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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 collapse resulted from government-imposed restrictions versus people voluntarily choosing to stay home to avoid infection. This paper examines the drivers of the economic slowdown using cellular phone records data on customer visits to more than 2.25 million individual businesses across 110 different industries. Comparing consumer behavior over the crisis within the same commuting zones but across state and county boundaries with different policy regimes suggests that legal shutdown orders account for only a modest share of the massive changes to consumer behavior (and that tracking county-level policy conditions is significantly more accurate than using state-level policies alone). While overall consumer traffic fell by 60 percentage points, legal restrictions explain only 7 percentage points of this. 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 influenced by the number of COVID deaths reported in the county; and showed a clear shift by consumers away from busier, more crowded stores toward smaller, less busy stores in the same industry. States that repealed their shutdown orders saw symmetric, modest recoveries in consumer visits, further supporting the small estimated effect of policy. Although the shutdown orders had little aggregate impact, they did have a significant effect in reallocating consumer visits away from "nonessential" to "essential" businesses and from restaurants and bars toward groceries and other food sellers.

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 the orders' impact on slowing the spread of the virus outweighed the economic damage they did.

It is not clear, however, that the economic decline actually came from the lockdown orders. Those lockdown orders were an endogenous policy response to the arrival of the disease. By many accounts, anxious individuals engaged in physical distancing on their own accord and understanding the size of that effect is critical for evaluating the 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).

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We collect data on the shutdown policy conditions at the county and city 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.[1] The results confirm the findings in Goolsbee et al. (2020) that using the local data on policy is a substantial improvement over the more commonly used state level measures.

Overall, the results in this paper 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 dividing line, both saw enormous drops in customer visits. 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 visiting stores.

We find evidence tying this voluntary decline in consumer visits 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 for establishments that were busier/larger before COVID. This is consistent with greater avoidance of and substitution away from establishments with higher potential transmission contacts and this is true in industries where there is no online sales alternative (e.g., auto-repair, hair salons, etc.).

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 consumer visits 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 visits that require interacting with others.

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

Because there is a rapidly expanding, contemporaneous empirical literature examining the COVID-19 pandemic, it difficult comprehensively to describe findings in the rest of the literature. Our study is related to two major strands of this work. One involves studies using cellular phone data to track how fear of the virus or lockdown orders have affected personal mobility and interactions. Important 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), Cronin and Evans (2020), Dave et al. (2020a), Fang et al. (2020), Goldfarb and Tucker (2020), Gupta et al. (2020a, 2020b), Maloney and Taskin (2020) and Nguyen et al. (2020), Amuedo-Dorantes et al. (2020), Benzell et al. (2020), Chernozhukov et al. (2020), Hsiang et al. (2020), Watanabe and Yabu (2020), Wilson (2020). A second area of related work includes studies that focus directly on the economic impact of lockdown policy, though often with different data. Papers using employment and jobs data include Forsythe et al. (2020), Rojas et al. (2020), Bartik et al. (2020), Bartik et al. (2020), Gupta et al. (2020a), and Aum et al. (2020). Other work also has included data on various measures of private sector spending such as Alexander and Karger (2020), Chetty et al. (2020), and Coibon et al. (2020).

Our paper differs from previous studies primarily in using an identification strategy of comparing businesses across counties within the same commuting zone during the same week. This allows us to flexibly control for other non-policy influences on consumer visits in a region (such as the underlying level of fear of infection) that might otherwise make lockdown policy endogenous. In addition, because we have the phone mobility records at the level of the individual business, we are able to document some consumer substitution patterns in a way that previous work has not. These patterns offer further insight into the sources of the consumer visit behavior that we document.

1. Data

Our data come from the SafeGraph panel of mobile phone usage (see SafeGraph, 2020 or Squire, 2019, 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 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 a train 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.

We measure consumer visits, not expenditures. If people shop half as frequently but spend twice as much each time they go out, we would not observe that behavior. We do not observe online spending, either (though we will look at some in-person industries where online commerce is not applicable). 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).

We collected local policy measures directly from searches as described in Goolsbee et al. (2020). That paper documents the data in detail; the data are freely available to other researchers.

2. The problem of confounding lockdown with fear

Fig. 1 shows the precipitous drop and partial recovery in visits to businesses in the SafeGraph data over March, April, and half of 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

[1] Important exceptions in the literature include Alexander and Karger (2020), Brzezinski et al. (2020) Cronin and Evans (2020) and Gupta et al. (2020a, 2020b) who use county level information similar to ours here. Those papers do not use the same type of identifying variation within commuting zone that we do, however.

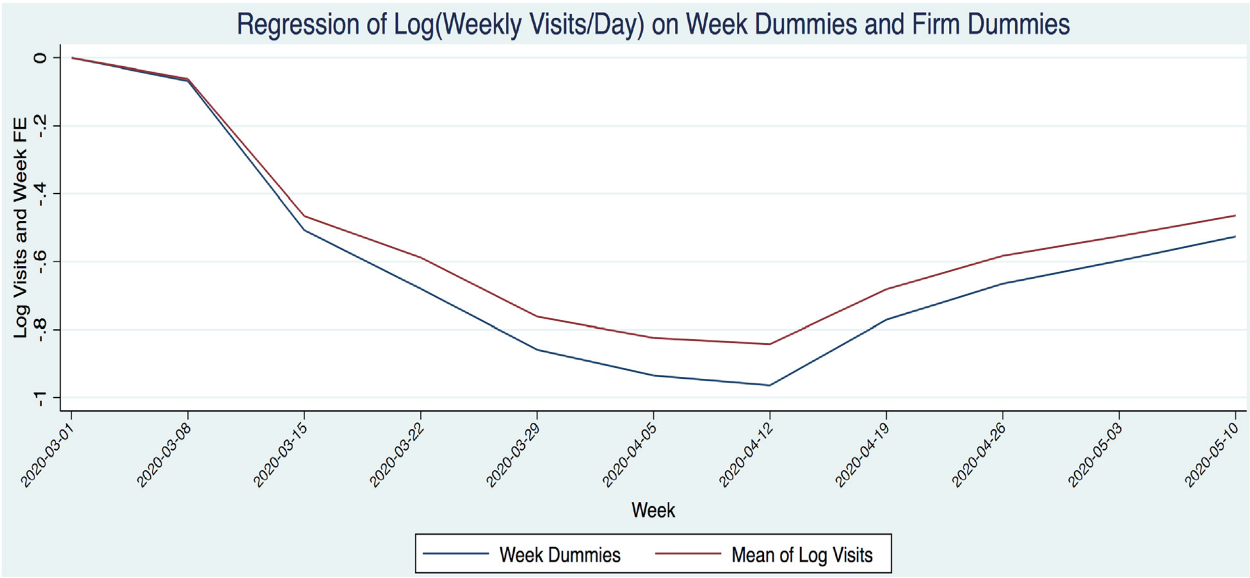

Fig. 1. Aggregate consumer visits over time.

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 a 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 Fig. 1 shows a considerable collapse of consumer visits before most shutdown orders were in place. This pre-trend poses a significant problem with interpreting any policy coefficient identified from the time series variation.

The basic problem is clear in Table 1. Here we 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 that does not deal with the simultaneity of rising fear and sheltering policy decisions is presented in column (1) and suggests a massive effect of S-I-P orders corresponding to a more than 70 log point decline in visits.

In column (2) we add the cumulative number of COVID deaths in the county to the basic regression. 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 et al., 1988). Local deaths are strongly related to the size of the reduction in consumer visits. Further, controlling for deaths reduces the estimated impact of county S-I-P policy by 25%.

Then, in column (3) we introduce our identification strategy by adding commuting-zone-by-week fixed effects. These fixed effects will 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 (3) to those in the table's other columns is important. It shows the correlation between the decline in consumer visits 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 visits. People greatly reduced their visits 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.

Fig. 2 shows why we use the control group methodology by exploring the pre-trends in visits for the treatment counties and control-group counties in the same commuting zone. The blue line in the figure plots the logged average number of visits in event time relative to the week that the county enacted an S-I-P order (normed to be zero in the event week so everything is relative to that). It shows a significant pre-trend before the policy comes into effect, just like in Fig. 1. The red line, however, shows this trend in visits in neighboring counties that did not put an S-I-P order in place in that same reference week. The pattern of consumer visits shows close to an identical pre-trend. Since our results are identified by the relative response, using this source of variation largely eliminates the conventional pre-trend bias problem.

The results in column (3) 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 still strongly correlated with declines in consumer visits. Within the same commuting zone in the same week, more deaths in a county significantly reduces local consumer visits. 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 consumer visits of around 30% or half the total decline observed in the data.

Column (4) confirms the finding in Goolsbee et al. (2020) that the standard state level policy variables have no effect once when added to the regression using the local policy data. The local data is

Table 1
Standard policy estimate: LN (visits/day).

	(1)	(2)	(3)	(4)
S-I-P Order	−0.733	−0.569	−0.077	−0.082
	(0.015)	(0.010)	(0.012)	(0.015)
State S-I-P				0.014
				(0.018)
ln(County deaths)		−0.075	−0.039	−0.039
[asinh transf]		(0.004)	(0.005)	(0.005)
N	23,865,724	23,865,724	23,865,721	23,865,721
R^2	0.854	0.860	0.880	0.880
FEs	Store	Store	Store	Store
			C-Zone x Week	C-Zone x Week
Weights:	Visits in Jan	Visits in Jan	Visits in Jan	Visits in Jan
Cluster SE:	County	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 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.

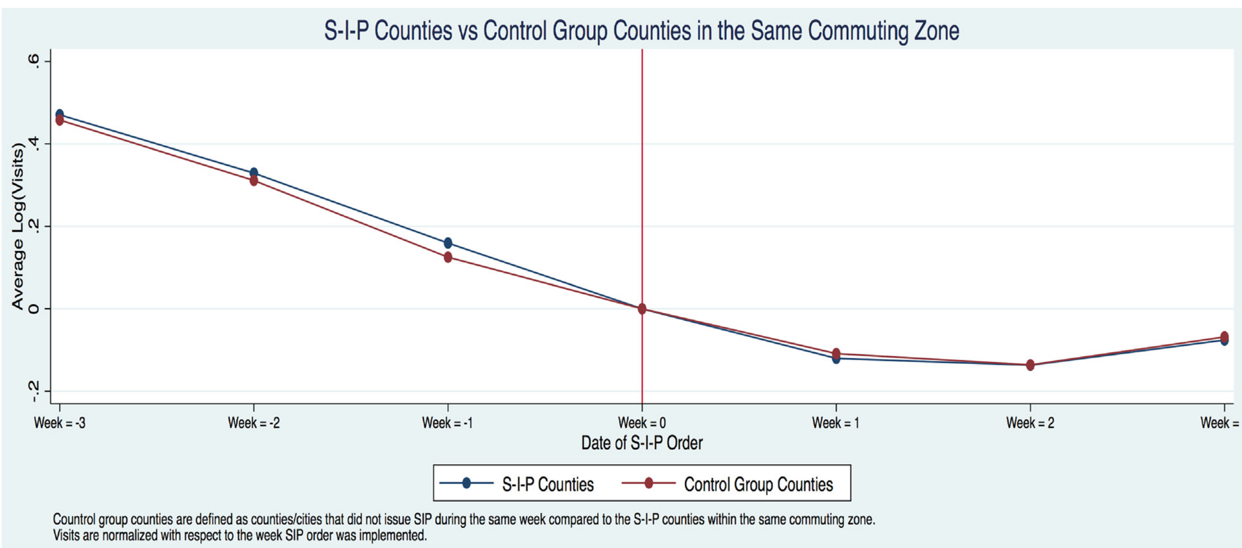

Fig. 2. Average log visits per day around S-I-P order date.

more accurate and the more commonly used measures simply add noise.

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 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 as 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

Table 2
Policy estimates by source of variation: LN (visits/day).

	(1) Border	(2) No Border	(3) Exit/Repeal
S-I-P Order	−0.081	−0.075	−0.074
	(0.015)	(0.018)	(0.013)
Repeal Order			0.008
			(0.020)
ln(County deaths)	−0.032	−0.042	−0.039
[asinh transf]	(0.011)	(0.006)	(0.005)
N	6,391,240	17,474,481	23,865,721
R^2	0.873	0.882	0.880
FEs	Store	Store	Store
Weights:	CZ × Week	CZ × Week	CZ × Week
Cluster SE:	Visits in Jan	Visits in Jan	Visits in Jan
	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.

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 consumer visits fell 7.4% when governments instituted the orders and rose 6.8% 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: anticipation and arbitrage

In Table 3 we look for evidence of shifting/gaming of S-I-P orders. High frequency data such as ours can overstate policy impacts if, in the week prior to the policy being put into place, people rush to engage in consumer visits that would have otherwise waited until later. Comparing before-and-after activity levels may 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 visits even though the overall number of visits did not change.

We investigate intertemporal shifting in column (1) of Table 3. Here, we add to our normal specification a simple weekly dummy equal to 1 in the week before an S-I-P order goes into effect. Interestingly, while the data and the previous literature has shown a strong trend decline in activity before sheltering policies were enacted,[2] our within metro-area identification strategy documents a small short-run anticipatory effect for the week before cities put the policy in place (i.e., the opposite of the pre-trend). People seem to be making extra visits before the rule comes into effect. While all the coefficients are small and of similar magnitudes, the combined coefficients suggest an overall impact about −9% (the sum of the coefficients in absolute value) but with 3.5% of that coming from the temporary anticipation and the full impact of the S-I-P order being −5.6%.

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 would expect to see the average distance traveled rise 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 consumer visits. 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.

[2] See the discussion of pre-trends in Alexander and Karger (2020) or Cronin and Evans (2020).

Table 3
Shifting.

	(1) Intertemporal	(2) Ln(Distance)
S-I-P Order	−0.0565	−0.007
	(0.017)	(0.015)
1 Week Ahead	0.035	
(Anticipation effect)	(0.013)	
ln(county deaths)	−0.039	−0.001
[asinh transf]	(0.005)	(0.005)
N	23,285,721	17,645,439
R^2	0.880	0.780
FEs	Store	Store
	CZ × Week	CZ × 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.

5. Fear and the choice of big versus small business

In this section we document several pieces of evidence that fear drives the decline rather than policy. First, in column (1) of Table (4) we follow the idea of Cronin and Evans (2020) that state declarations of a public health emergency, which happened quite a bit earlier than the S-I-P orders, gave governors additional powers and enabled them to tap emergency funding sources but did not have much direct impact on consumers. In our context, it is a policy that affects consumer visits mainly through the fear channel. The results confirm Cronin and Evans' broader finding in this setting: emergency declarations reduce consumer visits.

Second, 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 consumers' decisions about where to visit.

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 Fig. 3. Visits fall for all businesses, but fall 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.[3]

[3] 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 Fig. 2, however.

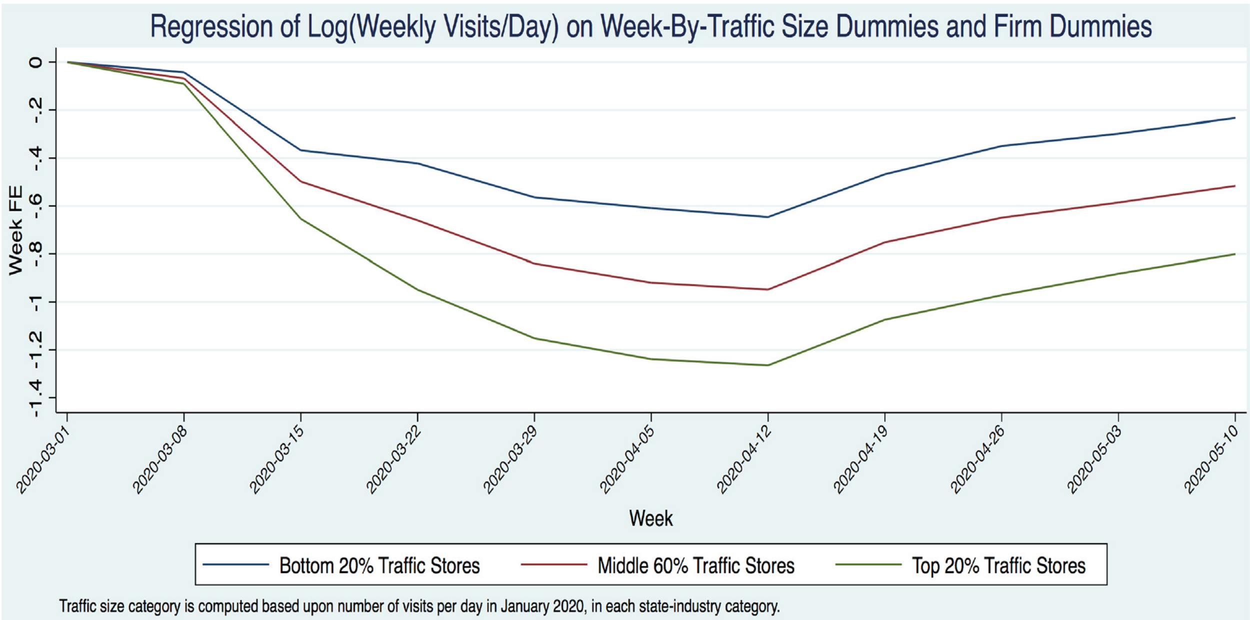

Fig. 3. Consumer visits over time by store size/traffic.

Table 4
Size of business: change LN(visits/day): Jan. to April 12.

	(1) EMERGENCY	(2) SIZE	(3) NON-RETAIL	(4) DEATHS	(5) S-I-P
{S=1: Small 20%}		0.491 (0.006)	0.543 (0.008)	0.445 (0.012)	0.401 (0.021)
{L=1: Large 20%}		−0.352 (0.013)	−0.643 (0.015)	−0.239 (0.019)	−0.243 (0.022)
ln(county deaths)	−0.039 (0.005)			−0.070 (0.010)	−0.087 (0.010)
ln(county deaths) × {S=1}				0.014 (0.005)	
ln(county deaths) × {L=1}				−0.032 (0.007)	
Emergency Declaration	−0.082 (0.034)				
S-I-P Order	−0.077 (0.012)				−0.174 (0.052)
S-I-P Order × {S=1}					0.084 (0.022)
S-I-P Order × {L=1}					−0.111 (0.026)
N	23,865,721	2,106,343	1,115,330	2,106,343	2,106,343
R^2	0.885	0.075	0.118	0.081	0.080
FEs	Store, CZ × Week	CZ	CZ	CZ	CZ
Cluster SE:	County	County	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 × 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.

In column (2) 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.

This is not likely to be an artifact of the larger stores being more able to switch to online shopping than small stores. In column (3) we restrict the sample to the more than half of our industries that are not in the retail sector where online commerce does not exist (e.g., barbers, hotels, auto repair shops, etc.). The results show the identical pattern but even more pronounced for businesses where there is not an online option.

Column (4) 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 (5) shows that the S-I-P orders did the same thing when they were announced.

6. Lockdowns and business diversion

The evidence points to a modest impact of shutdown orders on aggregate consumer visits. However, the orders could still have a significant impact on the *types* of businesses that consumers visit. We see that in the size results above, but potentially even more

Table 5
Business diversion.

	(1)
S-I-P Order	−0.028 (0.019)
Restaurant Order × {Restaurant = 1}	−0.318 (0.006)
Restaurant Order × {Food = 1}	0.251 (0.008)
Restaurant Order	0.080 (0.012)
Essential Biz Order × {Essential = 1}	0.489 (0.009)
Essential Biz Order	−0.419 (0.022)
Ln (cnty deaths) [asinh transform]	−0.035 (0.005)
N	23,865,721
R^2	0.885
FEs	Store CZ × Week
Cluster SE:	County × 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 × essential business level.

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 visits at restaurants and bars and, separately, restrictions of 'non-essential' businesses (and which industries the policies classified as 'non-essential').[4] 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 reduced visits to those establishments by a massive amount while at the same time increasing visits 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.

It is important to note that documenting diversion does not imply that the sheltering policy was a failure. The purpose of the sheltering policy was to slow the rate of spread of the virus and this kind of business reallocation might do exactly that. If restaurants and bars are more dangerous than grocery stores and liquor stores, diverting visits from one to the other could be fulfilling the main goal of the policy.[5]

7. Conclusion

The COVID-19 crisis led to an enormous reduction in consumer visits. 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 visits away from "disallowed" businesses and toward "allowed" ones.

[4] 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. These findings are similar to the results in Alexander and Karger (2020).

[5] Simulation work on selective lockdowns includes work like Baqaee et al. (2020) and Birge et al. (2020), For work estimating the impact of sheltering and other policies on the transmission of COVID see, among others, Berry et al. (2020), Chen et al (2020), Dave et al (2020a), Fowler et al. (2020).

Appendix Table: Change in LN(visits/day): Jan. to April 12

Worst 15 industries	Δln (v/day)	Best 15 industries	Δln (v/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.

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