (0.005)
N	6,391,240	17,474,481	23,865,721
R ²	0.873	0.882	0.880
FEs	Store	Store	Store
	CZ x Week	CZ x Week	CZ x Week
Weights:	Visits in Jan	Visits in Jan	Visits in Jan
Cluster SE:	County	County	County

Notes: The dependent variable is log number of average consumer visits per day to the store. S-I-P Order is the measure of shelter-in-place at the county level as described in the text. Repeal Order indicates locations where they repeal or let their order expire. The measure of County deaths is the log of an inverse hyperbolic sine transformation of the number of deaths in the county to account for the many zeros. The standard errors are clustered at the county level.

TABLE 3: SHIFTING

	(1) Intertemporal	(2) Ln(Distance)
S-I-P Order (t+1)	-0.008 (0.009)	
S-I-P Order (t)	-0.064 (0.010)	-0.015 (0.013)
S-I-P Order (t-1)	-0.054 (0.009)	
ln(county deaths) [asinh transf]	-0.039 (0.005)	-0.001 (0.005)
N	23,285,721	17,645,439
R ²	0.880	0.780
FEs	Store	Store
	CZ x Week	CZ x Week
Weights:	Visits in Jan	Visits in Jan
Cluster SE:	County	County

Notes: The dependent variable is log number of average consumer visits per day to the store in (1) and the log of average distance traveled to the store in (2). S-I-P Order is the measure of shelter-in-place at the county level as described in the text and the time script indicates whether the measure is contemporaneous, lagged or led one week. The measure of County deaths is the log of an inverse hyperbolic sine transformation of the number of deaths in the county to account for the many zeros. The standard errors are clustered at the county level.

TABLE 4: SIZE OF BUSINESS: CHANGE LN(VISITS/DAY): JAN. TO APRIL 12

	(1) SIZE	(2) DEATHS	(3) POLICY
{S=1: Small 20%}	0.491 (0.004)	0.445 (0.011)	0.400 (0.021)
{L=1: Large 20%}	-0.352 (0.004)	-0.239 (0.019)	-0.259 (0.022)
ln(county deaths)		-0.070 (0.010)	-0.088 (0.010)
ln(county deaths) x {S=1}		0.014 (0.005)	
ln(county deaths) x {L=1}		-0.032 (0.007)	
S-I-P Order			-0.039 (0.064)
S-I-P Order x {S=1}			0.084 (0.022)
S-I-P Order x {L=1}			-0.094 (0.026)
N	2,106,343	2,106,343	2,106,343
R ²	0.080	0.080	0.882
FEs	CZ	CZ	CZ
Cluster SE:	County	County	County

Notes: The dependent variable is the change in log number of average consumer visits per day to the store from January to the week of April 12th. The {S=1} variable indicates a firm is in the smallest 20% of firms in its state x industry measured as total visits in the month of January. The {L=1} variable indicates a firm in the largest 20% of firms by the same measure. The measure of County deaths is the log of an inverse hyperbolic sine transformation of the number of deaths in the county to account for the many zeros. The standard errors are clustered at the county level.

TABLE 5: BUSINESS DIVERSION

	(1)
S-I-P Order	-0.046 (0.017)
Restaurant Order x {Restaurant=1}	-0.289 (0.008)
Restaurant Order x {Food=1}	0.275 (0.009)
Restaurant Order	0.054 (0.013)
Essential Biz Order x {Essential=1}	0.475 (0.012)
Essential Biz Order	-0.382 (0.028)
Ln (cnty deaths) [asinh transform]	-0.035 (0.005)
N	23,865,721
R ²	0.885
FES	Store
	CZ x Week
Cluster SE:	County x Essential

Notes: The dependent variable is log number of average consumer visits per day to the store. S-I-P Order is the measure of shelter-in-place at the county level as described in the text. The other variables define essential and non-essential businesses, restaurants and bars, and non-restaurant food and beverage businesses as described in the text. The measure of County deaths is the log of an inverse hyperbolic sine transformation of the number of deaths in the county to account for the many zeros. The standard errors are clustered at the county x essential business level.

APPENDIX TABLE: CHANGE IN LN(VISITS/DAY): JAN. TO APRIL 12

Worst 15 industries	$\Delta \ln(v/\text{day})$	Best 15 industries	$\Delta \ln(v/\text{day})$
711190 Other Perf. Arts	-4.33	444210 Outdoor pwr eq stores	+0.17
711110 Theaters	-3.85	444220 Nurse/grdn/farm s.	+0.03
713920 Skiing facilities	-3.60	713910 Golf courses	+0.01
712130 Botanic gardens, zoos	-3.49	811411 Home & garden eq rpr	-0.18
811219 Other elec eq rpr	-3.16	541940 Veterinary services	-0.57
711211 Sports teams	-2.50	444130 Hardware store	-0.60
512131 Motion picture thtrs	-2.44	722320 Caterers	-0.62
448150 Clothing acc. stores	-2.35	447190 Gasoline stations	-0.63
711219 Other spect sports	-2.10	445110 Supermarkets	-0.63
713950 Bowling centers	-2.08	445120 Convenience stores	-0.64
448320 Luggage stores	-1.93	454310 Fuel dealers	-0.66
722410 Drinking places (alc)	-1.90	441222 Boat dealers	-0.67
448140 Family clothing s.	-1.87	441228 Motorcycle, atv dealers	-0.67
812990 Other pers services	-1.82	441310 Auto parts stores	-0.69
713940 Fitness centers	-1.75	446110 Pharmacies	-0.72

Notes: This is the raw change in the log number of visits per day from January 2020 to the week of April 12th by industry for the worst performing and best performing 6-digit NAICS codes in our sample.

FIGURE 1: AGGREGATE CONSUMER VISITS OVER TIME

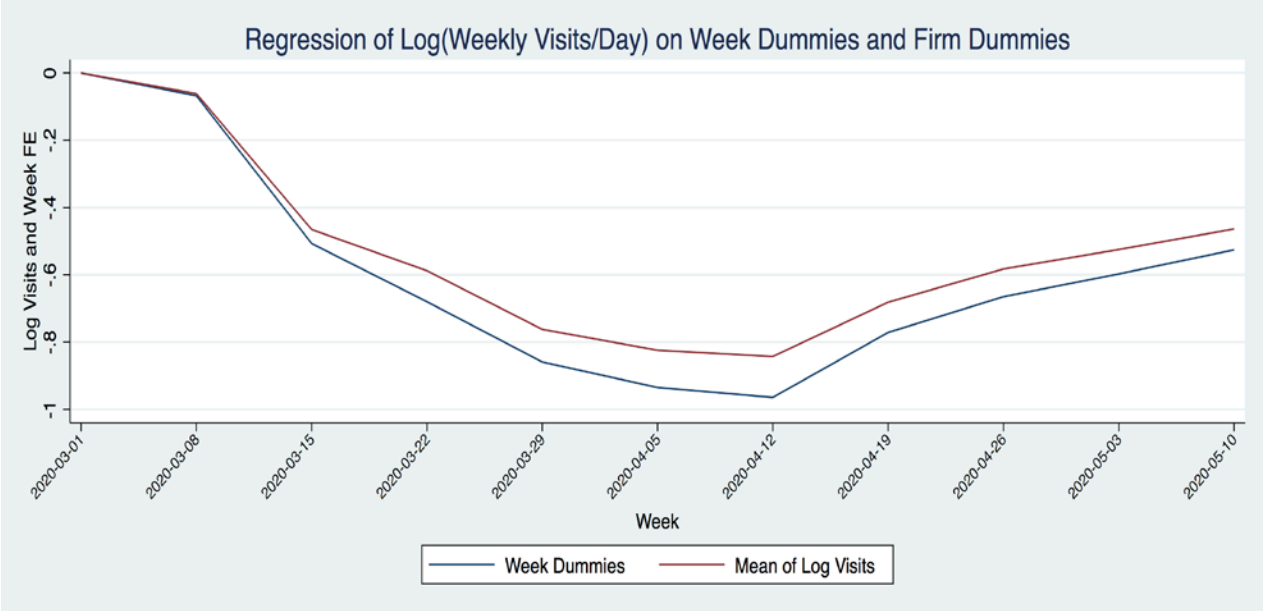


FIGURE 2: CONSUMER VISITS OVER TIME BY STORE SIZE/TRAFFIC

