Do localised lockdowns cause labour market externalities?

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Abstract

The business restrictions introduced during the Covid-19 pandemic greatly affected the labour market. However, quantifying their costs is not trivial as local policies affect neighbouring areas through spillovers. Exploiting the U.S. local variation in restrictions and commuting, we estimate the causal direct and mobility spillover impacts of lockdowns. Mobility spillovers alone account for 10-15% of U.S. job losses at peak. We corroborate these results with causal evidence for a consumption-based mechanism: shops whose clients reside in higher proportion in neighbouring areas under lockdown experience larger employment losses. Not accounting for mobility spillovers leads to overestimating direct lockdown effects, but underestimating total ones.

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1 Introduction

Since the appearance of Covid-19 in December 2019, economic activity around the globe has experienced an unprecedented disruption. Given the lack of effective treatments or vaccines during the initial stages of the pandemic, public health authorities were forced to rely on non-pharmaceutical interventions to curb the virus's spread, often restricting non-essential economic activity.

In the United States, no such policy was implemented at the federal level between January and June 2020. The decision was left to local officials, who enacted policies based on their own judgement, public health conditions and local institutional constraints, creating significant variation in the intensity of restrictions across time and space. The first set of stay-at-home orders were implemented as early as 15th March 2020, while the last were introduced almost three weeks later in South Carolina.

However, since the interconnectedness of geographically distant locations is one of the defining features of modern economies, these local measures are likely to have consequences beyond their immediate geographic scope. Indeed, a quarter of all U.S. workers commutes across county, state or even country borders on their way to work. Consumers look for goods and services outside their immediate neighbourhood (Davis et al., 2019). Supply chains stretch nation- and even world-wide. Consequently, policies restricting local economic activity are likely to generate negative spillover effects in both neighbouring and distant jurisdictions.

In this paper, we quantify the importance of commuting and consumption-related mobility in explaining the spatial diffusion of the labour market effects of local business restrictions enacted between February and June 2020. We define these externalities as *mobility spillovers*. First, we show through a simple decomposition framework that an unbiased estimate of the effects of lockdowns on employment must take into account the degree of connectedness between counties due to mobility. Second, we estimate this decomposition by OLS and find a statistically significant negative association between our measure of mobility spillovers and employment outcomes. Third, we exploit exogenous variations in exposure to neighbouring states' lockdowns to provide evidence supporting a causal interpretation of these mobility spillovers estimates: additional restrictions in neighbours to which a county is connected through mobility relationships *cause* a fall in the county's employment.

We find three results regarding the effects of mobility spillovers. First, the aggregate impact of mobility spillovers on employment is substantial, accounting for 10-15% of the total fall in U.S.-wide employment between February and the peak of the first wave (April 2020). Moreover, mobility spillovers alone account for around 25% of the our estimates of the total effects of lockdowns, given by the sum of direct (due to local lockdowns) and spillover (due to neighbours' lockdowns) effects. Second, when including mobility spillovers in the estimation framework we find larger total effects of lockdowns (the sum of direct and spillover effects) but smaller direct effects, with respect to a framework that does not allow for spillovers. Third, the spatial distribution of employment losses due to lockdowns is affected by the presence of mobility spillovers. Suburban areas were the most affected by lockdowns, local or neighbours', especially due to their large interconnection with their neighbours.

Finally, leveraging detailed geolocated visitor data, we provide novel causal evidence for a channel behind mobility spillovers: travel or economic restrictions in a county cause a fall in a proxy for employment in hospitality and retail businesses premises with a large proportion of visitors from that area, compared to businesses located in the same county but with exclusively local patrons. We provide evidence for a possible mechanism at work: lockdowns and travel bans are associated with a fall in the flows of visitors going from the area targeted by the policy toward business premises located in neighbouring states.

These results are relevant for policy-makers, as business restrictions cause significant economic effects extending beyond the jurisdiction in which they are enacted. While this does not preclude localised lockdowns from being optimal[1], our results show that the

[1] All state-wide orders generate potentially large spillovers too, both within the state in which they were enacted and in bordering counties.

spillovers are non-negligible and should be accounted for. Policies such as grants schemes for businesses affected by mandatory closures should be designed taking into account the spatial externalities affecting neighbouring regions. For example, compensating only directly-affected businesses may lead to differential treatments of direct local competitors located in different jurisdictions but equally affected by the same policy.

Additionally, our results contribute to the literature on spatial economics by providing robust causal evidence for the quantitative relevance of mobility-based interconnections in determining the spatial propagation of economic shocks. As the Covid-19 shock allows to identify certain mechanisms, our results and the challenges of the approach open further venues of research into the spillover effects of local policies enacted in response to large shocks.

Related Literature

Our paper is related to a rapidly developing literature on the economic effects of the Covid-19 pandemic. Coibion et al. (2020) find that almost 20 million jobs were lost in the United States by April 2020. Baek et al. (2020) find that an additional week of exposure to a stay-at-home (SAH) order increased state unemployment claims by around 1.9% of a state's employment level. On the consumption side, Goolsbee and Syverson (2020) find that SAH orders explain only 7% of the total fall in foot-fall traffic of local POIs. We contribute by i) providing an estimate of lockdowns' effects on labour market outcomes which approximates an ideal agnostic decomposition, and ii) establishing the causal effect of spillovers originating from neighbours' orders. Extending the previous analysis and exploiting novel data, we estimate the effects of a broader range of non-pharmaceutical intervention and account for spatial mobility spillovers (externalities). Moreover, we provide evidence for a mechanism behind these mobility spillovers through a proxy for shop-level employment. Our results indirectly intersect with other contributions; for example, we provide causal evidence for facts related to those observed in Althoff et al. (2020) regarding the effect of restrictions in areas whose economy relies on commuters and consumers' mobility.

Human interaction linkages across space have been found to be relevant drivers of the pandemic spread, as in Antràs et al. (2020) and, using granular data, Chang et al. (2020). These facts have relevant implications when studying the economic impact of the Covid-19 epidemic with SIR-economic models as in Birge et al. (2020) and Giannone et al. (2020). Fajgelbaum et al. (2021) designs an optimal lockdown policy when considerable commuting flows within large urban areas are present, taking into account both epidemiological and economic aspects. Bognanni et al. (2020) studies a SIR-economic model using daily spatial mobility data and alternative data on hours worked to study the evolution of both the economy and the pandemic with different types of non-pharmaceutical interventions. With respect to the existing literature, we provide *causal* evidence for the effects of spatial spillovers on the labour market, justifying previous theoretical analysis. Moreover, we further exploit the detail and depth of available mobility data, bringing our analysis of mobility matrices to a sectorial and even shop-specific level. Using POI-level data, we provide evidence for an underlying microeconomic mechanism at work in both our and other studies' theoretical models and empirical results, justifying the need for accounting for mobility dynamics and spatial economic interconnections when studying the effects of business and travel restrictions.

Finally, we differentiate between the extensive (how long) and intensive (how many workers are affected) margins of spatial restrictions. Contributions such as Borri et al. (2022) and Sauvagnat et al. (2020) use similar distinctions to estimate the restrictions' economic costs and social benefits. Using mobility data to map spatial economic interconnections, we further decompose these costs between those arising due to "own" and "neighbours" restrictions on both the extensive and intensive margins.

While we focus on commuting and consumption (mobility) linkages, several papers have dealt with supply-chain disruption effects. See Acemoglu and Tahbaz-Salehi (2020) and Woodford (2020) for a theoretical treatment and Meier and Pinto (2020) and Bodenstein et al. (2020) for empirical results. We recognise that the two approaches should complement each other to fully assess the spatial effects of local restrictions, as both channels can be relevant. While we provide a robustness check where we control for a proxy of spatial supply chains, our focus on mobility-related spillovers has other reasons. First, not only we provide aggregate evidence for their role, but we also provide causal evidence for a mobility-related microeconomic mechanism using granular data. Second, under some assumptions, our results can be interpreted as a lower bound of the overall effects of both mobility and supply-chain spillovers. For a more detailed discussion, see section 7.

Sections and Notation

Throughout the following pages, we will use "lockdown" as a shortcut to mean any order mandating some business closure, unless otherwise specified. These include SAH orders as well as less strict orders such closures and heavy capacity restrictions of malls, bars, restaurants, and other entertainment venues.

In Section 2 we present relevant facts about the U.S. labour market during the Covid-19 pandemic. In Section 3 we set how to reason about the spatial effects of lockdowns and provide a parsimonious framework. Section 4 presents the data, with the empirical application results in Section 5. We provide evidence for one mechanism behind these macro-level estimates in Section 6. We discuss further robustness checks and results in Section 7. Section 8 concludes.

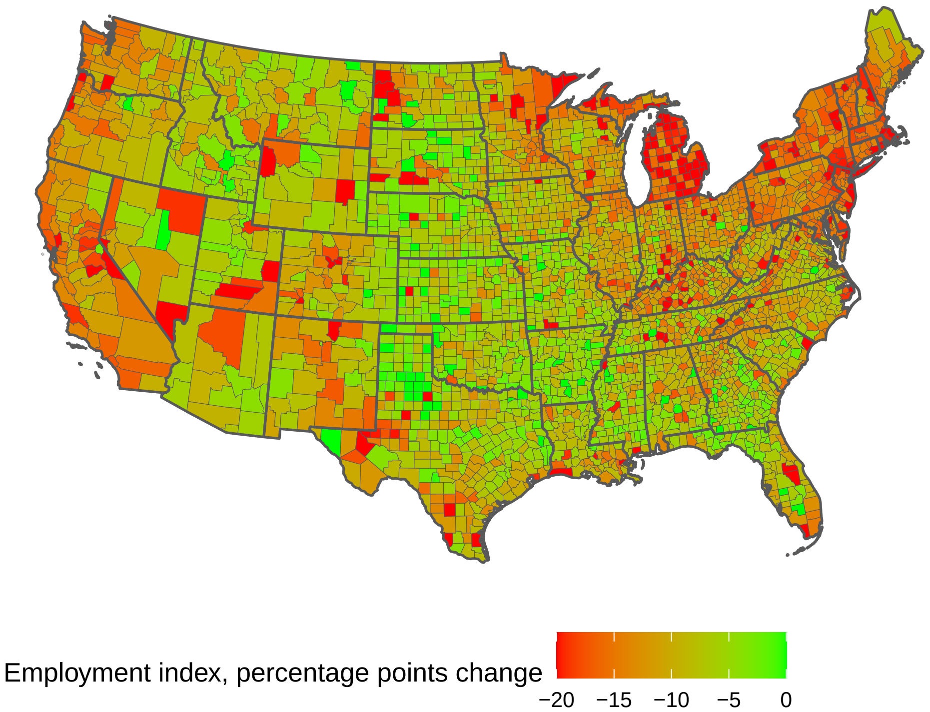
2 The labour market during the pandemic

In this section, we report a few relevant facts regarding the U.S. labour market during the first phase of the Covid-19 pandemic. Then, we connect these facts to commuting and consumption relationships with neighbours which implemented lockdown policies, showing a negative correlation between employment and neighbours' business restrictions.

The U.S. employment levels fell sharply during the "first wave" (March-June 2020) of the Covid-19 pandemic. April 2020 employment was down approximately 14% from the 2019 average, according to QCEW data. Even after the end of the harshest business restrictions, employment was still down 8% from the pre-covid baseline. However, these aggregate dynamics conceal highly heterogeneous local outcomes, as shown in Figure 1a.

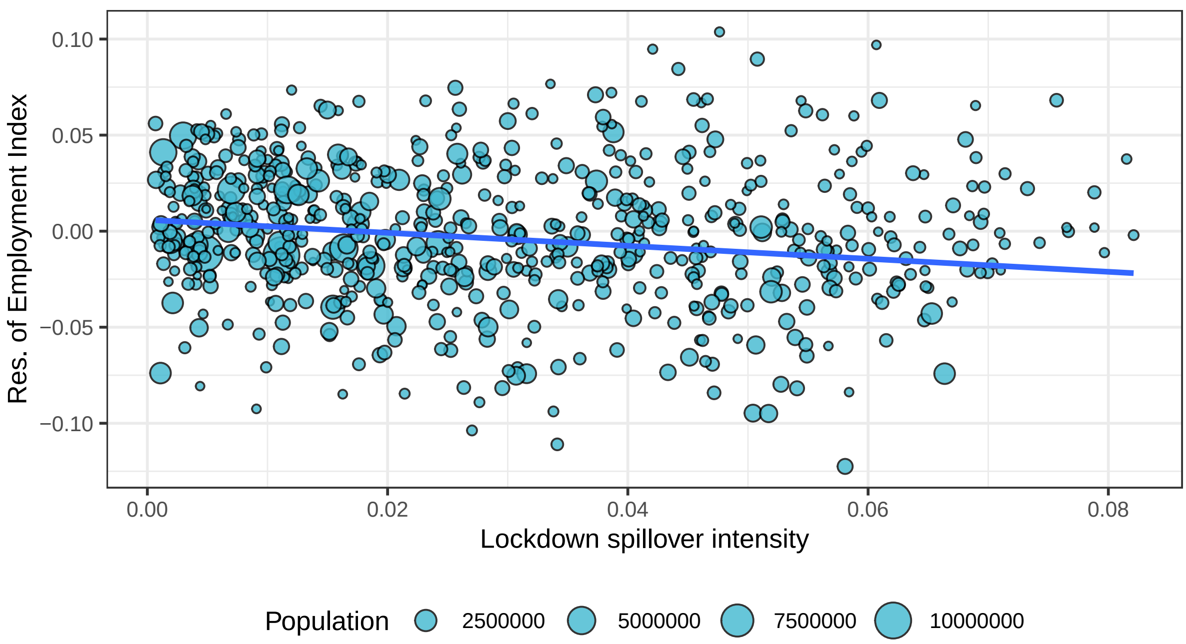
Figure 1: Labour market dynamics during the pandemic

(a) Change in employment index (1=2019 average), May 2020



Note: adjusted by the average February-May change between 2017 and 2019. Data from BLS's QCEW local area files.

(b) Employment Index and spillover intensity



Note: Residual of change in employment/population, April and May 2020, after controlling for time and countyXmonth fixed effects and the intensity of own lockdown; against lockdown intensity spillover. Only counties with a working age population over 150 000 are represented on the graph to enhance readability of the figure. The linear fit is weighted by working population.

While several rural counties in the Mid-West and South-West saw little to no changes in employment, large urban areas and - above all - their suburbs experienced an extremely large number of job losses. Table 1 shows how in "Non-Core" (highly rural) counties employment fell by 4.8% between February and May 2020. While large in absolute terms, this figure is less than half of the one experienced by "Large Fringe Metropolitan" counties (suburbs of large cities) and only one-third of the one experienced by "Large Central Metropolitan" counties.

Table 1: Summary statistics of labour market and lockdown facts, May 2020

	Large C. Metro		Large F. Metro		Medium Metro		Small Metro		Micropolitan		Non-Core	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Change in Employment Index (%)	-13.8	4.0	-10.4	4.7	-8.1	6.4	-6.7	4.6	-7.6	6.1	-4.8	7.7
Share Own Economy Closed (%)	20.4	7.9	22.6	9.6	15.1	13.3	11.8	11.6	11.8	9.5	9.7	11.4
Commuting Exposure to Neigh. Closures (%)	4.4	3.6	11.1	4.2	5.6	5.2	4.7	4.2	3.6	3.3	3.7	3.7
Consumption Exposure to Neigh. Closures (%)	5.3	3.0	8.2	5.9	6.3	4.4	5.3	3.7	6.0	3.8	7.1	6.1
Consumption Exposure to Neigh. SAH (%)	3.6	2.6	4.8	3.4	3.4	3.2	2.6	2.5	2.8	2.5	3.5	3.7

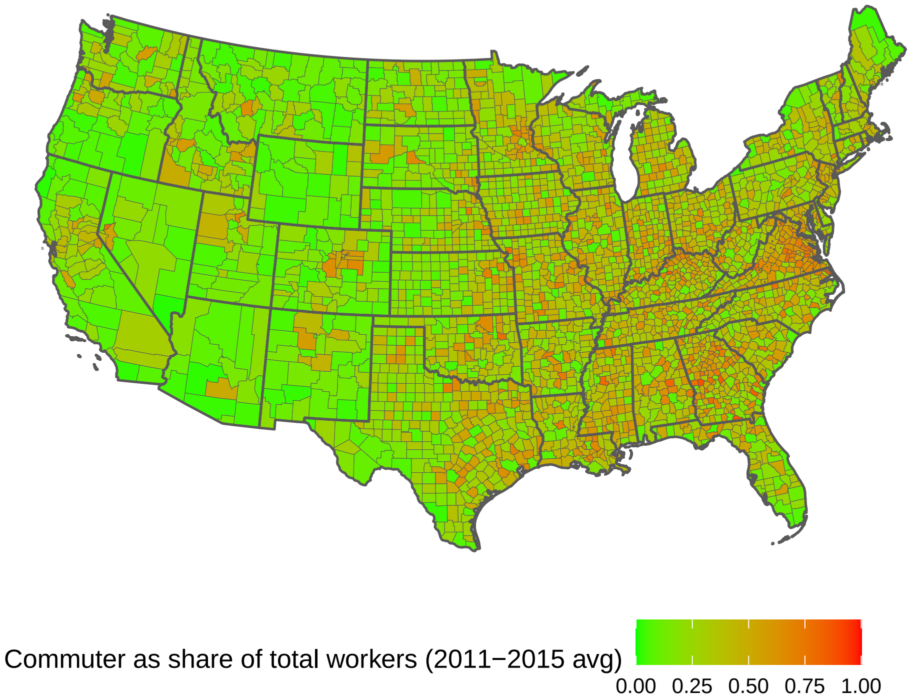
Note: Classification of counties according to National Centre for Health Statistics's 2013 classification. All data are referred to May 2020. *Large Central Metro*: counties in MSA of 1 million or more population that (1) contain the entire population of the largest city of the MSA, or (2) have their entire population contained in the largest principal city of the MSA, or (3) contain at least 250,000 inhabitants of any principal city of the MSA. *Large Fringe Metro*: counties in MSAs of 1 million or more population that did not qualify as large central metro counties. *Medium Metro*: counties in MSAs of population of 250,000 to 999,999. *Small Metro*: counties in MSAs of population less than 250,000. *Micropolitan*: counties in micropolitan statistical areas. *Noncore*: nonmetropolitan counties that did not qualify as micropolitan.

Table 1 also shows how urban areas remained under prolonged, harsher lockdowns during May 2020. Are these sufficient to explain the differences with rural areas? We argue this is not the case. Figure 2a illustrates the share of each county's working-age population which commutes to other counties, while Figure 2b maps the share of visitors to the retail and leisure sectors who reside in the same county as the visited shop. Using the direct commuting and consumption connections between counties, we build a set of indicators capturing how much the economy of each county was exposed to neighbours' lockdowns[2]. Table 1 shows that while Large Metro were more subject to local closures (defined as business restrictions implemented in the county itself), between 3.7% (Non-Core) and 11.1% (Large Fringe Metro) of the employed residents of a county were likely to work in a different county where business restrictions directly affecting their jobs were in place. Similarly, we find that - assuming that the number of visitors to the retail and accommodation sectors can be used as a proxy of consumption - 7.1% of the economy of

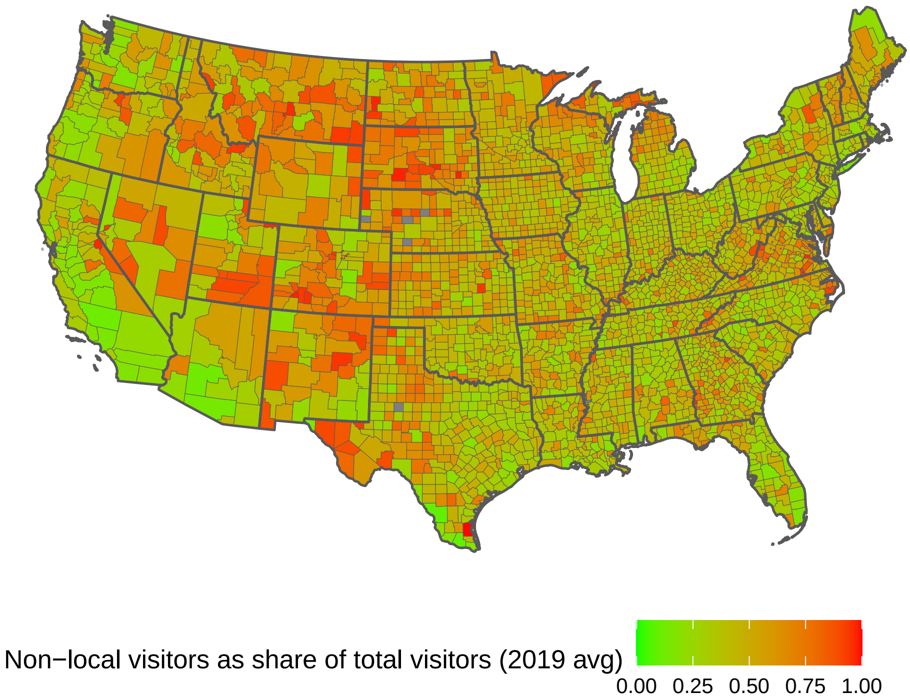
[2]We define this weighted lockdown intensity spillover in Section 4.1.2.

Figure 2: Mobility flows in the United States

(a) Work Commuting Intensity (outflows)



(b) Consumption Commuting Intensity (inflows)



Note: share of visits to Points Of Interest of local county in the leisure sector incoming from other counties, out of total visits. Data courtesy of SafeGraph Inc.

Non-Core counties was exposed to consumers from neighbouring counties where business restrictions were in place, and thus where consumers could be less likely to travel.

In Figure 1b we plot the residual of counties' employment index (with respect to a regression on local lockdown intensity, time FE and county by month FE) against our measure of exposure to neighbours' lockdowns. This confirms the negative relationship observed in the figures.

We will show that these facts are not only due to local sectors' closures or pre-determined factors such as the exposure to pandemic-sensitive sectors. Instead, we provide robust evidence for a causal relationship between local labour market outcomes and commuting (consumption) flows toward (from) areas in lockdown. In the next section, we lay out the framework within which we model the relationship between labour market outcomes and lockdown policies.

3 Estimation Framework

We now describe the framework within which we estimate the main and spillover effects of localised lockdowns.

Consider a set of locations $c \in C = \{1, 2, \ldots, N\}$. Each of these locations has "neighbours", locations with which it has some kind of economic relationship. Define E^e_{ct} the number of jobs present in c at time t (so that the overscript e stands for *"establishment-based"* employment). These do not need to be local residents, as workers can commute: workers residing in c may work elsewhere, as well as worker employed in c may be commuting from other locations. Define E_{cit} as the time-t number of workers living in c and working in i (commuters). Then, the employment of local residents of c (*"residence-based"* employment) is:

$$E_{ct} \equiv \sum_{i \in C} E_{cit} \tag{1}$$

Conversely, the total establishment-based employment of a location corresponds to the sum of all commuting flows toward that county

$$E^e_{ct} \equiv \sum_{i \in C} E_{ict} \tag{2}$$

One can think about the individual flow E_{cit} as a function of the current local economic conditions and pre-determined (at business cycle frequencies) characteristics such as geography, infrastructure or the local pool of skills, capital and labour supply. In other words - given all predetermined factors - E_{cit} will depend on the demand for goods and services experienced by counties i and c, and the consequent demand for labour. That is: $E_{cit} = E_{cit}(D_{it}, D_{ct}, \cdot)$ with D_{xt} being the demand for labour in county x at time t.

To understand how E^e_{ct} responded to the pandemic and the related policies, it is paramount to highlight why D_c and D_i can be affected by lockdowns. In the very first place, lockdowns directly affect the demand for labour in the location they are enacted, as business activity is constrained. However, business restrictions may have further second order effects, including externalities across space. This is because of several reasons. First, a number of sectors provide services that require the physical presence of the consumer in the shops' premises. If consumers cannot (i.e. due to a stay-at-home order) or do not need to (i.e. commuters who start working from home) reach a shop in a different county, labour demand may fall in the affected shops. Second, workers who are fired in location i but reside in c may adjust their consumption downward. If part of this consumption used to happen in shops located in c (Davis et al., 2019), then these will be affected by the redundancies generated in a different location. Third, as explored in the previous literature, supply chain disruptions may cause spillovers across distant locations. Fourth, higher-order network spillovers and general equilibrium effects may occur. In this sense, D_i, D_c, and thus each individual commuting flow E_{cit} (and E^e_{ict} as well), may be contemporaneously affected by policies implemented in *all* other counties. This makes a full-fledged analysis infeasible. To provide a tractable framework and pin down the most relevant facts and effects of lockdowns, we need to discipline our analysis and focus on the quantitatively and qualitatively relevant channels.

We focus on three main channels of direct and spillover effects:

1. The direct effects of county c's policies affecting local businesses,
2. The spillover effects originating from county i's policies and affecting county c through changes in the patterns of consumption mobility from i to c,
3. The spillover effects originating from county i's policies and affecting county c through the labour market outcomes of the workers who commute from c to i.

This is equal to assuming that all higher-order network effects or other sources of spillovers are either negligible (so that j cannot affect c if it is not its direct neighbour, despite having a common neighbour i), or perfectly correlated to the channels we consider. In Appendix A.5 we provide a robustness check to allow for independent effects of disruptions due to either the upstream or downstream supply chain for physical goods.

In the following analysis, we provide a framework to estimate each of these three channels. Then, we will estimate it by OLS and address potential endogeneity concerns through an IV approach. Finally, we will provide high-frequency, granular evidence for the mechanism behind aggregate estimates of the third channel.

Decomposition of lockdown effects

Given the channels/mechanisms we focus on, we show how we empirically estimate the effects of lockdowns and why their estimate is likely to be biased if it does not account for spillovers. Define a set of lockdown-independent determinants X_t, a vector of lockdown intensities across all locations $L_t = \{L_1, L_2, \ldots, L_N\}$ and lockdown-dependent determinants $M(L_t)$ (possibly a vector) and assume we can write $E_{ict} = E_{ic}(L_t, X_{ic_t}, M(L_t))$. Taking a first-order Taylor approximation around pre-pandemic employment E_{c0}, we can decompose total employment in county c at time t into a lockdown-related and a lockdown-independent term:

$$E_{ct}^{e} \approx E_{c0}^{e} + \sum_{i' \in C} \sum_{i \in C} \left(\frac{\partial E_{ic}}{\partial L_{i'}} + \left(\nabla_{M(L)} E_{ci}\right)' \left(\nabla_{L_{i'}} M(L)\right) \right) \Delta L_{i't} + \sum_{i \in C} \frac{\partial E_{ci}}{\partial X_{ci}} \Delta X_{cit} \tag{3}$$

Given the channels we focus on, we assume in first instance that $M(L)$ are consumption relationships, so that $M(L) = \{M_{1c}, \ldots, M_{cc}, \ldots, M_{Nc}\}$, where M_{ic} represents the consumption in location c happening due to visitors from county i. Under the main assumption of limiting our attention to first-order effects across all c and i for which $E_{ic} > 0$ or $M_{ic} > 0$ and assuming that the semi-elasticities of E and M with respect to L are equal across all (c, i) (for a complete treatment and interpretation of the assumptions required and the derivation of all equations see Appendix A.1), we can write this first-order approximation as a parsimonious specification of linear terms. Call the last term of Equation 3 $G(X_{ct})$. Then,

$$\frac{E_{ct}^{e}}{E_{c0}^{e}} \approx 1 + \frac{\partial E_{c}^{e}}{E_{c0}^{e} \partial L_{c}} \Delta L_{c} + \frac{\partial E_{c}^{e}}{E_{c0}^{e} \partial E} \frac{\partial E}{E_{c0} \partial L} \left(\sum_{i \in C} E_{ci} \Delta L_{it} \right) + \frac{\partial E_{c}^{e}}{E_{c0}^{e} \partial M} \frac{\partial M}{M \partial L} \left(\sum_{i \neq c} M_{ic} \Delta L_{it} \right) + G(X_{ct}) \tag{4}$$

Notice how this is a linear equation in a constant intercept, a control term depending on county-specific characteristics X and three lockdown-related terms. The first term $\frac{\partial E_{c}^{e}}{\partial L_{c}} \Delta L_{c}$ captures the direct impact of local restrictions on local employment. The second $\frac{\partial E_{c}^{e}}{E_{c0}^{e} \partial E} \frac{\partial E}{E_{c0} \partial L}$ captures the effect on local employment of restrictions imposed where local residents work (which may affect local employment through consumption). The third $\frac{\partial E_{c}^{e}}{E_{c0}^{e} \partial M} \frac{\partial M}{M \partial L}$ captures how much local consumption is affected by restrictions in other counties, on top of the effect running through local residents working elsewhere. The weights applied to these partial derivatives are intuitive. The first term requires a measure of the strength of county c's lockdown L_{ct}. The second is a weighted average of neighbours' lockdowns, using commuting flows as weights. The third is a weighted average of neighbours' lockdowns, using consumption flows as weights.

While Equation 4 is sufficient to illustrate our framework, in Appendix A.1 we allow for heterogeneity in economic sectors, and show under what assumptions we can still retain a similarly parsimonious specifications.

4 Data

The decomposition in the previous section shows how, in order to estimate the effects of lockdowns on the labour market, we need data on i) local labour market dynamics, ii) pre-pandemic commuting and consumption relationships, and iii) the size of the treatment across different areas and sectors.

We build a monthly and a weekly dataset with all the aforementioned elements. Both cover 3108 counties from mainland U.S. (except Alaska). The monthly dataset covers the period from January 2017 to June 2020, while the weekly one covers January 2019 to June 2020. The unit of observation of the monthly dataset is the county, whereas the unit of observation of the weekly dataset is the individual business (e.g. shop, branch or venue). In the weekly dataset, these are referred to as Places of Interest (POIs).

Labour Market. The monthly dataset contains data on county-level employment. Employment is establishment-based (employees are accounted for in the county where they work), from BLS's Quarterly Census of Employment and Wages (QCEW).

Mobility interconnections. To account for spatial interconnections, we use the American Community Survey's 2011-2015 cross-county commuting flows and SafeGraph Point-Of-Interests' (POIs) mobility data for consumers' mobility flows. The county-level commuting flows are from the 2011-2015 ACS commuting files. These represent physical commuting flows: the employment location is the county where the workers physically carry out their job. Mobility data gives us the number of visitors by Census Block Group of origin and shop of destination, but we aggregate them at the county level in the monthly dataset. Origin is always aggregated at the county level.

Covid and Demographic Data. Data on Covid-19 deaths and cases were obtained from the New York Times GitHub repository (Mitch Smith et al., 2020). Population data is from Census Current Population estimates.

Business restrictions and travel bans. We hand collect a dataset of county-level orders, differentiating between stay-at-home orders, retail closures, restaurant closures and bar closures. While we cross-check our data with Goolsbee et al. (2020), we consider the possibility of multiple spells of orders, consider dates until June 2020 (included) and make further differentiations about the type of business closures. We consider a county "under lockdown" of each type when there is either 1) a state-wide order, 2) a state order for specific counties, 3) a local lockdown order covering the whole county, or 4) city-wide lockdown orders covering more than half of the population of the county. Whenever a state allows a subset of counties to reopen "early", we classify that date as the end of the state-level order[3].

We distinguish between the following types of *"lockdown"*, based on the affected sectors:

1. *Stay-At-Home Order (SAH),* if the order involves the closure of part of the manufacturing sector and the total restriction of mobility beyond essential activities

2. *Retail*, if all non-essential retail businesses are closed but for delivery or curbside pickup or are allowed up to 25% of standard capacity

3. *Leisure*, if all restaurants are closed but for delivery or curbside pickup or are allowed up to 25% of standard capacity

4. *Bars*, if it involves at least the closure of bars or a reduction to 25% or less of the standard capacity. Notice how this definition de facto nests all other types, as bars are always the first to close. As a consequence, this is equivalent to "any restriction" being in place.

Finally, we consider interstate travel bans as an additional non-pharmaceutical interven-

[3]Some states (such as Maine) declared the end of their "state" order only when all counties were open, while others declared the "state-wide" order ended when they started introducing state-driven local restrictions; we adopt the second definition for all states.

tion (NPI) useful to explain the changes in consumers mobility, as it prevents non-essential travel. Using the report of travel bans from Ballotpedia and the cited sources,[4] we build a bilateral matrix of state-to-state and state-to-county mandatory travel bans, accounting for whether the travel ban regards banning incoming or outgoing travel. Using this, we build a panel of county-to-county daily travel bans status.

POI dataset and consumption flows. The weekly POI-level dataset uses the same business restriction data as the monthly one and includes location data from SafeGraph,[5] a data company that aggregates anonymised location data from numerous applications in order to provide insights about physical places, via the Placekey[6] Community. To enhance privacy, SafeGraph excludes census block group information if fewer than five devices visited an establishment in a month from a given census block group. The panel includes data from 45 million mobile phone devices in the US (or approximately 14% of the population, or 17% of active mobile phones in 2019). For each business, we observe a number of metadata (i.e. detailed sector and location), along with weekly visits, visitor origin Census Block Group (CBG), and visits by length of stay at the premises. The data undergo a complex censoring and anonymisation mechanism. In the Appendix, we describe and discuss how to manage it and how it can affect our results.

For more information on data collection and cleaning, see Appendix A.2.

4.1 Lockdown intensity index

In the rest of this section, we describe our measures of own lockdown and spillover exposure. The goal is to construct proxies for the variational terms of the decomposition described in Section 3, in order to estimate the derivative terms by OLS or IV. First, we provide indicators matching the decomposition in Equation 4 to introduce the estimation framework intuitively. Then, we allow for differences in county-level sector weights and types of lockdowns currently in force (in Appendix A.1 we describe how they match

[4] https://ballotpedia.org/Travel_restrictions_issued_by_states_in_response_to_the_coronavirus_(COVID-19)_pandemic,_2020-2021

[5] https://www.safegraph.com/

[6] https://www.placekey.io/

a decomposition similar to Equation 4, but derived under milder assumptions). We use the baseline indicators when working with POI-level data (as sector aggregates are oversets of individual POIs), and the sector-weighted ones when working with county-level employment data.

4.1.1 Baseline lockdown intensity index

To define notation, we consider a set of counties indexed by $c \in C$. Define flow_{cr} the number of workers living in county $c \in C$ which commute to county $r \in C$.

We say that a county is *"under lockdown of type Z on a given day"*, where Z is one of the lockdown types previously described, if there is a Z-type lockdown order put in place by some authority (state, county or others) affecting county c. For a given month t, we define the lockdown status of county c as

$$L^Z_{tc} = \frac{\text{Days in lockdown of type } Z}{\text{Days in month t}}$$

Consistently with the decomposition from Section 3, we define the intensity of own lockdown as

$$\kappa^{\text{own}}_{Ztc} = L^Z_{tc} \tag{5}$$

This captures how much local businesses are likely to be affected by local measures. We measure how much a county is exposed to neighbours' policies by weighting other counties' lockdowns by the commuting flows *toward* ($\kappa^{\text{outflow}}_{Ztc}$) or *from* ($\kappa^{\text{inflow}}_{Ztc}$) those counties:

$$\begin{aligned} \kappa^{\text{outflow}}_{Ztc} &= \frac{\sum\limits_{r \in C: r \neq c} L^Z_{tr} * \text{flow}_{cr}}{\text{F}_{tc}} \\ \kappa^{\text{inflow}}_{Ztc} &= \frac{\sum\limits_{r \in C: r \neq c} L^Z_{tr} * \text{flow}_{rc}}{\text{F}_{tc}} \end{aligned} \tag{6}$$

Where F_{tc} is a normalising factor, which we choose to be the population for each county

c at time t.

4.1.2 Sector-weighted lockdown intensity index

The definition of treatment intensity in the previous section relies on assuming that equal lockdown orders have the equal direct and spillover effects across all counties, regardless of local employment composition. However, an area with large manufacturing employment is unlikely to be affected by restrictions to the accommodation sector as much as a tourism-focused county.

We can address this issue while maintaining a parsimonious specification through an alternative assumption. We assume all lockdowns affect local economies in a linear proportion to the employment share of the affected sectors (for a detailed explanation of the assumptions needed to justify this step in Equation 4, see Assumption 5 in the Appendix). Under this assumption, we can limit our attention to a pair of sector-weighted lockdown indices. The first to capture direct effects. The second to capture spillover effects. We build the sector-weighted lockdown index for spillovers by weighting each order type $Z \in \{\text{bar, leisure, retail, sah}\}$ by the share of the economy in county r it affects (μ_{rZ})[7]:

$$\hat{\kappa}_{tc}^{own} = \sum_{Z} L_{tr}^{Z} \times \mu_{rZ} \tag{7}$$

$$\hat{\kappa}_{tc}^{outflow} = \frac{\sum\limits_{r \in C: r \neq c} \left(\sum\limits_{Z \in \overline{Z}} L_{tr}^{Z} \times \mu_{rZ} \right) \times \text{flow}_{cr}}{(\text{Population})_{tc}} \tag{8}$$

where the term $\sum\limits_{Z} \left(L_{tr}^{Z} \times \mu_{rZ} \right)$ is the share of the economy in r under business restrictions. To calculate μ_{rs} we use sector-level employment data from the 2019 QCEW[8].

[7] We assume that the industry composition of a commuting flow is the same as that of establishment-based employment in the destination county due to data availability.

[8] Due to data availability, we proxy the weight of non-essential manufacturing closures by the county-level weight of the manufacturing and construction sectors, times the state-level share of non-essential employment in the corresponding sectors.

4.1.3 Consumption flows and lockdown intensity

Finally, we build a set of treatment intensities based on consumption relationships with one's neighbours. We follow the same procedures as those described in the previous section, but we use *visits inflows* as weights for neighbours' lockdowns. We measure the consumption spillover intensity as:

$$\kappa_{tc}^{\text{consumption inflow}} = \frac{\sum_{r \in C: r \neq c} L_{tr}^{l} * \text{visits}_{rc}}{(\text{total visits})_c} * (\mu_{\text{non-essential retail}} + \mu_{\text{leisure}}) \tag{9}$$

where visits_{rc} are visits from neighbouring counties to retail and leisure POIs, L_{tr}^{l} is the share of the month spent under a *leisure lockdown*, $(\text{total visits})_c$ are total visits (to retail and leisure POIs), and μ_x is the share of employment in county c in sector x. This rescaling of the lockdown intensity by the share of the economy to which visits are referred to is meant to capture the extent to which visits from neighbours are important for the local economy. For example, a 10% drop in visits to the leisure sector is likely to have a larger effect on overall employment in Las Vegas compared to a major manufacturing centre such as Elkhart County, Indiana.

4.2 Descriptive statistics

Table 2 presents summary statistics on labour and consumption mobility within the U.S. economy. Panel A shows that on average 28% of employed residents commuted across county borders over 2011-2015. The proportion of out-of-county consumers is also substantial, as around a third of all consumption visits to leisure and retail establishments (NAICS 71 and 72) originate from outside the county. Finally, leisure and retail establishments represent around 12% of pre-pandemic employment at the Labour Market Area (LMA) level. Panel B shows descriptive statistics about the treatment (lockdown orders) between March and July 2020. On average, 10% of counties' employment was exposed to some form of lockdown order through commuting patterns, compared to 19% affected directly by local business restrictions. Spillover intensities are generally smaller than own lockdown ones as the proportion of cross-county commuters to local workers is approximately 1 to 3 in the United States, as can be seen in Panel A. In the case where counties enacted their own order, the mean duration was slightly less than three months.

Table 2: Summary statistics for the monthly dataset

Panel A: Commuting

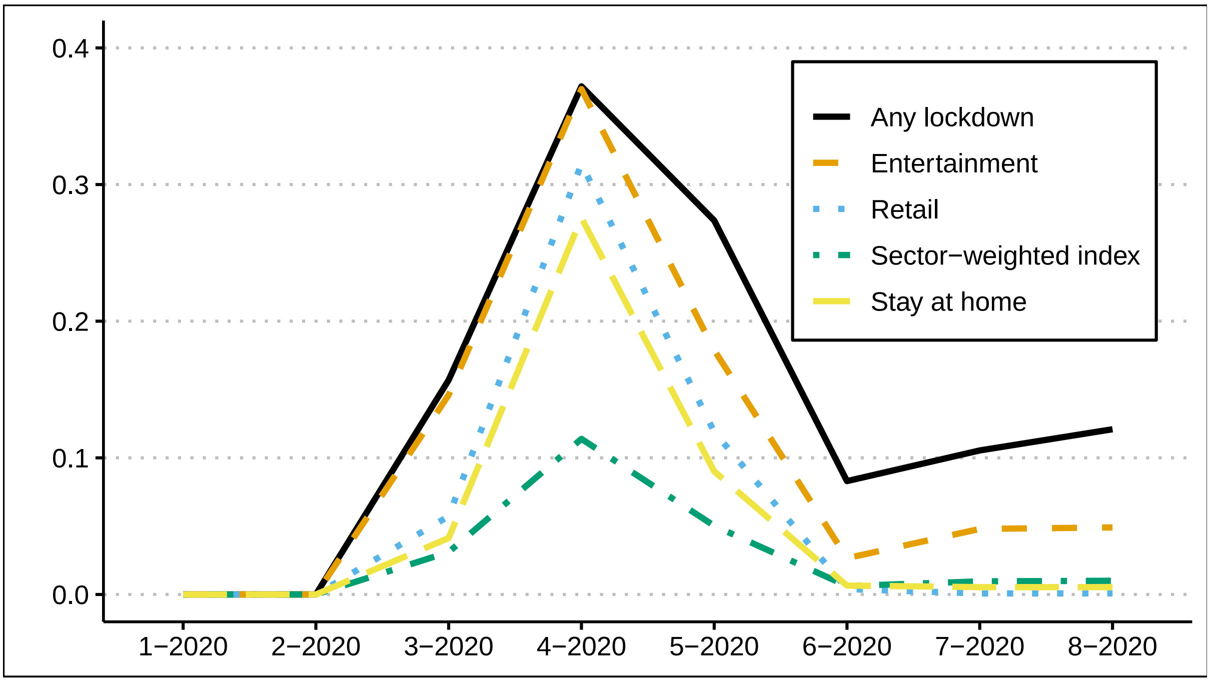
	N	Mean	Std.Dev	Min	Max
Commuting workers share of working residents, 2011 - 2015	3108	0.28	0.18	0.02	0.92
Commuting (inbound) workers share of total workers, 2010 - 2015	3108	0.26	0.13	0.00	0.92
Consumption inflow of total consumption (NAICS 71,72)	3108	0.33	0.12	0.09	1.00
Exposure to Leisure Sector (LMA level)	3108	0.12	0.03	0.00	0.57

Panel B: Treatment (Commuting)

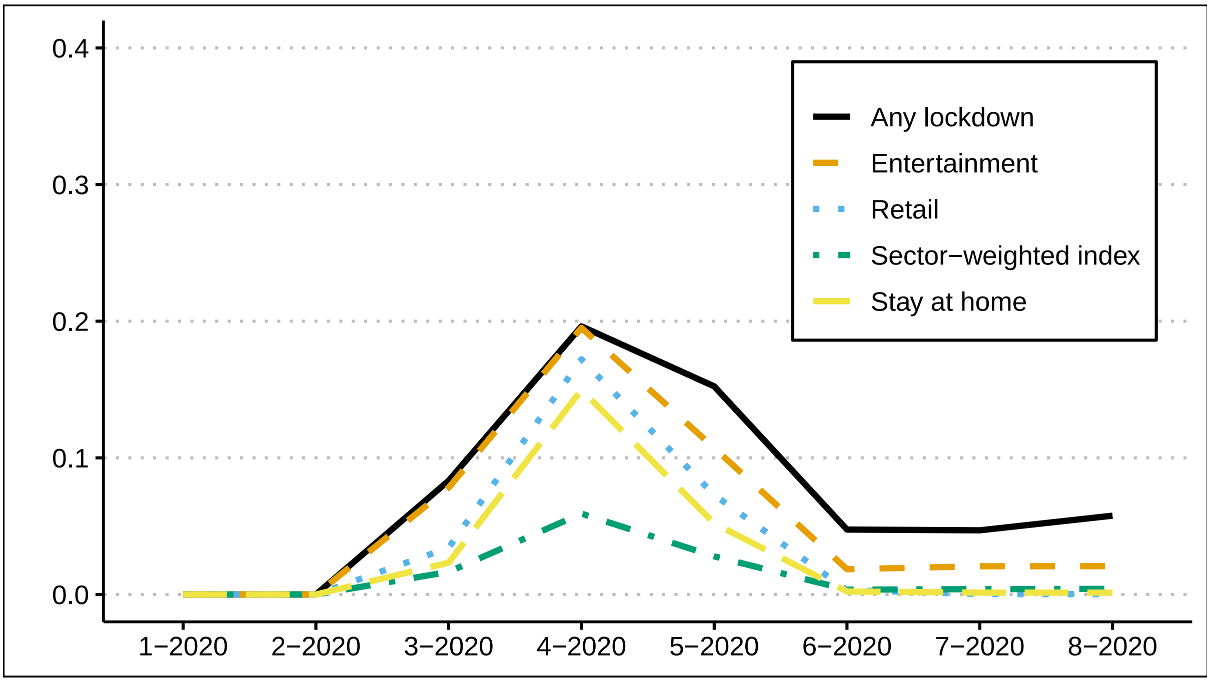
	N	Mean	Std.Dev	Min	Max
Average monthly exposure to neighbours orders (March-July 2020)	3108	0.10	0.06	0	0.37
Average monthly exposure to neighbours retail orders (March-July 2020)	3108	0.05	0.04	0	0.26
Average monthly exposure to neighbours SAH orders (March-July 2020)	3108	0.04	0.03	0	0.26
Average monthly exposure to own orders (March-July 2020)	3108	0.19	0.11	0	0.61
Average monthly exposure to own retail orders (March-July 2020)	3108	0.08	0.05	0	0.47
Average monthly exposure to own SAH orders (March-July 2020)	3108	0.07	0.06	0	0.47
Mean months under any order (March-July 2020)	3108	2.96	1.35	0	5.52
Mean months under retail order (March-July 2020)	3108	1.34	0.68	0	5.29
Mean months under SAH order (March-July 2020)	3108	1.13	0.84	0	5.29

Figure 3 shows the own (k_{Ztc}^{own}) and spillover (k_{Ztc}^{outflow}) average lockdown intensity by type, along with the sector-weighted index. We can observe a clear precedence of lockdown orders, with leisure closures being in place in more counties than retail closures, which in turn are enacted more often than stay-at-home orders. Figure 6 in the Appendix presents the share of counties under lockdown by month and type of order.

Figure 3: Own and spillover lockdown intensity over time, by type of order



(a) Average own lockdown intensity (κ^{own})



(b) Average lockdown spillover spillover (κ^{outflow})

5 Empirical Analysis

5.1 Methodology

Our goal is to provide an estimation of the labour market effects of lockdown policies. To do so, we implement the decomposition of Section 3, and extended in Appendix A.1, through OLS, using the exposure indicators we presented. We estimate the reduced form model

$$y_{tc} = \beta_0 + \beta_1 X_{tc} + I_c + I_t + \Theta_{tc} + \varepsilon_{tc} \tag{10}$$

where: y_{tc} is the county-level index of employment; X_{tc} is a vector of county-level controls not related to lockdowns; I_c are county by calendar month and I_t are time fixed-effects. Θ_{tc}, which we specify below, is a composite term measuring the effects of own and neighbours' lockdowns. The estimation of Θ_{tc}, and in particular of its spillover component, is the main goal of our analysis. In our baseline specification, we define Θ_{tc} as the sum of own and commuting-related spillover effects of lockdowns

$$\Theta_{\text{outflow}} = \gamma_1\, \kappa_{tc}^{\text{own}} + \gamma_2\, \kappa_{tc}^{\text{outflow}}$$

Where γ_1 measures the effect of own lockdown and γ_2 measures the spillover effects due to neighbouring authorities' lockdown status, weighted by outbound commuting (outflows) and share of neighbours' economies affected by the closures. Later, we add a further spillover term arising from consumption flows, $\kappa_{tc}^{\text{consumption inflow}}$, and its interaction with current business closures.

In the following sections, we present our baseline OLS estimations and discuss potential endogeneity concerns. We then turn to an IV approach to provide causal inference of the effects of lockdown orders and their commuting-related externalities.

Variable transformations and controls. We control all specifications by a third-degree polynomial of days since the first Covid-19 case was reported in the county. When indicated, we control by the county's LMA exposure to the leisure macrosector (NAICS 71 and 72), which may have been heavily affected regardless of the local lockdown status due to shifts in consumers' preferences or perceived contagion risk. Employment is indexed to its 2019 average, so that 1 = 2019 average employment. Lockdown spillovers intensities are normalised by population. Baseline estimates are run on 3108 counties. We drop all observations for which we do not have data on LMA's leisure macrosector employment when this variable is used as a control. Finally, we drop all counties for which we do not observe detailed enough data on employment composition by sector (see A.2 for more details). All errors are clustered by county. Unless otherwise specified, county-level regressions are weighted by total working age population, and all regressions include county by calendar month fixed effects and time fixed effects.

5.2 OLS

Column (1) in Panel A of Table 3 reports a baseline for the estimated direct effect on a county's employment index of one full month under complete (all relevant sectors) lockdown. We estimate an employment loss of 25.3 percentage points for a whole month.

In column (2) we introduce our measure of sector-weighted commuting spillovers. The direct effect of lockdowns is slightly smaller than the previous estimate (-22.9 pp.). Spillovers add further effects, so that when all local county residents commute to other counties, and these close their whole economy for a whole month, local county employment falls by a further 28.5 pp. (p-value 0.025). To interpret these results, consider that no more than 34% of U.S. workers were ever contemporaneously subject to closures and that only 48% of the U.S. population was employed before the pandemic. The average share of commuters among employed residents is 28%. Fitting the coefficients to the U.S. average, our estimates suggest that direct effects explain a 9.2% reduction in the U.S. aggregate employment, while spillovers explain an additional 1.3%. Column (3) considers the possibility of lagged effects on employment. While there seems to be a delayed effect of spillovers, it is not statistically significant, and the bulk of the influence of spillovers appears to be contemporaneous.

Table 3: NAICS-Weighted lockdown impact indicator

Panel A: Results

	Establishment-based Employment index (1 = 2019 employment)				
	(1) No Spillovers	(2) Baseline	(3) Lags	(4) IV (rescaled)	(5) IV (non-rescaled)
Own lockdown intensity	-0.253***	-0.229***	-0.178***	-0.213***	-0.129***
	(0.0189)	(0.0214)	(0.0185)	(0.0219)	(0.0319)
Commuting spillover		-0.285*	-0.274**	-0.473**	-1.476***
		(0.127)	(0.102)	(0.146)	(0.285)
L.Own lockdown intensity			-0.138***		
			(0.0193)		
L.Commuting spillover			-0.0651		
			(0.0827)		
Constant	0.983***	0.984***	0.985***		
	(0.000881)	(0.00105)	(0.00114)		
Observations	123606	123606	120663	123606	123606
R^2	0.815	0.816	0.824	0.078	0.027
Kleinberg-Paap F-stat				2163.11	139.61
CountyxMonth and Time FEs	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes
Industry Exposure	Yes	Yes	Yes	Yes	Yes

Panel B: First Stage

	IV (neighbours)	IV (neighbours 2)	IV (lags)	
	(1) Commuting spillover	(2) Commuting spillover	(3) Commuting spillover	(4) L.Commuting spillover
Lockdown Spillover IV (rescaled)	0.883***		0.804***	0.0345***
	(0.0190)		(0.0163)	(0.00638)
Own lockdown intensity	0.0578***	0.0830***	0.0571***	0.00205
	(0.00422)	(0.00566)	(0.00368)	(0.00209)
Lockdown Spillover IV (not rescaled)		0.892***		
		(0.0755)		
L.Lockdown Spillover IV (rescaled)			0.130***	0.850***
			(0.0174)	(0.0189)
L.Own lockdown intensity			-0.00263	0.0567***
			(0.00253)	(0.00395)
Observations	123606	123606	120663	120663

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the establishment-based index (with 2019 average = 1) on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. In columns 4 and 5 of Panel A we instrument spillover intensity with a NAICS-weighted variation of the instruments outlined in section 5.3. These instruments exploit variation in neighbouring states' lockdown orders, which we argue are exogenous to unobservables that may also determine the own county outcomes. The lockdown intensity measure is proportional to the time spent under the corresponding lockdown order. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). The own lockdown intensity is proportional to the share of month under lockdown, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours over population. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. All specifications include a measure of the local market area's industrial exposure to the effects of Covid-19. All regressions are weighted by the county-level total working population.

In Table 4 we add a term capturing the spillover effect of lockdowns through consumption inflows, i.e., the degree to which the local economy is exposed to visits to the leisure and retail sectors from consumers residing in other counties. The way this measure is built is outlined in section 4.1.3. If neighbours' policies prevent or discourage consumers' mobility, counties highly exposed to the inflow of consumers will likely suffer additional losses in revenues, and thus jobs. Additionally, we control for the interaction of this term with local closures of the same sectors to account for the fact that some consumption inflows may be affected due to *either* local closures, *or* neighbours' policies.

Table 4: NAICS-Weighted lockdown impact indicator - consumption

	Establishment-based Employment index (1 = 2019 employment)				
	(1) No Spillovers	(2) Baseline	(3) Lags	(4) IV (rescaled)	(5) IV (non-rescaled)
Own lockdown intensity	-0.253***	-0.226***	-0.163***	-0.222***	-0.151***
	(0.0189)	(0.0238)	(0.0194)	(0.0244)	(0.0339)
Commuting spillover		-0.230	-0.284**	-0.433**	-1.526***
		(0.139)	(0.102)	(0.152)	(0.316)
Consumption Spillover		-0.358***	-0.0231	-0.393***	-0.315*
		(0.0846)	(0.109)	(0.115)	(0.129)
Consumption Spillover × share month in ent. order		0.246*	-0.0569	0.332*	0.369*
		(0.114)	(0.139)	(0.131)	(0.150)
L.Own lockdown intensity			-0.115***		
			(0.0203)		
L.Commuting spillover			0.0193		
			(0.0855)		
L.Consumption Spillover			0.0966		
			(0.111)		
L.Consumption Spillover × L.share month in ent. order			-0.218		
			(0.116)		
Constant	0.983***	0.984***	0.985***		
	(0.000881)	(0.00105)	(0.00105)		
Observations	123606	123606	120663	123606	123606
R^2	0.815	0.817	0.824	0.081	0.024
Kleinberg-Paap F-stat				869.97	41.2
CountyxMonth and Time FEs	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes
Industry Exposure	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the establishment-based index (with 2019 average = 1) on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. In columns 4, 5 and 6 we instrument spillover intensity with the instruments outlined in section 5.3. The instruments exploit variation in neighbouring states' lockdown orders, which we argue are exogenous to unobservables that may also determine the own county outcomes. The lockdown intensity measure is proportional to the time spent under the corresponding lockdown order. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). The own lockdown intensity is proportional to the share of month under lockdown, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours over population. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. All specifications include a measure of the local market area's industrial exposure to the effects of Covid-19.

Column (1) presents a benchmark specification identical to column (1) in the previous table. Column (2) considers both commuting and consumption spillovers. We find that commuting spillovers are large but not statistically significant when consumption-related spillovers are added, with the estimated effect of the latter being both significant and large. In a county where *all* jobs are either in the leisure or retail sectors, and *all* visitors to those sectors are from outside the county, local employment would suffer a statistically significant reduction of 35.8 percentage points. At the same time, we find that this employment loss is partially offset when the local leisure sector is mandated to close. That is, a large part of those job losses are due to *either* neighbours' or local closures. For example, think about a restaurant that lays off most of its waiters as clients from neighbouring counties cannot visit the premises due to curfews. The same jobs would also be lost if the restaurant was ordered to cease all dine-in activity. The interaction term allows to capture this phenomenon. The lagged specification in column (3) suggests that the effect of consumption spillovers may have a delayed onset, however the estimate is too imprecise to allow a firm conclusion.

5.3 IV strategy

Lockdown policies are chosen by local state, county or city authorities, which may directly or indirectly include the state of the economy among key indicators to determine their policies. If local or neighbours' economic activity affects the decision process when imposing local lockdowns - or when some counties have decisional weight in determining the state lockdowns - we can run into an endogeneity problem. This section formalises our concerns and proposes a solution to provide causal evidence for spillover effects.

Lockdown implementation process

First, we formalise how to think about the decision process that leads a county and its neighbours to be in lockdown. We say that sector Z in county c is under lockdown at time t (L^Z_{tc} according to the previously introduced notation) if there is at least one order of a relevant administrative authority that affects that county and sector. These orders can be at the state, county or city level or county/city-specific orders imposed by the state. Call these orders $O^Z_{ta(c)}$, with $a(c)$ being the local authority imposing the order on county c. Then, c's lockdown status is $L^Z_{tc} = \max\{O^Z_{ta_1(c)}, O^Z_{ta_2(c)}, \ldots\}$. In principle, local lockdowns can affect - both directly and through unobservables - that determine local and neighbours' economic outcomes y_{tc} and y_{tr}. In turn, this can affect the likelihood of imposing lockdown orders, creating an endogeneity problem: county c's spillover indicators $\kappa^{\text{outflow}}_{tc}$ may be endogenous to c's outcomes y_{tc} if a common $a(r) = a(c)$ or independent $a(r) \neq a(c)$ decision-maker with authority over r takes y_{tc} into account when determining $O^Z_{ta(r)}$. That is, we are concerned that neighbours' orders can be a function of y_{tc}: $O_{ta(r)} = O_{ta(r)}(y_{tc}, \cdot)$. Formally,

$$O_{ta(r)} = O_{ta(r)}(y_{tc}, \cdot) \implies \kappa^{\text{outflow}}_{tc}(L, \text{flows}) = \kappa^{\text{outflow}}_{tc}(L(y_{ct}, \cdot), \text{flows}) \tag{11}$$

which implies that estimates of the effect of κ^{outflow} will be biased due to reverse causality between own county outcomes and neighbours' lockdown status.

For example, consider a state-wide order affecting all counties within that state. Suppose the state implements a reopening based on health and economic indicators correlated to the local county's outcomes y_{tc} (due to size or preferences which make c pivotal in the lockdown decision process). In that case, the error term $\varepsilon = y - \beta X$ will be correlated with the regressors κ^{outflow} if innovations to y_{tc} cause changes in the neighbouring counties' orders too (i.e. if a state reopens a "block" of neighbouring counties at once, based on outcomes from c). Thus κ^{outflow} is now possibly endogenous to y_{tc}, biasing OLS estimates.

To address this issue, we propose an instrument for κ^{outflow} based on neighbouring states' orders. These are unlikely to be endogenous to the outcome y_{tc} of an individual county located in a different state.[9] Hence, we claim that this instrument satisfies the exclusion restriction. Furthermore, the proposed instrument is relevant as it is correlated with the spillover intensity measure $\kappa^{outflow}$, since $cov(L^{\mathrm{Z}}_{tr}, O^{\mathrm{Z}}_{ts(r)}) > 0$ as state-wide restrictions have hierarchical precedence over local restrictions.

[9] While some groups of states agreed on what pandemic-related factors to base the reopening on (Caspani and Resnick-Ault, 2020; Klayman, 2020), they explicitly mentioned not coordinating the reopening dates, consistently with what we observe in the data. These agreements are not in conflict with the assumption that neighbouring states' lockdown orders are - after controlling for pandemic-related indicators - exogenous to local employment outcomes.

Instrument Computation

We build instruments for county c's neighbours' lockdown intensity by i) dropping all same-state counties and ii) proxying neighbour r's lockdown indicator by its state-level order. The variation we use to identify the effects of lockdowns comes exclusively from the exposure (through commuting relationships) of county c to state-wide lockdown orders enacted by neighbouring states which affect c's out-of-state neighbours.

To see how this relates to our treatment variable, consider the following decomposition of the lockdown spillover intensity measure:

$$\kappa_{stc}^{\text{outflow}} = \underbrace{\frac{\sum\limits_{r\in C:r\neq c,s(r)\neq s(c)} L_{tr}^{\text{Z}} * \text{flow}_{cr}}{\text{Population}_{tc}}}_{\text{Contribution from out-of-state counties}} + \underbrace{\frac{\sum\limits_{r\in C:r\neq c,s(r)=s(c)} L_{tr}^{\text{Z}} * \text{flow}_{cr}}{\text{Population}_{tc}}}_{\text{Contribution from same-state counties}} \tag{12}$$

Where $s(\cdot)$ is the state of a county. The variation in the first term comes from counties in different states. Under the assumption that neighbouring states' orders are exogenous to own-county outcomes, we can use $O_{ts(r)}^{\text{Z}}$ (the state-wide order affecting r) as an exogenous proxy for their lockdown status L_{tr}^{Z}. The instrument is then calculated as:

$$\kappa_{\text{IV},Ztc}^{\text{outflow}} = \frac{\sum\limits_{r\in C:r\neq c,s(r)\neq s(c)} O_{ts(r)}^{\text{Z}} * \text{flow}_{cr}}{(\text{Population})_{tc}} \tag{13}$$

We call this instrument the *non-rescaled instrument*, which is highly dependent on how much a county is exposed to commuting toward other states than its own. However, notice that this instrument is implicitly imposing that the best prediction of the y_c-exogenous variation in L_{tr}^{Z} for all counties located in the same state as c is zero. In other words, the variation coming from the second component in Equation 12 is ignored. This does not need to be the case. An improvement can be to proxy the same-state counties' lockdown state by the conditional commuting-weighted average of the neighbouring states' lockdowns. In this way, we can define an equally valid but potentially stronger instrument. After some algebra, this is equivalent to Equation 14: the conditional flow-weighted average of neighbouring states' closures (first term), scaled by the overall share of commuting individuals in the county (second term). For proof of this, see Appendix A.3.

$$\kappa_{\text{IV},Ztc}^{\text{outflow}} = \frac{\sum\limits_{r\in C:r\neq c,s(r)\neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\sum\limits_{r\in C:s(r)\neq s(c)} \text{flow}_{cr}} \times \frac{\sum\limits_{r\in C:r\neq c} \text{flow}_{cr}}{(\text{Population})_{tc}} \tag{14}$$

This *rescaled instrument* exploits the variation of neighbouring states even for counties with weak exposure to out-of-state orders. As long as $cov(L^{\text{Z}}_{ts(r'\neq r)}, O^{\text{Z}}_{t\cancel{s}(r)}) \neq 0$, this will capture part of the y_c-exogenous variation in the treatment for same-state counties. Similar instruments can be derived for all other spillover variables in the same fashion.

Given the way we have defined both instruments, an IV specification will estimate the Local Average Treatment Effect (LATE) for counties whose spillover intensity has increased due to closures in neighbours who are in a different state.

First Stage

Panel B in Table 3 reports the results for the first stage of columns (4) and (5) of Panel A. The rescaled instrument is strong, with the first stage coefficient of lockdown intensity being close to 1. in addition, the first stage F-statistic is large (reported with the IV results in Panel A). Note that for the non-rescaled instrument, the variation is coming mostly from counties close to a state border, which may be different than the average county.

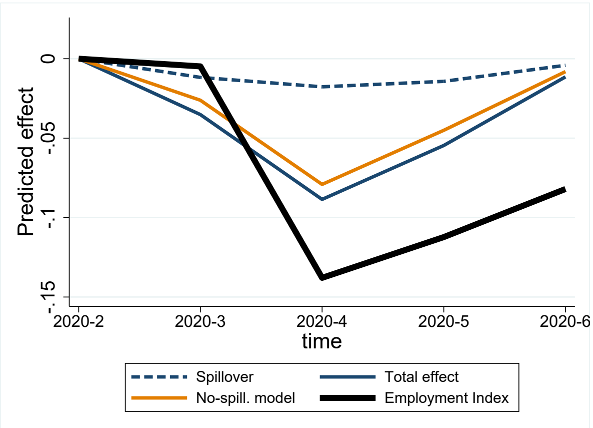
Results

The IV estimates support a causal interpretation of the OLS results. We find larger coefficients for the lockdown spillovers, but this may be due to the subset of counties on which the effects are identified. All coefficients are highly significant. In Table 4 we show what the estimates for the consumption-based spillovers are significant and similar to (in fact slightly larger than) those estimated using OLS, for both the rescaled and the non-rescaled instruments.

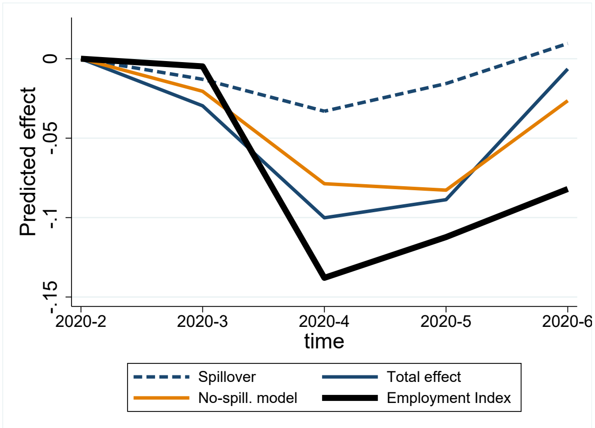
5.4 Spatial distribution of lockdown effects

Using county-level data, we find sizeable job losses due to mobility spillovers. Consequently, specifications ignoring them are likely to be biased. This is both a *quantitative* and *spatial* bias since the relative intensity of mobility spillovers varies across time as well as space.

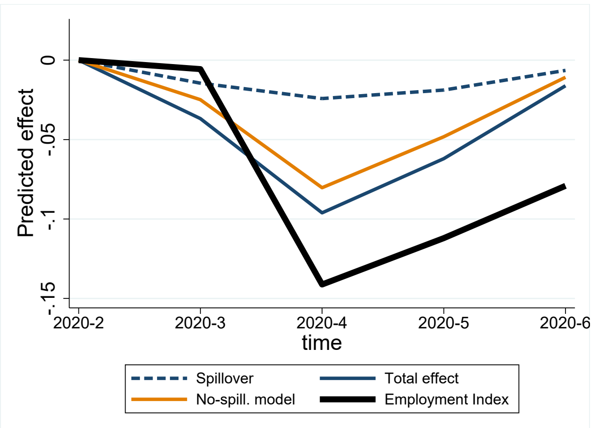
Figure 4: Effects of lockdown on establishment-based employment index



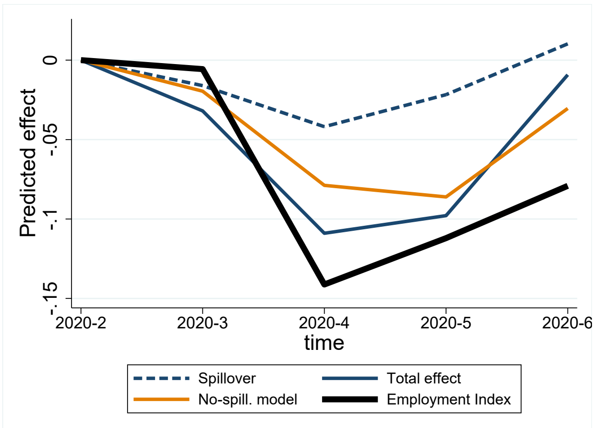
(a) No-lags, all counties



(b) Lags, all counties



(c) No-lags, High Commuting counties



(d) Lags, High Commuting counties

Note: "No lags" specification corresponds to Baseline in Table 4. "Lags" specification refers to a specification with two lags of each covariate of the "no lags" specification.

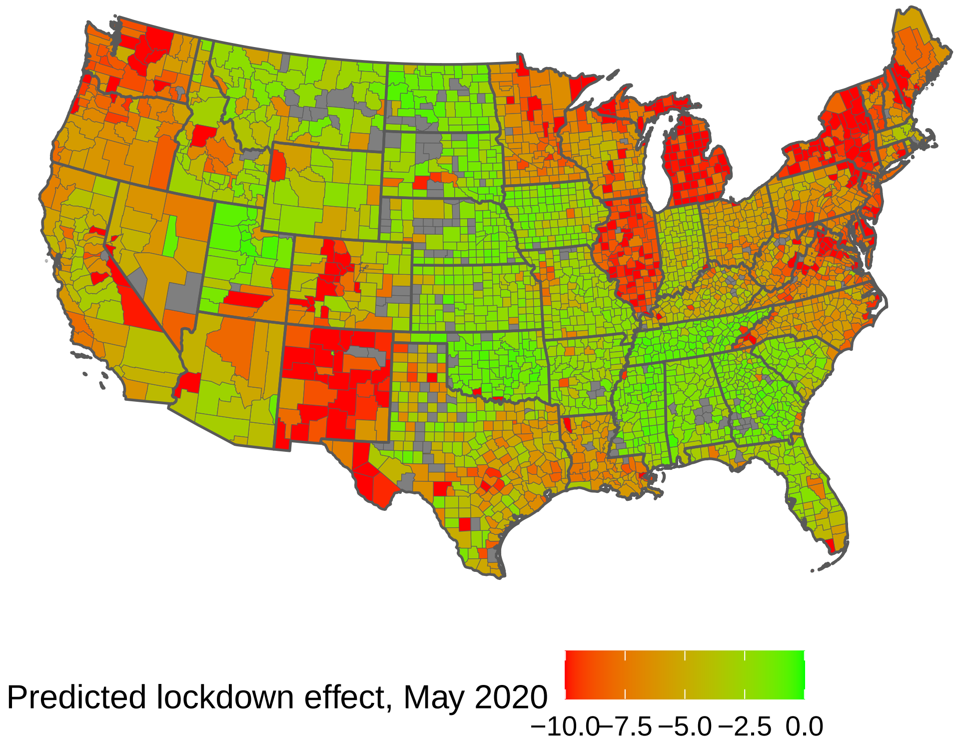
To interpret these results, we fit the coefficients to the average county treatment intensity. Figure 4 compares the average fitted effects of business restrictions on employment for the U.S. as a whole. We compare our spillover specification (with consumption-weighted terms) against the reference specification without spillovers. We find that approximately 10% of the total change in employment can be attributed to spillovers. This share increases once we account for lags. A model not accounting for mobility spillovers strongly underestimates the fall in employment observed both in suburban areas and certain rural counties highly exposed to consumption inflows (for example, due to tourism) and commuting, as shown in the panels which consider above-median commuting counties only. For these, spillovers account for a larger share of the total change in employment than what we found for the U.S. average. To show this on a map, in Figure 5a we plot the predicted effects of lockdowns in the spillover model, while in Figure 5b we plot the difference (in percentage points) in the predicted change in May 2020 employment due to lockdowns between our baseline model and the alternative without spillovers. This figure highlights the spatial bias arising from not considering lockdown spillovers, as the difference between the models' predicted effects is not uniform across space.

We also find a considerable correlation between the urban nature of the county and the intensity of mobility spillovers. In Table 5 we report the direct, spillover and total effect of lockdown on employment by urban/centrality classification. Interestingly, the specification with spillovers does not imply much larger employment losses for central metropolitan counties than the no-spillover baseline. The rationale is that the counties corresponding to big cities are less exposed to commuting (Fig. 2a) and inflow consumption (Fig. 2b) than others. Instead, suburban areas were heavily affected by spillovers (almost twice as much as Central Metro counties for Fringe metro ones), and half of the employment fall in Non-Core explained by lockdowns areas appears to be due to spillovers.

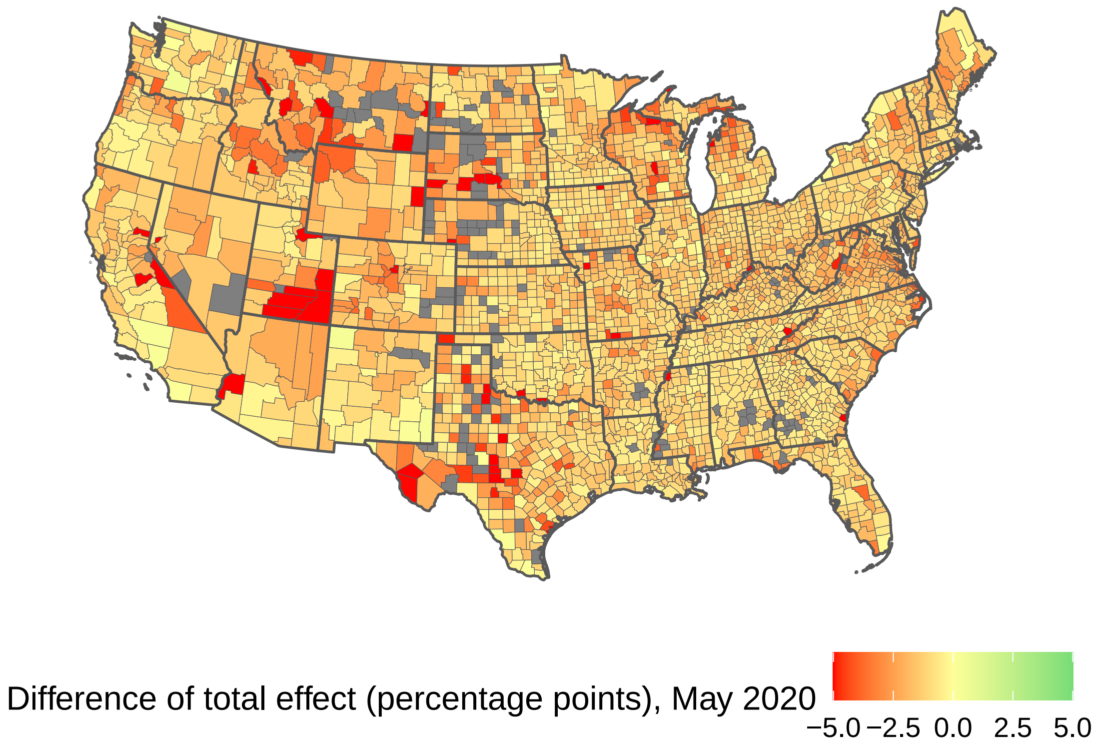
6 P.O.I. Employment and Consumers Mobility

While we have already found evidence that consumption-driven spillovers are relevant explanatory variables for local employment changes, and that this effect is causal, here we provide further evidence for a micro-level mechanism behind these results. We build a POI-level foot-fall proxy for employment, and we estimate a linear model of how neigh-

Figure 5: Spatial effects of lockdowns and spillovers



(a) Predicted effects of the model including commuting and consumption spillovers. Employment Index (2019 county average = 1), QCEW data



(b) Differences between predicted effects of the spillovers model, against the no-spillover model. Employment Index (2019 county average = 1), QCEW data

Table 5: Fitted employment effects of lockdowns (percentage points), May 2020

	Large C. Metro		Large F. Metro		Medium Metro		Small Metro		Micropolitan		Non-Core	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Direct lockdown effect	-4.8	1.8	-5.1	2.1	-3.4	3.1	-2.7	2.6	-2.7	2.1	-2.4	2.6
Mobility spillover effect	-1.2	0.6	-2.2	1.0	-1.7	0.9	-1.6	0.9	-1.8	1.0	-2.1	1.4
Total lockdown effect	-6.0	2.0	-7.3	2.7	-5.1	3.6	-4.2	3.0	-4.4	2.6	-4.5	3.6
Lockdown effect (no spill. OLS)	-5.3	2.0	-5.7	2.4	-3.8	3.5	-3.0	2.9	-3.0	2.4	-2.7	2.9
Difference of total effect	-0.6	0.6	-1.6	0.9	-1.3	0.8	-1.3	0.9	-1.5	1.0	-1.8	1.3

Note: Classification of counties according to National Centre for Health Statistics's 2013 classification. All data are referred to May 2020. *Large Central Metro*: counties in MSA of 1 million or more population that (1) contain the entire population of the largest city of the MSA, or (2) have their entire population contained in the largest principal city of the MSA, or (3) contain at least 250,000 inhabitants of any principal city of the MSA. *Large Fringe Metro*: counties in MSAs of 1 million or more population that did not qualify as large central metro counties. *Medium Metro*: counties in MSAs of population of 250,000 to 999,999. *Small Metro*: counties in MSAs of population less than 250,000. *Micropolitan*: counties in micropolitan statistical areas. *Noncore*: nonmetropolitan counties that did not qualify as micropolitan.

bours' restrictions affect local businesses.

Foot-fall employment proxy. We argue that, using mobility data, we can build a foot-fall proxy for employment. We focus our analysis on those sectors where customers are unlikely to be visiting the venue for more than four hours[10]. For these sectors, we proxy the POI-level employment through the number of observed visits with a duration equal or greater to four hours. We validate the aggregate trends and the county-level correlation between our employment proxy for restaurants (NAICS 7225) and the corresponding QCEW data, finding a consistent cross-sectional correlation of approximately 0.76 between January and June 2020. In Figure 8 in the Online Appendix we show the scatterplot of the two indexes for April and May 2020. The linear correlation between the two indexes is quite large (0.77). Notice that QCEW data capture all workers who worked at least once during the month and are thus eligible for unemployment insurance, and not average monthly employment, so we do not expect the two to be correlated 1:1.

Consumers' origin proxy. Since we know the origin Census Block Group of POI visitors, we are able to build a measure of pre-pandemic exposure of each individual business premise to consumers coming from a given county.

Estimation strategy. As a novelty with respect to the literature, we estimate how the employment of individual POIs more exposed to pre-pandemic out-of-county visits

[10] An example of the opposite case are hotels, where customers are likely to remain on the premises for prolonged periods of time.

(POIs whose 2019 visitors came from outside of the county) are more affected by neighbours' business and travel restrictions. Using the same techniques employed to derive the treatment indicators and instruments for the county-level employment estimates, we build for each POI a measure of the share of 2019 visitors that reside in a county where business restrictions are in place. We also build a mobility restriction index, by using as a lockdown variable the weekly maximum between state-wide travel bans and county-level SAH orders, to capture the role of restrictions completely impeding consumer mobility.

Results. Table 6 reports the weekly OLS estimates for restaurants and catering (NAICS 7225 and 7223). Column (1) presents a baseline estimation which omits employment spillovers due to neighbours' policies. Our employment proxy for restaurants falls on average by 15.7 percentage points when the restaurant sector is mandated to close. This coefficient is likely to be dampened due to the Laplacian noise applied to anonymise the underlying data. Column (2) adds the spillovers. We predict a slightly smaller effect of closures (13.9 p.p. decrease in the long visits index) than the no-spillover specification. We find a strong correlation between local POIs employment and closures in the neighbouring counties from which the visitors of the POI come from. For a POI whose visitors all come from neighbouring counties under lockdown, we estimate a reduction of 40.5% in the employment proxy when no local closures are in place.

Since out-of-county visitors accounted for approximately 33% of all visitors before the pandemic, this implies that we estimate an additional U.S.-wide impact of neighbours' closures of up to 13.7% lower employment in restaurants at peak, without accounting for the interaction term (which does not appear to be statistically significant). Column (3) controls explicitly for whether mobility was restricted in the county of origin of the POI's 2019 visitors (excluding the POI's location itself). We do this by including travel bans toward the POI's location (due to either the origin state or the POI's one) and Stay-At-Home orders, which impose to travel only for essential reasons. When all visitors of a POI were from areas under travel ban or SAH order, we estimate a fall of 88 p.p. in the Long Visits Index. However, more than half of this effect is offset if the POI is directly

Table 6: OLS estimates of lockdown own and spillover effects on Restaurants' and Non-Essential Retail's footfall-based employment index (1 = POI's 2019 average)

	Long visits index, Restaurants			Long visits index, Non-Essential Retail		
	(1) Baseline	(2) Baseline	(3) Mobility	(4) Baseline	(5) Baseline	(6) Mobility
Closed Sector	-0.157***	-0.139***	-0.191***	-0.157***	-0.119***	-0.133***
	(0.0242)	(0.0150)	(0.0208)	(0.0242)	(0.0152)	(0.0184)
Neighbours' restaurants closures		-0.405***				
		(0.105)				
Closed Sector × Neighbours' restaurants closures		0.0128				
		(0.108)				
Neighbours' limited mobility			-0.880***			-0.430***
			(0.172)			(0.0573)
Closed Sector × Neighbours' limited mobility			0.447**			0.215***
			(0.160)			(0.0396)
Neighbours' retail closures					-0.351***	
					(0.0512)	
Closed Sector × Neighbours' retail closures					0.140**	
					(0.0536)	
Constant	0.961***	0.967***	0.969***	0.961***	0.967***	0.967***
	(0.00750)	(0.00529)	(0.00463)	(0.00750)	(0.00625)	(0.00626)
Observations	11939550	14560575	14560575	11939550	11939550	11939550
R^2	0.132	0.141	0.141	0.132	0.132	0.132
POI FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the long-visits index of individual POIs in NAICS 7225 and 7223 - for columns 1-3 labelled "Restaurants" - and in the non-essential retail sector - for columns 4-6 labelled "Non-Essential Retail" - (with each POI's 2019 average = 1) on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). The own lockdown intensity is proportional to the share of month under lockdown, whereas spillover measures are proportional to the consumption inflows between the observed county and its neighbours over population. All consumption inflows are 2019 averages from SafeGraph data. "Closed Sector" regressor refers to the specific order which closed the subset of POIs considered in the specification, equivalent to an "entertainment" (restaurants) order or "retail" order as defined in the main text. Controls include month and county X calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county, death/population ratio and cases/population ratio.

mandated to close.

We find similar evidence for non-essential retail (columns 4-6). IV specifications analogous to those previously used for the county-level analysis, shown in Appendix A, lend validity to a causal interpretation of these findings. In the Online Appendix, we provide a robustness check using a Poisson regression specification with the long visits count as dependent variable (Table 11).

6.1 Further Evidence of the Mechanism

The results presented so far show that POIs whose 2019 visitors came from locations more subject to business and travel restrictions were also more likely to experience a reduction in their employment proxy. This supports the assertion that the dynamics observed at an aggregate level, where we find that neighbours' lockdowns cause local falls in employment, is not only a macroeconomic phenomenon, but is also present in the employment dynamics of individual shops.

To provide further direct evidence of the potential mechanism underlying these results. Instead of collapsing the exposure to neighbours' policy into a unique treatment variable for each POI, we now analyse how directed flows of visitors from individual states to individual POIs are affected by local and neighbours' restrictions. That is, for each POI i in state j' and week t, we observe the visitor count $\text{visitors}_{i(j')jt}$, where j is the visitors' origin state. We examine how these incoming consumer flows vary across time with local and neighbouring states' lockdown intensity.

Given the nature of the data, we present results from a Poisson regression. We estimate the following specification by maximum likelihood under the assumption that the dependent variable is Poisson distributed conditional on the independent variables $y|x \sim \text{Poisson}(\lambda = \theta x)$:

$$\begin{aligned} E\left(\text{visitors}_{i(j')jt}|X\right) = \exp\Big(&\gamma_1 L^{Z(i)}_{j't} \times \mathbb{1}(j = j') + \gamma_2 L^{Z(i)}_{j't} \times \mathbb{1}(j \neq j') + \gamma_3 L^{\text{mobility}}_{jt} \\ &+\gamma_4 L^{\text{mobility}}_{jt} \times L^{Z}_{j't} + \alpha_{i(j')j} + \alpha_t + \ln(\alpha)\Big) \end{aligned} \tag{15}$$

We report the results in Table 7. The coefficients represent the increase in the expected value of the log visitors count for a small change in the independent variable. Thus, the estimates can be interpreted as approximate semi-elasticities, subject to the usual caveat for large coefficients.

Table 7: Poisson Regression estimates of lockdowns' own and spillover effects on Restaurants' and Non-Essential Retail's visitors from a given state

	Visitors, Restaurants			Visitors, Non-Essential Retail		
	(1) No spillover	(2) Baseline	(3) Only neighbours	(4) No spillover	(5) Baseline	(6) Only neighbours
Same state = 1 × closed	-0.291***	-0.294***		-0.298***	-0.301***	
	(0.0290)	(0.0291)		(0.0357)	(0.0357)	
Same state = 0 × closed	-0.915***	-0.769***	-0.686***	-1.08***	-0.928***	-0.725***
	(0.0685)	(0.0636)	(0.0914)	(0.1067)	(0.1278)	(0.135)
Mobility ban		-0.232***	-0.180**		-0.343***	-0.218***
		(0.0571)	(0.0548)		(0.0557)	(0.0556)
Mobility ban × closed		-0.0399	-0.0720		0.102	0.0680
		(0.0610)	(0.0736)		(0.0818)	(0.0850)
Constant	4.033***	4.034***	2.001***	3.635***	3.636***	1.953***
	(0.0106)	(0.0106)	(0.0290)	(0.0103)	(0.0103)	(0.0305)
Observations	42332989	42332989	18197387	25586334	25586334	8410924
POIxVisitor origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. The table presents estimates from a Poisson regression of the count of total weekly visitors restaurant (NAICS 7223 and 7225) and non-essential retail (NAICS 44 and 45 excluding 445, 446, 447, 4413, 4414, 4523) POIs on measures of the own state lockdown intensity and spillovers from neighbouring states' travel bans. The observation level is POI by week by visitor state of origin. The dependent variable is the count of visitors, calculated as the total number of geolocated unique weekly visitors (irrespective of visit length) to the POI premises. In columns (3) and (6), we exclude observations where the origin state is different from the state in which the POI is located. Controls include county by week fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county.

Excluding fixed effects, the average number of visitors is 4 per POI per month. Column (1) includes only restaurant POIs and does not include spillovers. We find that closing a POI corresponds to a fall of 0.91 log-points in the count of visitors from states different from the one the POI is located in and a fall 0.29 log-points for the flows from the same state. In specification (2), we include a variable capturing whether mobility is banned for non-essential reasons between two states due to either a travel ban or a SAH order. This explains an average fall of 0.23 log-points of those flows affected by mobility restrictions. The interaction between closures and neighbours' mobility restrictions is not significantly different from zero. Specifications (4) to (6) replicate the results for non-essential retail

POIs, providing qualitatively comparable results.

Together, these sets of results show that the effect previously observed at the county level corresponds to what is happening at the level of individual businesses. Neighbours' closures cause falls in the POI employment proxy, and are associated with falls in the number of visitors from the state where they are enacted. This establishes a direct, measurable connection between observed employment figures at both the county and POI level, and measures taken in distant, but connected, locations.

7 Additional Results

Lockdown anticipation. In Appendix A.4 we address possible concerns about agents' anticipation of lockdown orders. Using Google Trends data, we find some evidence that own-state lockdowns were slightly anticipated in compliers. However, we find no evidence of anticipation of neighbouring states' orders.

Business revenues. Using data from Chetty, Raj and Friedman, John N and Hendren, Nathaniel and Stepner, Michael and Team, The Opportunity Insights (2020), we examine the effect of spillovers on small businesses' revenues as further evidence for our consumption-based mechanism. We find that these are negatively affected by both own and neighbours' restrictions, consistent with the fall in visits we observe at the POI-level.

Supply-chain spillovers. In Online Appendix A.5 we show, using CFS data, that our results are robust to accounting for a proxy of supply-chain disruptions. However, as precise data on domestic U.S. production links are not available, a discussion of how our results may be affected by omitting supply-chain spillovers is due.

The literature has shown how both downstream and upstream spillovers across the disrupted supply chains contribute to a reduction in local employment. Suppose that the supply-chain spillover treatment is positively correlated to our mobility spillovers, but we omit it from our specification. Then, we would be imputing at least part of the negative effect of supply-chain spillovers to mobility ones, while the rest would be ascribed to other covariates or to the residual. Thus, we would be estimating a lower bound of the total spillover effects (given by the sum of mobility and supply-chain ones), while overestimating the role of mobility spillovers. However, as we provided evidence for a direct mechanism, it is unlikely that the estimated effect of mobility spillovers could be fully attributable to supply chain spillovers.

Suppose now supply-chain spillovers are negatively correlated to mobility spillovers. Then, we would be underestimating the role of the latter. In this case, we would be estimating a lower bound of total spillovers, *and* underestimating mobility-related spillovers.

Unemployment. We repeat the same analysis of the main text for unemployment. Looking at unemployment can be interesting for several reasons. First, previous results in the literature were focused on unemployment claims. Second, unemployment is residence-based, meaning that unemployed persons are accounted for on the basis of where they live. This creates a more direct relationship with commuting flows. All our results are robust. Spillovers explain 15-20% of the total increase in residence-based unemployment.

All additional tables and figures are in Online Appendix A.

8 Conclusions

The high degree of economic interconnection across counties and states is a characteristic of the U.S. economy. For this reason, the business restrictions implemented to stop the spread of Covid-19 have generated negative mobility-related spillovers across geographically close locations. These spillovers have affected both employment and consumption.

The size of mobility-related spillover effects is large: we estimate that they account for a further 1.5-3 p.p. fall in employment (out of a total of -14 pp.) between February and April 2020. In counties highly exposed to commuting, spillovers accounted for twice these figures. Empirically, accounting for mobility spillovers implies larger negative estimates of the impact of lockdowns on employment. Our analysis contributes to explain not only aggregate effects, but also the spatial distribution of labour market outcomes during the pandemic. Using mobility data, we provide empirical causal evidence at a highly granular level of how neighbours' business restriction reduce employment and visitors of local businesses, even if the latter are not subject to restrictions.

These findings have relevant policy implications. First, the presence of spillovers poses a problem for a *national* policy-maker: local officials with the power to impose local economic restrictions may not have incentives to internalise lockdown externalities. It is unclear *whose* welfare would be maximised under this institutional framework.

Second, policy-makers should account for spillovers when designing policies. According to our results, job-retention schemes, loans or grants focused only on businesses located in areas subject to business restrictions are unlikely to have targeted all the economic actors affected by relevant lockdown externalities. This is the case of England's Local Restriction Support Grants, which provided additional support only to businesses *within* the geographical areas affected by the restrictions, but not those which experienced further losses in revenues due to spillovers.

Finally, our results provide evidence for a mechanism behind how events in one location can affect the economy of its *economic* neighbours. While we study the Covid-19 shock, our results contribute more generally to uncover the existence and estimate the relevance of economic interconnections running through consumers' and workers' mobility. The evidence for the economic relevance of networks and mobility-related mechanisms found by our and other contributions should inform future research aimed at analysing the effects of local policies, urban growth, spatially-heterogeneous trade shocks and other topics connected to spatial externalities.

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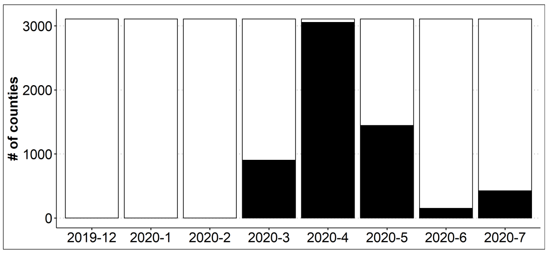
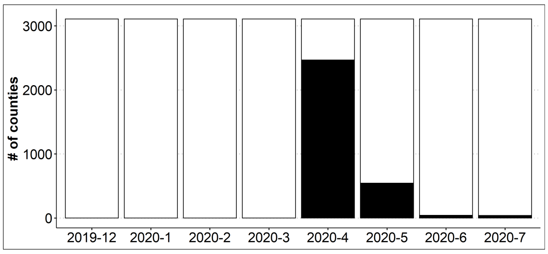
A Online Appendix - Not for publication

Additional figures and tables

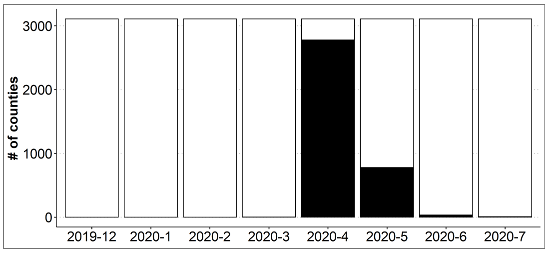
Figure 6: Number of counties in each type of lockdown over time (height of black bar corresponds to the number of counties under lockdown. A county is in lockdown if has spent at least half of the month under the corresponding order).



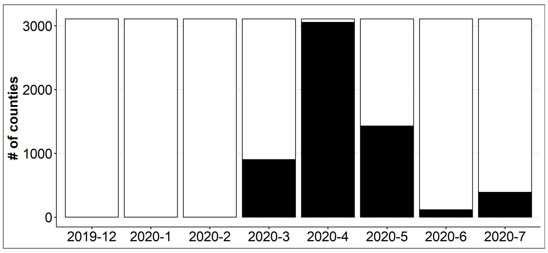
(a) Any lockdown



(b) Stay at home lockdown

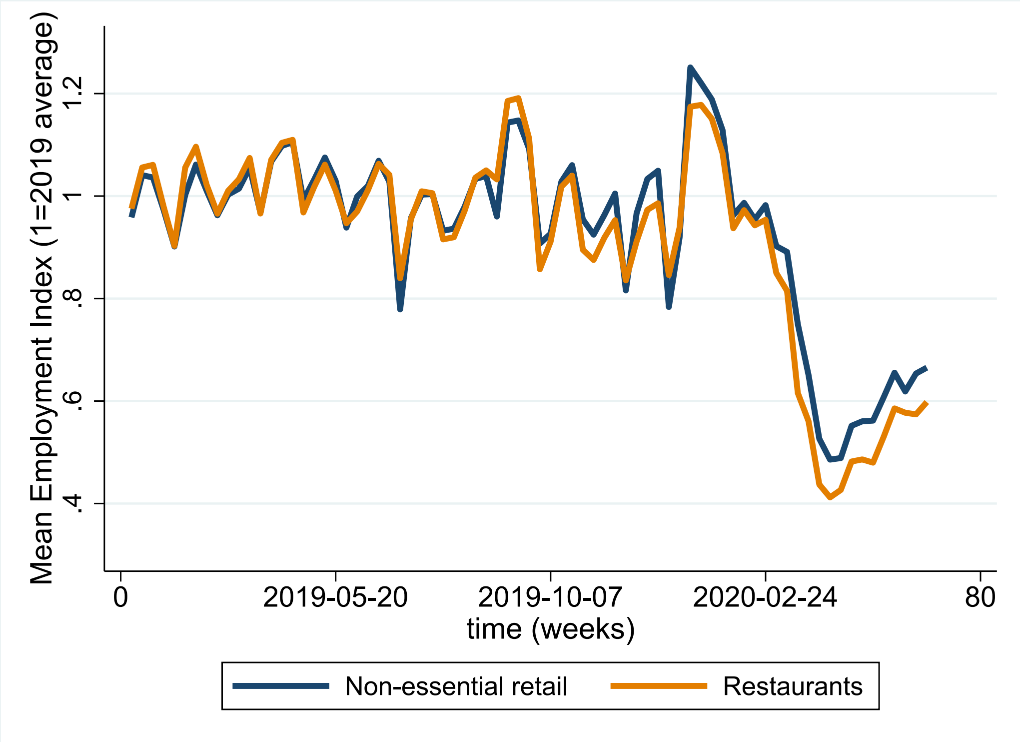


(c) Retail lockdown

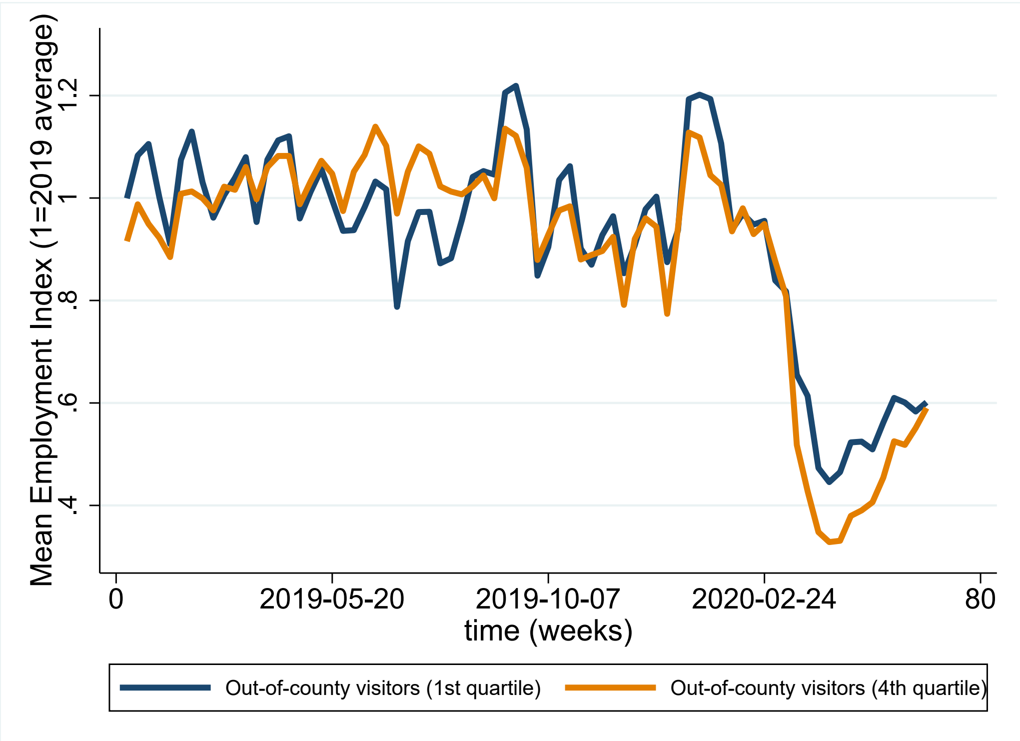


(d) Restaurant lockdown

Figure 7: Aggregate dynamics of the long-visits employment index



(a) Long-visits index, by sector



(b) Restaurants' long-visits employment index, bottom vs top quartile for consumption inflows from different counties than the POI's location

Table 8: Multiple orders' OLS estimates, outflows

	Unemployment/(working age population)				
	(1) Baseline No Spillovers	(2) Baseline	(3) Lags	(4) Exposure	(5) Exposure+Lags
share month in retail order	0.0241***				
	(0.00314)				
Share of Month in Any Lockdown	0.0148***				
	(0.00310)				
Own Retail lockdown		0.0489***	0.0274***	0.0290	0.0179
		(0.00658)	(0.00496)	(0.0162)	(0.0142)
Retail lockdown spillover		0.0234*	0.0252**	0.0264**	0.0286**
		(0.00971)	(0.00876)	(0.00976)	(0.00880)
Own Any lockdown		0.0202***	0.00991*	-0.0287***	-0.0266***
		(0.00454)	(0.00457)	(0.00860)	(0.00704)
Any lockdown spillover		0.0164	-0.0220*	0.0258**	-0.0149
		(0.0110)	(0.0110)	(0.00966)	(0.00987)
L.Own Retail lockdown			0.0387***		0.0125
			(0.00545)		(0.00936)
L.Own Any lockdown			0.0324***		0.0294**
			(0.00524)		(0.0113)
L.Retail lockdown spillover			0.0672***		0.0704***
			(0.0110)		(0.0109)
L.Any lockdown spillover			0.0248**		0.0284***
			(0.00849)		(0.00827)
Own Retail lockdown × Exposure				0.223	0.147
				(0.131)	(0.118)
Own Any lockdown × Exposure				0.374***	0.260***
				(0.0659)	(0.0515)
L.Own Retail lockdown × L.Exposure					0.236***
					(0.0685)
L.Own Any lockdown × L.Exposure					0.0397
					(0.0841)
Observations	133644	133644	130536	129759	126741
R^2	0.881	0.889	0.901	0.898	0.911
Unemployment mean 02-2020	.025	.025	.025	.025	.025
County and Month FEs	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes
Industry Exposure	No	No	No	Yes	Yes
Joint significance spillover	-	.0006	0	0	0

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the unemployment to working population ratio on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. The lockdown intensity measures are proportional to the time spent under the corresponding lockdown order. We define being under "any lockdown order" as restrictions equally or more stringent than mandated bar closures, whereas "retail lockdown" orders correspond to being under restrictions equally or more stringent than closing non-essential retail and leisure businesses. The own lockdown intensity is proportional to the own-county commuting flow, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. In columns 4 and 5 we also include a measure of the local market area's industrial exposure to the effects of Covid-19.

Table 9: IV estimates of Restaurants' employment proxy

	Long visits index, Restaurants	
	(1) No Spillover	(2) Baseline IV
Closed Sector	-0.187***	-0.120***
	(0.0198)	(0.0169)
Neighbours' restaurants closures		-0.429**
		(0.132)
Closed Sector × Neighbours' restaurants closures		-0.0816
		(0.129)
Constant	0.952***	
	(0.00757)	
Observations	14560575	14560575
R^2	0.139	0.004
Kleinberg-Paap F-stat		69.7
POI FE	Yes	Yes
Time FE	Yes	Yes
Covid controls	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the long-visits index of individual POIs in NAICS 72225 (with each POI's 2019 average = 1) on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). We instrument spillover intensity with the instruments outlined in section 5.3. The own lockdown intensity is proportional to the share of month under lockdown, whereas spillover measures are proportional to the consumption inflows between the observed county and its neighbours over population. All consumption inflows are 2019 averages from SafeGraph data. "Closed Sector" regressor refers to the specific order which closed the subset of POIs here considered, equivalent to an "entertainment" (restaurants) order as defined in the main text. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county, death/population ratio and cases/population ratio.

Table 10: IV estimates of Non-Essential Retail's employment proxy

	Long visits index, Non-Essential Retail	
	(1)	(2)
	Baseline	Baseline IV
Closed Sector	-0.157***	-0.0506**
	(0.0242)	(0.0186)
Neighbours' retail closures		-0.616***
		(0.0916)
Closed Sector × Neighbours' retail closures		0.0925
		(0.0649)
Constant	0.961***	
	(0.00750)	
Observations	11939550	11939550
R^2	0.132	0.002
Kleinberg-Paap F-stat		91.85
POI FE	Yes	Yes
Time FE	Yes	Yes
Covid controls	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the long-visits index of individual POIs in the non-essential retail sector (with each POI's 2019 average = 1) on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). We instrument spillover intensity with the instruments outlined in section 5.3. The own lockdown intensity is proportional to the share of month under lockdown, whereas spillover measures are proportional to the consumption inflows between the observed county and its neighbours over population. All consumption inflows are 2019 averages from SafeGraph data. "Closed Sector" regressor refers to the specific order which closed the subset of POIs here considered, equivalent to a "retail" order as defined in the main text. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county, death/population ratio and cases/population ratio.

Table 11: Poisson Maximum Likelihood estimates of long-visits proxy

	Long visits index, Restaurants			Long visits index, Non-Essential Retail		
	(1) pois_retail_ns_dy	(2) pois_restrnts_baseline_dy	(3) pois_restrnts_mobility_dy	(4) pois_retail_ns_dy	(5) pois_retl_baseline_dy	(6) pois_retl_mobility_dy
Closed Sector	-2.522***	-1.689***	-2.461***	-2.522***	-1.866***	-2.032***
	(0.434)	(0.252)	(0.303)	(0.434)	(0.246)	(0.275)
Neighbours' restaurants closures		-6.258***				
		(1.708)				
Neighbours' limited mobility			-14.89***			-5.247***
			(3.237)			(1.336)
Neighbours' retail closures					-4.384***	
					(1.142)	
Observations	1185675	1467225	1467225	1185675	1185675	1185675
R^2						
POI FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Covid contrpois	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. All coefficients represent changes - ceteris paribus - in the log of E(y—x), where $y \sim \text{Poisson}(\beta x)$.

A.1 Derivation of Decomposition

In principle, any county i' can affect through its lockdown decisions the outcomes of any flow E_{ic}, including any $c, i \neq i'$. This regardless of direct connections or distance. For example, sufficiently large job losses in the manufacturing sector in Europe could trigger - through a cascade of network and general equilibrium effects - job losses at a local restaurant in the U.S. West Coast. To take care of this problem, we need to make an assumption about what are the relevant cross-counties effects which we allow for. We make the following two assumptions:

Assumption 1 $\frac{\partial E_{ic}}{\partial L_{i'}} = 0 \; \forall i' \neq c$

Assumption 2 $\frac{\partial M_{ic}}{\partial L_{i'}} = 0 \; \forall i' \neq i$

The first assumption is equal to assuming that jobs in county c cannot be affected *directly* by lockdowns in county i. This is a reasonable assumption if, by focusing on mobility spillovers, we think that cross-border flows of workers are not affected, as for legal and political reasons travel for "work" reasons is deemed as an essential activity. In fact, this is what the various laws and rules have specified. The second assumption means that the various $M_{ic}(L_t)$ can only be affected by lockdowns in i. If we think about M_{ic} as the consumption flows of residents of i which spend in county c, then this seems a reasonable assumption as long as we ignore second-order effects (such as the lockdown in county j causing job losses in county i, and *through that*, further reductions in consumption in county c). In fact, if we assume that second-order effects are negligible, there is no reason why business restrictions in counties different than i should affect i's consumers' behaviour. Later, we will relax this assumption to allow for the fact that also policies in c can affect i's consumption behaviour, as shops may have already been closed.

Under these assumptions, we can simplify Equation 3 as:

$$E^e_{ct} \approx E^e_{c0} + \sum_{i \in C} \frac{\partial E_{ic}}{\partial L_c} \Delta L_{ct} + \sum_{i \in C} \sum_{z \in C} \frac{\partial E_{ic}}{\partial M_{zc}} \frac{\partial M_{zc}}{\partial L_z} \Delta L_{zt} + \sum_{i \in C} \frac{\partial E_{ci}}{\partial X_{ci}} \Delta X_{cit} \tag{16}$$

In order to achieve an estimable equation, we further need to assume some regularity in the way each work or consumption flow respond to changes in L. We assume that all semi-elasticities of E and M are equal across all pair.

Assumption 3 *1.* $\frac{\partial E_{ic}}{E_{ic}\partial L_i} = \frac{\partial E_{i'c}}{E_{i'c}\partial L_{i'}} \; \forall i, i' \in C$

2. $\frac{\partial E_{ic}}{E_{ic}\partial M_{zc}} = \frac{\partial E_{i'c}}{E_{i'c}\partial M_{z'c}} \; \forall i, z, i', z' \in C$

3. $\frac{\partial M_{zc}}{M_{zc}\partial L_z} = \frac{\partial M_{zc}}{M_{zc}\partial L_{z'}} \; \forall z, z' \in C$

Finally, in order to allow for the fact that local residents may work in other counties but consume locally, and thus assuming that M_{cc} cannot be affected by orders in other counties can be a very strong assumption, we relax Assumption 2 in the following way. We assume that $M_{cc} \propto \sum_{i \in C} E_{ci}$. That is, consumption in c from residents in c is proportional to the employed residents in c. In a similar fashion to the previous assumptions, assume that:

Assumption 4 *1.* $M_c c \propto \sum_{i \in C} E_{ci}$

2. $\frac{\partial E_{ci}}{\partial L_{i'}} = 0 \; \forall i' \neq c$

3. $\frac{\partial E_{ci}}{E_{ci}\partial L_i} = \frac{\partial E_{ci'}}{E_{c'i}\partial L_{i'}} \; \forall i, i' \in C$

After some algebra, this leads from Equation 16 to Equation 4 in the main text. Notice how this seemingly innocuous assumption actually implies a stronger implicit one: that all commuting flows and lockdowns are the same. This seems unreasonable, if we think about the sector composition of local employment. In fact, we are implicitly assuming that closing the leisure sector would affect a tourism-intensive economy as much as a tech-intensive one. While the results up to here are sufficient to provide a compact treatment and an intuitive explanation of the relevant facts, we derive further results which account for local sector compositions in the next section.

Lockdown and Sectors specificity

Different locations have different employment compositions, and lockdowns may target specific sectors of the economy. If this is the case, Equation 4 is not a good approximation of the effects of lockdowns.

Here, we derive a decomposition which allows for this phenomenon.

Assumption 5 *Suppose there are $j = 1, 2, \dots, J$ sectors so that $\sum_{j=1}^{J} E_{cij} = E_{ci}$ and $\sum_{j=1}^{J} E_{icj} = E_{ic}$. Suppose $L_i = (L_{i1}, \dots, L_{iJ})$. Then,*

1. $\dfrac{\partial E_{cij}}{\partial L_{ij'}} = 0 \;\; \forall j \neq j'$
2. $\dfrac{\partial E_{icj}}{\partial L_{cj'}} = 0 \;\; \forall j \neq j'$
3. $\dfrac{\partial E_{cij}}{E_{cij}\partial L_{ij}} = \dfrac{\partial E_{cij'}}{E_{cij'}\partial L_{ij'}} \;\; \forall j \neq j'$
4. $\dfrac{\partial E_{icj}}{E_{cij}\partial L_{ij}} = \dfrac{\partial E_{icj'}}{E_{cij'}\partial L_{ij'}} \;\; \forall j \neq j'$

Under these and the former assumptions, we obtain that the first term of the decomposition can be simplified into

$$\sum_{i} \sum_{j \in J} \frac{\partial E_{icj}}{E_{icj}\partial L_{cj}} E_{icj} = \frac{\partial E_c^e}{E_c^e \partial L} E_c^e \sum_{j \in J} \frac{E_{cj}}{E_c^e} \Delta L_{cj} \tag{17}$$

While the second is

$$\sum_{i \in C} \sum_{i} \sum_{j \in J} \frac{\partial E_{ic}}{\partial E_{cij}} \frac{\partial E_{cij}}{\partial L_{ij}} \Delta L_{ijt} = \frac{\partial E_c^e}{E_c^e \partial E} \frac{\partial E}{E \partial L} \sum_{i \in C} \left(\sum_{j \in J} L_{ijt} \times \frac{E_{ij}}{E_i^e} \right) E_{ci} \tag{18}$$

The first equation is the same appearing in Equation 7 in the main text, while the second is, up to the normalisation, the same as the one in 8. The first term is then the derivative of employment, multiplied by the weighted average of measures of sectorial lockdown intensities, where the weights are county c's sector employment shares. Similarly, the second term weights spillovers both by the size of the affected sectors and by the links between county c and i.

A.2 Data cleaning and collection

Labour Market Data

We use LAU and QCEW data "as they are". Since the data suffer from seasonality, in each regression we include a county by calendar month (January, February, ...) fixed effect. We drop Alaska, Hawaii and all U.S. oversea territories from all our analyses. We use data from January 2017 in each estimation involving LAU or QCEW variables. For LAU data, we use observations up to July 2020. For QCEW data, we use observations up to June 2020.

Labour Market Exposure

We build the local labour market exposure using QCEW data. For all regressions including an "exposure" term, we use 2019 yearly average QCEW data at the county level. We use the "leisure" supersector and the overall employment of a given county and compute

$$\text{Exposure}_i = \frac{(\text{Leisure Employment})_i^{2019}}{(\text{Total Employment})_i^{2019}}$$

In all regressions where exposure is among the controls but is not interacted with local lockdowns, we use the Labour Market Area (LMA) exposure to capture how the average exposure of the local commuting area may be affecting the employment of all counties. This is calculated as

$$\text{Exposure}_{\text{lma}(i)} = \sum_{j \in C:\text{lma}(j)=\text{lma}(i)} \alpha_j \text{Exposure}_j$$

For α_j being the share of employment within of the LMA of county j

We create industry weights to create the indicator at Equation 8 using QCEW 2019 local area data. We determine the weights using the following NAICS:

1. Bar orders: NAICS 72 - NAICS 7225

2. Entertainment orders: (NAICS 71, 72, 812) - bar orders
3. Retail orders: (NAICS 44, 45) - (NAICS 445, 446, 447)
4. SAH orders: (NAICS 31, 32, 33) $\times$ (state-level share of non-essential manufacturing sectors)

Mobility Data

We exclude all POIs for which we have no observation in 2020 or in 2019. That is: we exclude all POIs which were not open in 2019 (or for which we have zero visits) or that closed before the start of 2020. We derive a matrix of visitors flows from each county FIPS to each POIs. Due to the data censoring used in the data source, 1 visit from a given Census Block Group is reported as zero and all observations between 2 and 4 are reported as 4. To avoid estimating too small falls in visitors when the observations fall below 5 but not below 2, we set all observations between 2 and 4 to 2.

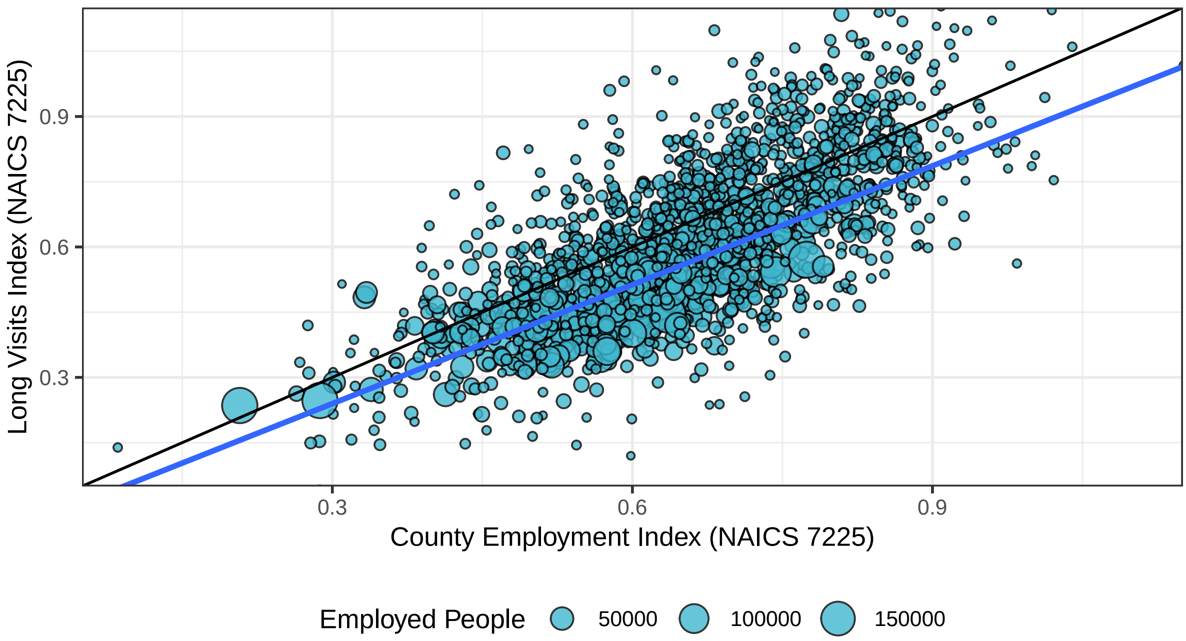
To build our consumption flows weights for lockdown spillovers, we consider only NAICS 71 and 72.

For our POI-level foot-fall employment proxy, we consider separately restaurants (NAICS 7225, plus catering services NAICS 7223) and non-essential retail (NAICS 44, 45 minus 445, 446, 447, 4413, 4414, 4523). We calculate the average long visits per week over 2019 for each POI (after having performed the substitutions described in the first paragraph of this section), and drop all those POIs whose average long visits in 2019 fall within the censored range (< 4). To exclude outliers, we drop the top 1% of the 2019 mean long visits distribution separately for restaurants and non-essential retail to trim outliers. For POIs which were open less than all 52 weeks in 2019, we use as 2019 mean the conditional average of all non-missing observations. All employment-proxy regressions are weighted by the 2019 long visitors mean number, so that the coefficients capture the fall in the overall POIs' average employment, and not the average deviation within each POI.

As an additional note, Safegraph censors all its data on visits, visitors and long visits through a randomised laplacian noise. If, after the noise is added, the number of visits falls into the 2-4 range, then it is set as 4. If it is one or zero, it is set at zero. This implies that our estimates are likely to suffer from an attenuation bias, as our data are noised up, reducing the correlation. The censoring introduces another problem, as POIs with low average 2019 long visits are likely to have been heavily censored, containing a low signal-to-noise ratio. To take care of this, drop all POIs with less than 4 average weekly long visits in 2019, ad these are likely to have been affected by large levels of noise. In fact, recall that Safegraph's data are based on a panel of the population. It would then be either: i) unlikely that any shop with less than an average of 4 weekly long visits captures workers/shifts or ii) the signal-to-noise ratio in these observations would be extremely low, as it would be the symptom of a frequent censoring to zero (when actual data + noise ≤ 1). We choose a cutoff of 4 as it is the upper bound of the "censoring area": shops with less than 4 average visits would likely be heavily censored in more than half of the observations. Our results are robust to different cutoffs, such as 1 and 5 (results provided on request). All regressions are weighted by 2019 average long visits.

Finally, the reader may be concerned about whether "long visits" may actually capture workers in shopping centres and malls, where consumers may spend long hours as these may contain cinemas, restaurants and retail shops all within the same building. However, Safegraph splits shopping centres into the individual shops within the building. Moreover, by dropping the top 1% of POIs by average 2019 long visits, we are likely to exclude POIs with extreme behaviours, possibly due to a misclassified NAICS or to particular behaviours in the specific business. As these behaviours/misclassifications are likely to be systematic, we would expect that POIs suffering from either the problems will have a very large number of long visits. Dropping the extreme tail is likely to solve or at least alleviate any problem arising from these phenomena.

Figure 8: QCEW employment index for the Restaurants sector (NAICS 7225) and Long Visits Index for NAICS 7225, U.S. Counties, April and May 2020



Note: The blue line represents the linear fit of the correlation. The black line is the bisectrix. For readability, only counties with at least 1000 average people employed in NAICS 7225 in 2019 are reported. All counties for which data for the sector are flagged as incomplete in QCEW data are dropped. In the calculation of the Long Visits Index, all POIs with less than a weekly average of 4 long visits in 2019 are dropped. Long Visits Index = 1 represents the 2019 average.

A.3 Rescaled Instrument

Here we illustrate the derivation of the rescaled instrument. Recall that the non-rescaled instrument is

$$\kappa^{\text{outflow}}_{\text{IV},Ztc} = \frac{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\text{Population}}$$

Which we use to instrument

$$\kappa^{\text{outflow}}_{Ztc} = \frac{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} L^{\text{Z}}_{tr} * \text{flow}_{cr}}{\text{Population}} + \frac{\sum\limits_{r \in C: r \neq c, s(r) = s(c)} L^{\text{Z}}_{tr} * \text{flow}_{cr}}{\text{Population}}$$

Then, one can see the non-rescaled instrument as imposing $L^{\text{s}}_{tr} = O^{\text{s}}_{ts(r)}\ \forall r : s(r) \neq s(c)$ and $L^{\text{s}}_{tr} = 0\ \forall r : s(r) = s(c)$. This may not be the best exogenous predictor of $\kappa^{\text{outflow}}_{Xtc}$. In fact, it may be that the variation exogenous from y_c registered at one's neighbouring states is able to explain the lockdown intensity observed among neighbours within the same state. Then - among many possibilities - we can create an instrument which takes this into account by imposing

$$L^{\text{Z}}_{tr'} = \frac{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} \text{flow}_{cr}}\ \forall r' : s(r') = s(c), r' \neq c \tag{19}$$

From which we obtain

$$\begin{aligned} \kappa^{\text{outflow}}_{\text{IV},Ztc} =& \frac{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\text{Population}} \\ &+ \frac{\sum\limits_{r \in C: r \neq c, s(r) = s(c)} \left(\frac{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\sum\limits_{r \in C: r \neq c, s(r) \neq s(c)} \text{flow}_{cr}} \right) * \text{flow}_{cr}}{\text{Population}} \end{aligned} \tag{20}$$

Algebra leads to

$$\kappa^{\text{outflow}}_{\text{IV},Ztc} = \frac{\sum_{r\in C:r\neq c,s(r)\neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\text{Population}} \frac{\sum_{r\in C:r\neq c,s(r)\neq s(c)} \text{flow}_{cr}}{\sum_{r\in C:r\neq c,s(r)\neq s(c)} \text{flow}_{cr}} + \left(\frac{\sum_{r\in C:r\neq c,s(r)\neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\sum_{r\in C:r\neq c,s(r)\neq s(c)} \text{flow}_{cr}} \right) \frac{\sum_{r\in C:r\neq c,s(r)=s(c)} *\text{flow}_{cr}}{\text{Population}} \tag{21}$$

Where rearranging allows to write this as

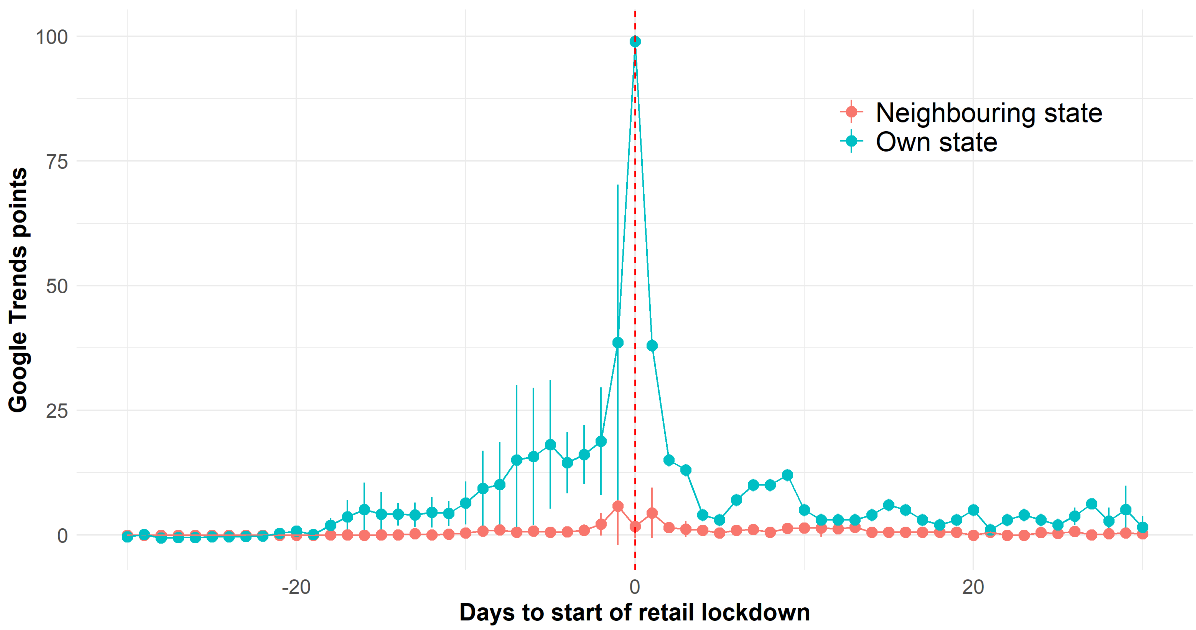
$$\kappa^{\text{outflow}}_{\text{IV},Ztc} = \left(\frac{\sum_{r\in C:r\neq c,s(r)\neq s(c)} O^{\text{Z}}_{ts(r)} * \text{flow}_{cr}}{\sum_{r\in C:r\neq c,s(r)\neq s(c)} \text{flow}_{cr}} \right) \frac{\sum_{r\in C:r\neq c,s(r)\neq s(c)} \text{flow}_{cr} + \sum_{r\in C:r\neq c,s(r)=s(c)} \text{flow}_{cr}}{\text{Population}} \tag{22}$$

Where the numerator of the second term is just the sum of all flows toward counties $r \neq c$, leading to Equation 14.

A.4 Google Trends and Lockdown Anticipation

A worry about our estimates is that own and neighbours' lockdowns may be anticipated by announcements. Thus, employers, employees and consumers may take early actions such as closing earlier than the lockdown date itself or, viceversa, going to pre-lockdow runs which may require calling in extra staff or delay lay-offs previously planned due to other reasons. Such anticipation effects could invalid our analysis. Using Google Trends, we inspect the attention to state-level neighbours' lockdowns. We find some evidence for anticipation/early attention of up to two weeks of own closure measures, driven mainly by "late" implementers. For early implementers, we find no anticipation beyond a few days from the implementation due to the lag announcement-implementation and early news speculations on governors' plans. However, we find no anticipation for neighbours' policies, suggesting that we fully capture the effects of neighbours' lockdowns.

Figure 9: Event study for anticipation of retail closures: own order and neighbouring states' orders



A.5 Downstream supply chain disruption

Table 12: OLS estimates, outflows

	Establishment-based Employment index (1 = 2019 employment)			
	(1)	(2)	(3)	(4)
	No Spillovers	Baseline	Lags	IV (rescaled)
Own lockdown intensity	-0.253***	-0.238***	-0.161***	-0.179***
	(0.0189)	(0.0253)	(0.0197)	(0.0305)
Commuting spillover		-0.261*	-0.245*	-0.296
		(0.127)	(0.0964)	(0.155)
Downstream supply chain spillover		0.0277	-0.0160	-0.107*
		(0.0278)	(0.0201)	(0.0433)
Consumption Spillover		-0.394***	-0.0700	-0.273**
		(0.0720)	(0.0876)	(0.104)
Consumption Spillover × share month in ent. order		0.289***	0.0128	0.185
		(0.0875)	(0.111)	(0.115)
L.Own lockdown intensity			-0.144***	
			(0.0219)	
L.Commuting spillover			-0.0651	
			(0.0752)	
L.Downstream supply chain spillover			0.0688***	
			(0.0202)	
L.Consumption Spillover			-0.0168	
			(0.104)	
L.Consumption Spillover × L.share month in ent. order			-0.0922	
			(0.102)	
Constant	0.983***	0.983***	0.984***	
	(0.000881)	(0.00126)	(0.00120)	
Observations	123606	123606	120663	123606
R^2	0.815	0.817	0.825	0.068
Kleinberg-Paap F-stat				212.71
CountyxMonth and Time FEs	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes
Industry Exposure	Yes	Yes	Yes	Yes

Note: Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001.

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents the same results as the main text results, but with the addition of a proxy derived from CFS 2017 shipment data, capturing how much a county's manufacturing and wholesale trade sectors are exposed to closures in the counties where their production is shipped. The term itself does not appear to be significant in all OLS specification, but it is in the IV one. All other spillover coefficients remain significant and similar to the findings in the main text.

A.6 Small Business Revenue

Table 13: NAICS-Weighted lockdown impact indicator

	Small Business revenues: percentage change from January 2020					
	(1)	(2)	(3)	(4)	(5)	(6)
	No Spillovers	Baseline	Lags	IV (rescaled)	IV (non-rescaled)	IV (Lags, res)
Own lockdown intensity	-0.253***	-0.229***	-0.178***	-0.213***	-0.129***	-0.164***
	(0.0189)	(0.0214)	(0.0185)	(0.0219)	(0.0319)	(0.0190)
Commuting spillover		-0.285*	-0.274**	-0.473**	-1.476***	-0.409***
		(0.127)	(0.102)	(0.146)	(0.285)	(0.115)
L.Own lockdown intensity			-0.138***			-0.131***
			(0.0193)			(0.0189)
L.Commuting spillover			-0.0651			-0.155
			(0.0827)			(0.0935)
Constant	0.983***	0.984***	0.985***			
	(0.000881)	(0.00105)	(0.00114)			
Observations	123606	123606	120663	123606	123606	120663
R^2	0.815	0.816	0.824	0.078	0.027	0.104
Kleinberg-Paap F-stat				2163.09	139.61	900.63
Unemployment mean 02-2020	.025	.025	.025	.025	.025	.025
CountyxMonth and Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry Exposure	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the small businesses' revenue index (with January 2020 average = 1) on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. In columns 4, 5 and 6 we instrument spillover intensity with the instruments outlined in section 5.3. The instruments exploit variation in neighbouring states' lockdown orders, which we argue are exogenous to unobservables that may also determine the own county outcomes. The lockdown intensity measure is proportional to the time spent under the corresponding lockdown order. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). The own lockdown intensity is proportional to the share of own flows, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours over population. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. All specifications include a measure of the local market area's industrial exposure to the effects of Covid-19.

A.7 Tables - Unemployment

A.7.1 OLS

Table 14: OLS estimates, outflows

	Unemployment/(working age population)					
	(1) No Spillovers	(2) No Spillovers Alt.	(3) Baseline	(4) Lags	(5) Exposure	(6) Exposure+Lags
Share of Month in Any Lockdown	0.0187***					
	(0.00281)					
Own Any lockdown		0.0417***	0.0326***	0.0228***	-0.0249*	-0.0292**
		(0.00850)	(0.00470)	(0.00490)	(0.0115)	(0.0108)
Any lockdown spillover			0.0204*	-0.00929	0.0288***	-0.00278
			(0.00961)	(0.00944)	(0.00784)	(0.00808)
L.Own Any lockdown				0.0430***		0.0306*
				(0.00473)		(0.0123)
L.Any lockdown spillover				0.0661***		0.0713***
				(0.0120)		(0.0117)
Own Any lockdown × Exposure					0.460***	0.408***
					(0.0925)	(0.0857)
L.Own Any lockdown × L.Exposure						0.128
						(0.106)
Constant	0.0299***	0.0300***	0.0300***	0.0286***	0.0302***	0.0287***
	(0.000210)	(0.000269)	(0.000835)	(0.000719)	(0.000865)	(0.000757)
Observations	133644	133644	133644	130536	129759	126741
R^2	0.876	0.878	0.884	0.891	0.890	0.899
Unemployment mean 02-2020	.025	.025	.025	.025	.025	.025
County and Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry Exposure	No	No	No	No	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the unemployment to working population ratio on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. The lockdown intensity measures are proportional to the time spent under any lockdown order, where we define "any lockdown order" as any order that is equally or more stringent than mandated bar closures. The own lockdown intensity is proportional to the own-county commuting flow as described in the main text, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. In columns 5 and 6, we also include a measure of the labour market area's industrial exposure to the effects of Covid-19.

A.7.2 IV estimates

Table 15: Single instrument estimation for Any lockdown spillover

Panel A: First Stage

	Baseline	Lags		Exposure	Exposure + Lag	
	(1)	(2)	(3)	(4)	(5)	(6)
	Any lockdown spillover	Any lockdown spillover	L.Any lockdown spillover	Any lockdown spillover	Any lockdown spillover	L.Any lockdown spillover
Any Lockdown Spillover IV (rescaled)	0.983***	0.831***	-0.00530	0.976***	0.823***	-0.00519
	(0.0156)	(0.0191)	(0.0118)	(0.0157)	(0.0196)	(0.0121)
Own Any lockdown	0.165***	0.180***	0.00813	0.246***	0.280***	0.0164
	(0.00961)	(0.0110)	(0.00494)	(0.0174)	(0.0191)	(0.00876)
L.Any Lockdown Spillover IV (rescaled)		0.193***	1.032***		0.195***	1.029***
		(0.0345)	(0.0171)		(0.0345)	(0.0171)
L.Own Any lockdown		-0.0486*	0.128***		-0.0930***	0.153***
		(0.0189)	(0.0103)		(0.0242)	(0.0145)
Own Any lockdown × Exposure				-0.693***	-0.832***	-0.0728
				(0.109)	(0.109)	(0.0581)
L.Own Any lockdown × L.Exposure					0.342**	-0.223***
					(0.107)	(0.0620)
Observations	133644	130536	130536	129759	126741	126741

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from the first stage of a model regressing the unemployment to working population ratio on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders, where we instrument spillover intensity with the rescaled instrument outlined in section 5.3.

Panel B: Results

	Unemployment/(working age population)					
	(1)	(2)	(3)	(4)	(5)	(6)
	No Spillovers	No Spillovers Alt.	Baseline	Lags	Exposure	Exposure+Lags
Share of Month in Any Lockdown	0.0187***					
	(0.00281)					
Own Any lockdown		0.0417***	0.0327***	0.0220***	-0.0276*	-0.0316**
		(0.00850)	(0.00470)	(0.00445)	(0.0112)	(0.0104)
Any lockdown spillover			0.0405**	0.00356	0.0499***	0.00867
			(0.0126)	(0.00887)	(0.0109)	(0.00774)
L.Any lockdown spillover				0.0721***		0.0804***
				(0.00942)		(0.00909)
L.Own Any lockdown				0.0466***		0.0337**
				(0.00526)		(0.0125)
Own Any lockdown × Exposure					0.484***	0.423***
					(0.0919)	(0.0866)
L.Own Any lockdown × L.Exposure						0.134
						(0.106)
Observations	133644	133644	133644	130536	129759	126741
R^2	0.876	0.878	0.107	0.168	0.154	0.226
Kleinberg-Paap F-stat			3944.17	1389.93	3884.22	1319.95
Unemployment mean 02-2020	.025	.025	.025	.025	.025	.025
County and Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry Exposure	No	No	No	No	Yes	Yes

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the unemployment to working population ratio on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders, where we instrument spillover intensity with the rescaled instrument outlined in section 5.3. The instrument exploits variation in neighbouring states' lockdown orders, which we argue are exogenous to unobservables that may also determine the own county outcomes. The lockdown intensity measures are proportional to the time spent under the corresponding lockdown order. We define being under any lockdown order as restrictions equally or more stringent than mandated bar closures. The own lockdown intensity is proportional to the own-county commuting flow, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. In columns 5 and 6, we also include a measure of the local market area's industrial exposure to the effects of Covid-19.

A.7.3 Sectors-weighted Lockdown index

Table 16: NAICS-Weighted lockdown impact indicator

Panel A: First Stage

	IV (neighbours)	IV (neighbours 2)	IV (lags)	
	(1) Lockdown spillover	(2) Lockdown spillover	(3) Lockdown spillover	(4) L.Lockdown spillover
Lockdown Spillover IV (rescaled)	0.968***		0.883***	0.0127*
	(0.0168)		(0.0146)	(0.00611)
Own lockdown	0.101***	-0.0200	0.106***	-0.00897
	(0.00923)	(0.0145)	(0.00917)	(0.00507)
Lockdown Spillover IV (not rescaled)		0.894***		
		(0.0790)		
L.Lockdown Spillover IV (rescaled)			0.127***	0.963***
			(0.0191)	(0.0179)
L.Own lockdown			-0.0275***	0.0989***
			(0.00776)	(0.00927)
Observations	126549	126549	123606	123606

Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from the first stage of a model regressing the unemployment to working population ratio on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders, where we instrument spillover intensity with the instruments outlined in section 5.3.

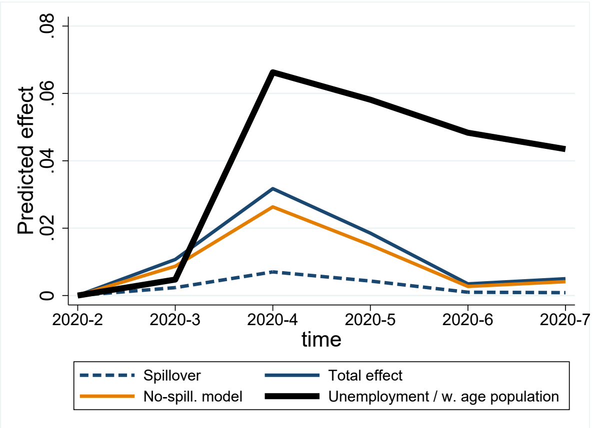
Panel B: Results

	Unemployment/(working age population)					
	(1) No Spillovers	(2) Baseline	(3) Lags	(4) IV (rescaled)	(5) IV (non-rescaled)	(6) IV (Lags, res)
Own lockdown	0.184***	0.188***	0.135***	0.191***	0.190***	0.135***
	(0.0202)	(0.0198)	(0.0183)	(0.0200)	(0.0195)	(0.0177)
Lockdown spillover		0.149***	0.0536*	0.252***	0.189*	0.103***
		(0.0322)	(0.0263)	(0.0343)	(0.0923)	(0.0238)
L.Own lockdown			0.171***			0.183***
			(0.0162)			(0.0169)
L.Lockdown spillover			0.252***			0.346***
			(0.0350)			(0.0412)
Constant	0.0293***	0.0285***	0.0273***			
	(0.000959)	(0.000919)	(0.000852)			
Observations	126549	126549	123606	126549	126549	123606
R^2	0.892	0.894	0.904	0.178	0.184	0.252
Kleinberg-Paap F-stat				3334.49	127.84	1498.06
Unemployment mean 02-2020	.025	.025	.025	.025	.025	.025
CountyxMonth and Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covid controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry Exposure	Yes	Yes	Yes	Yes	Yes	Yes

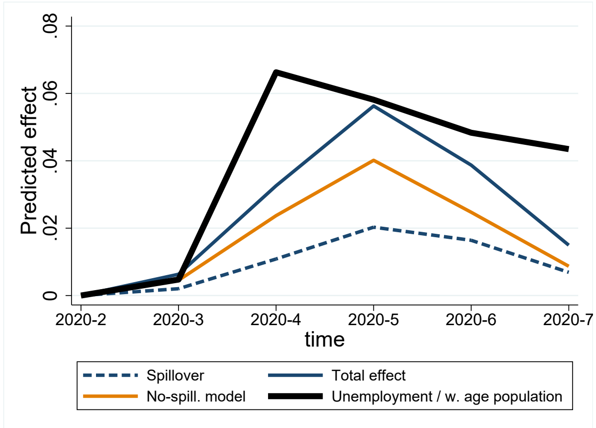
Note: Standard errors in parentheses. Errors clustered by county. Significance levels: * = 0.05; ** = 0.01; *** = 0.001. This table presents estimates from a model regressing the unemployment to working population ratio on measures of the own county lockdown intensity and spillover intensity coming from neighbour's lockdown orders. In columns 4, 5 and 6 we instrument spillover intensity with the instruments outlined in section 5.3. The instruments exploit variation in neighbouring states' lockdown orders, which we argue are exogenous to unobservables that may also determine the own county outcomes. The lockdown intensity measure is proportional to the time spent under the corresponding lockdown order. The intensities are calculated in proportion to the share of sectors affected by lockdown orders in either the own county or the neighbouring counties (see section 4.1.2). The own lockdown intensity is proportional to the own-county commuting flow, whereas spillover measures are proportional to the commuting outflow between the observed county and its neighbours. All commuting flows are 2011-2015 averages. Controls include month and county x calendar month fixed effects, together with a third-degree polynomial of days since the first Covid-19 case was registered in the county. All specifications include a measure of the local market area's industrial exposure to the effects of Covid-19.

Figure 10: Effects of lockdown on unemployment/working-age population

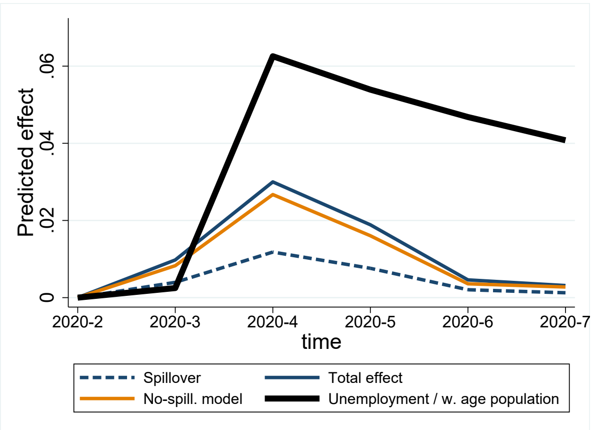
Note: "No lags" specification corresponds to Baseline in Table 16. "Lags" specification refers to a specification with two lags of each covariate of the "no lags" specification.



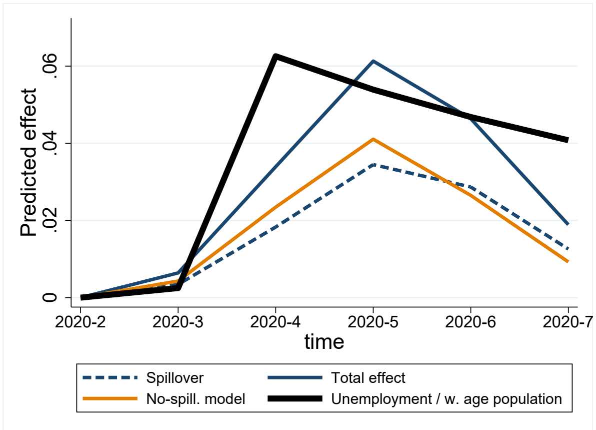
(a) No-lags, all counties



(b) Lags, all counties



(c) No-lags, High Commuting counties



(d) Lags, High Commuting counties