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Abstract

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In the introductory chapter of this dissertation, I motivate my use of disaggregate, cross-sectional variation for studying the effects of various policies on the aggregate economy. In particular, I argue that the topics empirically studied in this dissertation are historically unprecedented in nature and therefore necessitate a cross-sectional approach. I briefly review the anatomy of the linear least squares estimator to illustrate how standard cross-sectional designs can only recover the *relative* effects of policies. I then preview the theoretical tools I use in this dissertation to make the mapping between cross-sectional estimates and aggregate effects explicit.

In the second chapter of my dissertation, I generalize a textbook currency union model to incorporate trade in intermediates among labor markets in order to study the local, spillover, and aggregate effects of government spending. This theory provides a theoretical framework for interpreting the empirical, cross-sectional moments estimated in the subsequent chapter. The spillover effects of government spending mediated by trade in intermediates represents a novel and understudied mechanism by which local fiscal multiplier estimates likely represent a lower bound on the aggregate, Zero Lower Bound (ZLB) fiscal multiplier. In my framework, there is both a local and a spillover (relative) multiplier of government spending. Theoretically, summing both multipliers together yields an approximate lower bound on the aggregate, ZLB fiscal multiplier.

In the third chapter, I use geographic variation in government spending under the 2009 Recovery Act and import-export linkages between states from the 2007 Commodity Flow Survey to estimate a local relative multiplier of 1.46 and a spillover relative multiplier of 1.33. Adding both together yields an approximate lower bound on the aggregate, ZLB fiscal multiplier of 2.8, nearly doubling the lower bound implied by the local multiplier estimate alone. A sectoral decomposition of both estimated multipliers strongly corroborates the trade in intermediates spillover mechanism.

The final chapter studies the relative and aggregate economic effects of a prominent policy intervention implemented around the world during the COVID-19 pandemic by similarly interpreting causally identified, cross-sectional estimates through the lens of a currency union model. In particular, the high-frequency, decentralized implementation of Stay-at-Home orders in the U.S. is used to disentangle the labor market effects of SAH orders from the general economic disruption wrought by the COVID-19 pandemic. The analysis implies that each week of SAH exposure increased a state's weekly initial unemployment insurance (UI) claims by 1.9% of its employment level relative to other states. A back-of-the-envelope calculation implies that, of the 17 million UI claims between March 14 and April 4, only 4 million were attributable to SAH orders. A currency union model is developed to provide conditions for mapping this estimate to aggregate employment losses attributable to such orders.

To Adrienne and Pax,

You are my peace—my core of quiet within the storm.

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Except for the first two paragraphs and portions of the conclusion, the third chapter of my dissertation is a reprint (with permission) of "Unemployment Effects of Stay-At-Home Orders: Evidence from High Frequency Claims Data," coauthored with my officemates and dear friends ChaeWon Baek, Todd Messer, and Preston Mui — all of whom have granted permission for its inclusion here. This article has been accepted for publication at the *Review of Economics and Statistics*, which retains first publication credit, © 2020 by the President and Fellows of Harvard College and the Massachusetts Institute of Technology. All of us are grateful to Christina Brown, Bill Dupor, Yuriy Gorodnichenko, Christina Romer, Maxim Massenkoff, and Benjamin Schoefer, as well as the editor and two anonymous referees, for helpful comments and suggestions.

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Chapter 1

Introduction

This dissertation studies the economic effects of prominent policies implemented during the two largest economic downturns in the U.S. since the Great Depression. In Chapters 2 and 3, I study the employment and output effects of the government spending component of the American Recovery and Reinvestment Act of 2009. In Chapter 4, my coauthors and I study the disemployment effects of Stay-at-Home (SAH) orders implemented in the U.S. and around the world in the early months of the COVID-19 pandemic—a public health intervention deemed essential to stopping the spread of the novel coronavirus.

In each instance, the policy intervention under study was implemented in the midst of historically unprecedented conditions. The 2009 Recovery Act was passed during a severe economic contraction in which monetary policy was constrained by the Zero Lower Bound (ZLB)—features which make this episode singular in the U.S. context. Previous estimates of the aggregate effects of fiscal policy in the U.S. that rely upon time series variation, therefore, are not easily applied to understanding the effects of the government spending portion of the 2009 Recovery Act.

Similarly, the SAH orders implemented to combat the COVID-19 pandemic imposed severe restrictions on the economy not experienced in the U.S. at least since the wartime restrictions on the home front during WWII or the comparable public health interventions implemented during the 1918 Influenza Pandemic.[1] In terms of the aggregate time-series, this essentially amounts to just three observations for empirically studying the effects of such restrictions on economic activity.

The unprecedented nature of both the Great Recession and the COVID-19 pandemic necessitate turning to disaggregate, cross-sectional variation to study the effects of policies implemented during these episodes. This is exactly what I do in the empirical portions of this dissertation. In each of the baseline analyses, I use arguably exogenous state-level variation in policy treatment to estimate the economic effects of either government spending or SAH

[1] Brunet (2021) presents evidence that such wartime restrictions attenuate the aggregate effects of fiscal policy. Whether the restrictions imposed on the economy during the COVID-19 pandemic similarly limited the effects of government spending is left for future research. Correia, Luck, Verner, et al. (2020) argue that severe public health interventions imposed in the U.S. during the 1918 Influenza Pandemic that restricted economic activity in the short-run had positive effects on growth in the long-run.

orders.

Even if the causal effects of such policies are credibly identified, there is a conceptual wrinkle in that the cross-sectional analyses only identify a policy's *relative* effect rather than its *aggregate* effect. To have an intuition for why this is the case, it is helpful to dig into the anatomy of the linear least squares estimator, the bedrock of my empirical analysis. In the bi-variate case, the least squares estimator can be rewritten as a weighted sum of the ratio of *pairwise* differences in outcomes (y_i) and treatment intensity (g_i)[2]:

$$\begin{aligned}\hat{\beta}_{LS} &= \frac{\sum_i (y_i - \bar{y})(g_i - \bar{g})}{\sum_i (g_i - \bar{g})^2} \\ &= \frac{\sum_{i,j} (y_i - y_j)(g_i - g_j)}{\sum_{i,j} (g_i - g_j)^2} \\ &= \frac{\sum_{i,j} \frac{y_i - y_j}{g_i - g_j} (g_i - g_j)^2}{\sum_{i,j} (g_i - g_j)^2} \\ &= \sum_{i,j} \frac{y_i - y_j}{g_i - g_j} \times \omega_{i,j}\end{aligned}$$

Clearly then, any symmetric or aggregate effect of the policy intervention is differenced out in the estimation procedure.

But of course, understanding the aggregate effect of government spending and of SAH orders is the primary motivation for the analysis. I bridge this conceptual gap between the cross-sectional estimates and the implied aggregate effects of such policies by studying variants of workhorse currency union models from the literature.

In Chapter 2, I do this by generalizing a textbook currency union model to incorporate trade in intermediates among labor markets to study a novel and understudied mechanism by which local fiscal interventions have spillover effects. In this framework, the local and spillover (relative) multipliers of government spending together sum to a theoretical object that is argued by some in the literature to represent a rough lower bound on the aggregate, ZLB fiscal multiplier.

In the theoretical portion of Chapter 4, my coauthors and I study a benchmark, quantitative currency union model adapted to study the relative and aggregate employment effects of SAH orders. We model regional SAH orders in the model as either a one-time shock to firm-productivity (a local "supply shock") or a one-time shock to the household's discount factor (a local "demand shock"). We then consider a model under either sticky prices or fully flexible prices. In all four scenarios, we study the on-impact, relative employment effect of the shocks so as to be conceptually analogous to the high-frequency unemployment effects we estimate empirically. Through the lens of the model, only three of the four theoretical scenarios are consistent with our empirical estimates. Depending upon the nature of the

[2]See Gelman and Park (2009) for an exploration of how a linear regression can be approximated average differences between upper and lower quantiles of the sample. This algebraic identity makes an appearance there as well.

shock and its perceived persistence, our back-of-the-envelope calculation based on our empirical estimates either represents an upper or a lower bound on the aggregate employment losses attributable to SAH orders. Nevertheless, in any of the empirically relevant cases, the model implies that SAH orders contributed only a minority share of the overall increase in unemployment in the U.S. through April 3, 2020.

Chapter 2

A Theory of Fiscal Policy Spillovers Via Trade in Intermediates

2.1 Introduction

Policymakers frequently rely upon government spending and other forms of fiscal policy to stabilize economies in distress. In such settings, a key policy parameter is the size of the aggregate fiscal multiplier; however, given that recessions occur relatively infrequently, there is limited aggregate variation available for estimating this policy-relevant, aggregate multiplier directly. An alternative approach for studying the effects of fiscal policy is to rely upon subnational, regional variation in government spending. This, however, introduces a potential inconsistency since the so-called local fiscal multiplier identified using geographic variation is conceptually distinct from—albeit related to—the aggregate fiscal multiplier.

In this chapter, I argue theoretically (and in the next chapter, empirically) for the economic significance of trade in intermediate goods used in final production—an understudied mechanism in the literature—for mapping the local multiplier to the aggregate multiplier.[1] To argue this point formally, I generalize a textbook currency union model to incorporate trade in intermediates among labor markets.

In my model, because of roundabout production between states, local government spending has both a local component and a spillover component. This, in turn, implies two distinct, state-level, output multipliers: (i) The cumulative multiplier on home-state output of home-state government spending, which I refer to as the local (relative) multiplier and (ii) The cumulative multiplier on home-state output of government spending in the region

[1] In reviewing the local multiplier literature, Chodorow-Reich (2019) emphasizes four other mechanisms by which the aggregate closed economy multiplier may differ from the local multiplier: (i) the response of monetary policy, (ii) relative price changes and expenditure switching across regions, (iii) income and wealth effects, and (iv) factor mobility. Chodorow-Reich (2019) argues that, on balance, the externally financed, local multiplier provides an approximate lower bound on the closed economy, zero-lower bound, aggregate multiplier.

in which the home-state is located, which I refer to as the spillover (relative) multiplier.[2] Summing both multipliers together yields an approximate lower bound on the aggregate, fiscal multiplier when monetary policy is constrained by the Zero Lower Bound (ZLB).[3]

A simple example helps to illustrate how local fiscal multpliers depend upon the trade in intermediates among local labor markets. Consider an economy comprised of N local labor markets, each of equal size. Local production uses labor and a bundle of intermediates sourced symmetrically from each N local labor markets with cost share θ_N, which is in turn dependent on N ($\theta_N \to 1$ as $N \to \infty$, so that there is essentially zero local payments to labor). With rigid prices, zero profits and a mechanical marginal propensity to consume (mpc) in terms of local production, one dollar of local government spending increases local income by $\frac{1-\theta_N}{1-\frac{1}{N}\theta_N^2}\frac{1}{1-mpc}$. Thus, as the size of the local economy becomes small relative to entire economy ($N \to \infty$)—and the importance of intermediates sourced from the rest of the economy becomes large—the observed local relative multiplier tends towards zero even though the total government spending multiplier is constant at $\frac{1}{1-mpc}$.[4]

This is the basic force by which the local multiplier, all else equal, tends to understate the total effect of government spending when there is trade in intermediates among labor markets. In this example, relative prices are held fixed. This serves to illustrate that trade in intermediates among labor markets is conceptually *distinct* from other trade-related sources of spillovers—as mediated by relative price adjustments—previously emphasized in the literature (e.g. degree of home bias in consumption and expenditure switching motives).

The theory developed in the sequel provides an interpretative lens for cross-sectional (relative) multipliers estimated in the next chapter in terms of the implied aggregate, ZLB fiscal multiplier. For ease of exposition, I defer a broader review of the literature until Chapter 3.

2.2 Open Economy Currency Union Model with Roundabout Production

The currency union model presented in this chapter generalizes the complete-markets model developed in Farhi and Werning (2016). Specifically, I introduce a notion of roundabout production in which states source intermediate goods produced by other states within a common area, which I refer to as a region.

[2]Note that these multipliers are, both empirically and theoretically, defined as *relative* multipliers since they represent effects on the home state relative to the currency union as a whole.

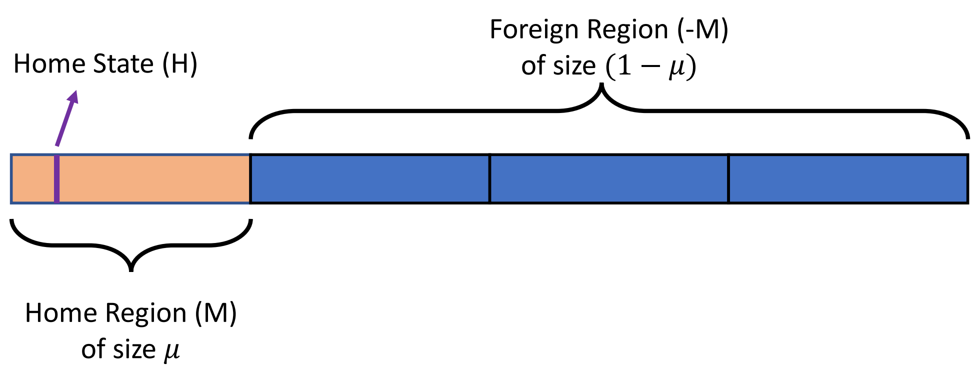
[3]This argument relies upon the equivalence of the region-aggregated system and the standard model without roundabout production. Regarding the approximate lower bound result, see arguments and discussion in Farhi and Werning (2016) and Chodorow-Reich (2019).

[4]For each dollar of local purchases either from the government or from private consumers, local income in this simple example increases by $(1-\theta) + \frac{\theta^2}{N}(1-\theta) + \frac{\theta^4}{N^2}(1-\theta) + \cdots = (1-\theta)/(1-\frac{\theta^2}{N})$.

Households

There is a continuum of states that comprise a currency union of measure one. Let $i \in [0,1]$ denote the typical state. The currency union is further partitioned into equally sized regions with measure $\mu > 0$. With slight abuse of notation, let $\mu(i)$ denote the region in which state i is located. As constructed, each state i is small relative to the region in which it is located and as such does not have an effect on region-wide outcomes. Figure 2.1 illustrates the conceptual distinction between states, regions, and the currency union as a whole.

Figure 2.1: Diagram of States, Regions, and Currency Union



Let H denote a particular state, which I will refer to as the home state. I will refer to the home region, denoted by $M \equiv \mu(H)$, as the region in which the home state is located. All foreign states in the home-region are treated symmetrically throughout the analysis. All other states/regions in the currency union are treated symmetrically. I collectively refer to the remaining foreign regions with $-M$. $-M$ has measure $1-\mu$.

Following Farhi and Werning (2016), I assume that the economy is initially in steady state and all uncertainty is resolved at time $t = 0$, at which point the future values of shocks are known to the agents in the economy. This allows for a characterization of the dynamics of the economy as deterministic functions of each of the forcing variables in the various systems of differential equations.

In each state, there is a representative household with preferences:

$$\int_0^\infty e^{-\rho t}\left[\frac{C_t^{1-\sigma}}{1-\sigma} + \zeta\frac{G_t^{1-\sigma}}{1-\sigma} - \frac{N_t^{1+\phi}}{1+\phi}\right]dt,$$

with N_t representing locally supplied labor, G_t representing government purchases of goods produced in the home state, and C_t representing the household's consumption index of home-state and imported goods:

$$C_t = \left((1-\alpha)^{\frac{1}{\eta}} C_{H,t}^{\frac{\eta-1}{\eta}} + \alpha^{\frac{1}{\eta}} C_{F,t}^{\frac{\eta-1}{\eta}}\right)^{\frac{\eta}{\eta-1}},$$

where $C_{H,t}$ is a CES aggregator of varieties produced in the home state, which I will henceforth refer to as home-produced goods except where there is likely to be some confusion:

$$C_{H,t} = \left(\int_0^1 C_{H,t}(j)^{\frac{\epsilon-1}{\epsilon}} dj \right)^{\frac{\epsilon}{\epsilon-1}} .$$

$j \in [0,1]$ refers to a particular good's variety produced in the home state. $C_{F,t}$ is a composite of imported goods from the rest of the currency union:

$$C_{F,t} = \left(\int_0^1 C_{i,t}^{\frac{\gamma-1}{\gamma}} di \right)^{\frac{\gamma}{\gamma-1}} ,$$

with $C_{i,t}$ as a CES aggregater of state i-produced goods, defined in an equivalent way to $C_{H,t}$. This implies that foreign-produced goods enter symmetrically into the home-state household's consumption. α determines the degree of home-bias in consumption.

When $\alpha \to 0$, all consumption is of goods produced within the home state. In the absence of externally sourced intermediate goods, this scenario would refer to a closed, regional economy with little to no trade with the rest of the currency union. With roundabout production, the home state may nevertheless trade considerably with the rest of the currency union even if, on the household side, all consumption is in terms of home-produced goods.[5] In that case, observed trade flows between states would entirely represent trade in intermediate goods. Because most within-state flows are over short-distances and cross-state flows are between manufacturers and wholesalers[6], a relatively low α may be the empirically relevant scenario; regardless, the model is analyzed under an arbitrary value of α.

The home-state household maximizes utility subject to the sequence of budget constraints for $t \geq 0$:

$$\dot{D}_t = r_t^* D_t - \int_0^1 P_{H,t}(j) C_{H,t}(j) dj - \int_0^1 \int_0^1 P_{i,t}(j) C_{i,t}(j) dj di + W_t N_t + \Pi_t + T_t,$$

with $P_{i,t}(j)$ being the price of state i variety j. W_t is the nominal wage, Π_t is nominal profits and T_t is a nominal lump sum transfer, all expressed in the common currency. Bond holdings are D_t and r_t^* is the union interest rate.

Government

Government consumption of home-produced goods is determined by a CES aggregator of home varieties:

$$G_t = \left(\int_0^1 G_t(j)^{\frac{\epsilon-1}{\epsilon}} \right)^{\frac{\epsilon}{\epsilon-1}}$$

[5]For example, consider a classic non-tradable industry: restaurants. Even though the consumption is highly local, restaurants often source ingredients from all over the country.

[6]See Hillberry and Hummels (2003).

which it sources at minimum cost given prices of each j variety: $P_{H,t}(j)$. The government finances its spending through lump-sum taxes on the home-state households. Because of Ricardian equivalence, the timing of taxation is irrelevant.[7]

In general, I will let G_t refer to government spending in the home state. I will let G_t^M refer to government spending in the representative (foreign) state in the home-region. G_t^{-M} refers, in turn, to spending in a representative state in the foreign regions.

Firms

Production with Region-Sourced Intermediates

Dropping i subscripts except where necessary, the typical firm in each state produces a differentiated good using a Cobb-Douglas production function:

$$Y_t(j) = A_{i,t} N_t(j)^{1-\theta} M_t(j)^{\theta},$$

where the intermediates used in production, $M_t(j)$ is a composite of goods imported from other states within the region:

$$M_t(j) = \left(\int_{\mu(i)} M_{s,t}(j)^{\frac{\gamma-1}{\gamma}} ds \right)^{\frac{\gamma}{\gamma-1}},$$

where s indexes states within the region. $M_{s,t}(j)$, in turn, is a composite of state s-produced varieties k:

$$M_{s,t}(j) = \left(\int_0^1 M_{s,t}(j,k)^{\frac{\epsilon-1}{\epsilon}} dk \right)^{\frac{\epsilon}{\epsilon-1}}.$$

In the analysis presented below, the only shocks I consider are home state and home-region government spending. As such, without loss of generality let $A_{i,t} = A_{H,t} = A$ for all $t \geq 0$. Additionally, following Farhi and Werning (2016), I introduce a constant input tax $(1+\tau)$, so that real marginal cost in terms of the home-state producer price index (PPI) is

$$MC_t = \left(\frac{P_t^M}{\theta} \right)^{\theta} \left(\frac{W_t}{1-\theta} \right)^{1-\theta} \frac{1+\tau}{A} \frac{1}{P_{H,t}} \propto \frac{W_t^{1-\theta} {P_t^M}^{\theta}}{P_{H,t}},$$

where P_t^M refers to the price index of home-region produced intermediates.[8]

[7]The assumption of complete markets implies that the distribution of taxation across states does not matter for implications of the model. In this sense, it does not matter if the government is local or federal. See Farhi and Werning (2016) for a discussion on this point.

[8]The employment tax is set to be $\tau = -\frac{1}{\epsilon}$, which offsets the monopoly distortion in steady state and simplifies the derivation without altering the main result.

Price-Setting

The Law of One Price holds so that the price of varieties in terms of the shared currency is identical regardless of where the variety is purchased.

Firms are subject to Calvo price-setting frictions. In particular, in each period a random flow of firms, ρ_δ, are allowed to change their price, they do to maximize discounted future profits. When $\rho_\delta \to \infty$, prices are fully flexible; when $\rho_\delta \to 0$, prices are rigid and no firm is able to update its price. Each firm's problem is to choose P_t^{reset} to solve the standard price-reset problem:

$$\max_{P_t^{reset}} \int_0^\infty e^{-\rho_\delta s - \int_0^s r^*_{t+z} dz} \left(P_t^{reset} \tilde{Y}_{t+s|t} - P_{H,t+s} MC_{t+s} \tilde{Y}_{t+s|t} \right) ds,$$

where $\tilde{Y}_{t+k|t} = \left(\frac{P_t^{reset}}{P_{H,t+k}}\right)^{-\epsilon} \tilde{Y}_{t+k}$. $\tilde{Y}_{t+k}$ refers to *total* demand for goods produced in the home state in period $t+k$, including the production of intermediate goods. Note that $P_{H,t+s}MC_{t+s}$ refers to nominal marginal costs. Firms take sequences of W_t, $\tilde{Y}_t$, and $P_{H,t}$ as given. The wage W_t adjusts flexibly each period.

Equilibrium Conditions

For a typical firm j in the home state, demand for intermediates and labor are related to one another by the firm's intratemporal first order condition:

$$M_t(j) = \frac{\theta}{1-\theta} \frac{W_t}{P_t^M} N_t(j)$$

We can thus write labor demand by firm j simply in terms of total gross production by firm j:

$$N_t(j) = \tilde{Y}_t(j) \frac{1}{A} \left(\frac{\theta}{1-\theta}\right)^{-\theta} \left(\frac{W_t}{P_t^M}\right)^{-\theta}$$

where again $\tilde{Y}_t(j)$ represents total gross production by firm j, which can further be written as

$$\tilde{Y}_t(j) = \left(\frac{P_t(j)}{P_{H,t}}\right)^{-\epsilon} \tilde{Y}_t$$

with $\tilde{Y}_t = \tilde{C}_t + G_t + \tilde{X}_t$. Total gross production in the home state and is comprised of demand for home-produced goods by union-wide consumers, the government, and firms in the home-region that use home-state goods as intermediates.

As is standard, we integrate over all home-state firms to get home-state labor demand:

$$N_t = \int_0^1 N_t(j) dj = \tilde{Y}_t \frac{1}{A} \left(\frac{W_t}{P_t^M}\right)^{-\theta} \Delta_t$$

where $\Delta_t = \left(\frac{\theta}{1-\theta}\right)^{-\theta} \int_0^1 \frac{P_t(j)^{-\epsilon}}{P_{H,t}} dj$ is the standard measure of price dispersion.

Demand for home-produced intermediates is determined by the terms of trade between the home state and the rest of the region in which it is located

$$\tilde{X}_t = \left(\frac{P_{H,t}}{P_t^M}\right)^{-\gamma} X_t^M$$

where X_t^M is total intermediates demanded by all firms within the home-region.

By assumption, except for the home state, all states within the home-region are identical, each of which have producer price index P_t^M. Following the same logic as in the derivation of labor demand for the home state, integrating over firms in the typical foreign state in the home region (and recalling that the home region has mass μ) yields

$$X_t^M = \mu \tilde{Y}_t^M \frac{1}{A}\left(\frac{W_t^M}{P_t^M}\right)^{1-\theta} \Delta_t^M$$

where previously undefined variables are analogous to those for the home state.

Under complete markets[9] and because households face the same sequence of interest rates, consumption in the home state is linked to consumption in the foreign states (in the home region and in a typical foreign region) by

$$C_t = C_t^M \left(\frac{P_{M,t}}{P_t}\right)^{\frac{1}{\sigma}} = C_t^{-M}\left(\frac{P_{-M,t}}{P_t}\right)^{\frac{1}{\sigma}} = \mu C_t^M \left(\frac{P_{M,t}}{P_t}\right)^{\frac{1}{\sigma}} + (1-\mu) C_t^{-M}\left(\frac{P_{-M,t}}{P_t}\right)^{\frac{1}{\sigma}},$$

where $P_{M,t}$ is the CPI of the typical state in the home region and $P_{-M,t}$ is the CPI of the typical state in the foreign region.

The Euler equation takes the usual form

$$\frac{\dot{C}_t}{C_t} = \frac{1}{\sigma}(r_t^* - \pi_t - \rho)$$

with $\pi_t = \dot{P}_t / P_t$ as CPI inflation.

State, Region, and Currency-Union Multipliers

Following Farhi and Werning (2016), I log-linearize the model around a symmetric steady state with zero trend inflation, denoting deviations in terms of (total) private consumption of home-produced goods, total consumption of home-produced goods, and public consumption

[9] Complete financial markets means that households in the currency union trade in financial securities prior to the resolution of risk at $t = 0$ so as to perfectly share risk. I assume complete markets for tractability. In general, weakening this assumption with incomplete markets—so that households only have access to a one-period bond—would require us to also keep track of the home state's net foreign asset position; however, under complete markets the path of NFA_t is pinned down by other variables in the system along with the initial condition that $C_0 = C_0^*$. See Farhi and Werning (2016) for a discussion of this more general setting.

of home-produced goods as

$$\tilde{c}_t = (1-\mathcal{G})(\log(Y_t - G_t) - \log(Y - G)) \approx \frac{Y_t - G_t - (Y - G)}{Y},$$

$$y_t = \log Y_t - \log Y \approx \frac{Y_t - Y}{Y}, \quad g_t = \mathcal{G}(\log G_t - \log G) \approx \frac{G_t - G}{Y},$$

where $Y_t = \tilde{C}_t + G_t$ represents final consumption of home-produced goods.[10] $\mathcal{G} = \frac{G}{Y}$ is the share of government spending in final consumption of home-produced goods in the typical state. Thus, up to a first-order approximation: $y_t = \tilde{c}_t + g_t$. In log-deviations, union-wide consumption is given by $c_t^* = \mu \tilde{c}_t^M + (1-\mu)\tilde{c}_t^{-M}$.

State Equilibrium System

In Appendix A.1, I show that the log-linearized equilibrium can be reduced to a system of differential equations characterizing the path of consumption of home-produced goods and home PPI:

$$\dot{\tilde{c}}_t = \hat{\sigma}^{-1}(r_t^* - \pi_{H,t} - \rho) - \alpha(\omega - 1)\dot{c}_t^* \tag{2.1}$$

$$\dot{\pi}_{H,t} = \rho\pi_{H,t} - \rho_\delta(\rho_\delta + \rho)(1-\theta)\left[\beta_{\tilde{c}}\tilde{c}_t + \beta_g g_t + \beta_{g^M} g_t^M + \beta_{c^*} c_t^* + \beta_{c^M}\tilde{c}_t^M\right], \tag{2.2}$$

and an initial condition (as a result of complete markets) that consumption of home-produced goods be equal (on impact) to consumption in the rest of the currency union: $\tilde{c}_0 = c_0^*$.

The coefficients in the Euler equation are related to primitives of the model according to $\omega = \sigma\gamma + (1-\alpha)(\sigma\eta - 1)$, $\hat{\sigma} = \frac{\sigma}{(1-\alpha+\alpha\omega)(1-\mathcal{G})}$ and $\tilde{\theta} = \frac{\epsilon-1}{\epsilon}\theta$; the coefficients $\{\beta_{\tilde{c}}, \beta_g, \beta_{g^M}, \beta_{c^*}, \beta_{c^M}\}$ in the New Keynesian Philips Curve (NKPC) are derived and defined in Appendix A.1.

Because the Euler equation follows from the assumption of complete markets and the specification of household preferences, the degree of trade in intermediates (θ) does not show up. Of course, θ does appear in the NKPC since roundabout production induces strategic complementarities in price setting behavior à la Basu (1995).[11]

Relative to the baseline model presented in Farhi and Werning (2016), the introduction of trade in intermediates between states within a region introduces two new *forcing variables* into the state-level system[12]: (i) government spending in the rest of the region in which the home-state is located (g_t^M) and (ii) the path of consumption of goods produced by the home-region ($\tilde{c}_t^M$).[13] That the path of the state economy depends upon government spending in

[10] Note: This is not value added in the home-state since a portion of value added of home-produced goods is attributed to labor income in states in the rest of the home-region.

[11] Note that trade in intermediates between states is *distinct* from the degree of home-bias in preferences. In particular, when $\sigma = \eta = \gamma = 1$, so that the Cole and Obstfeld (1991) holds, the system is *independent* of the degree of home-bias as parameterized by α but is *not* independent of the cost-share of intermediates θ.

[12] This dynamic system nests Farhi and Werning (2016) when $\theta = 0$.

[13] Recall that I assume that region-wide government spending is the same for all states except for the home-state, which is small relative to the region in which it's located. From the perspective of the home-state, region-wide variables are exogenously determined.

the rest of the region is unsurprising since firms in the rest of the region source intermediate goods from the home state to furnish goods purchased by the government. The latter force arises in general equilibrium because region-wide government spending itself alters the path of consumption of goods in the rest of the region.

In partial equilibrium, with the feedback from consumption of region-produced goods held constant, government spending in the home state and government spending in the home region enter the system with coefficients that are both proportional to and sum to $\phi \times \frac{(1-\tilde{\theta})}{(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi}$—the inverse of the Frisch elasticity scaled down a term that is strictly less than one. As will be seen in the next subsection, this term is important for determining the observational equivalence between the region-aggregated system and the model developed in Farhi and Werning (2016).

Region Equilibrium System

To aggregate the model to the region-wide system, recall that the home state is small relative to the region. Thus, when spending in the home state is equal to what is spent in all other states in the region (i.e. $g_t = g_t^M$), it has to be the case that consumption of home-produced goods is the same as consumption of region produced goods for the typical state within the region (i.e. $\tilde{c}_t = \tilde{c}_t^M$ for all $t \geq 0$.)

Thus, combining coefficients where appropriate yields the (unchanged) Euler equation and the region-wide NKPC:

$$\dot{\tilde{c}}_t^M = \hat{\sigma}^{-1}(r_t^* - \pi_t^M - \rho) - \alpha(\omega - 1)\dot{c}_t^* \tag{2.3}$$

$$\dot{\pi}_t^M = \rho\pi_t^M - \rho_\delta(\rho_\delta + \rho)\frac{(1-\tilde{\theta})(1-\theta)}{(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi}\left[(\hat{\sigma}+\phi)\,\tilde{c}_t^M + \phi g_t^M + \hat{\sigma}\alpha(\omega-1)c_t^*\right], \tag{2.4}$$

where I have also plugged in the coefficient definition for the loading on c_t^* in the NKPC to ease exposition. These two equations are observationally equivalent to equations (5) and (6) in Farhi and Werning (2016). The only difference is the introduction of trade in intermediates which flattens out the NKPC by the scaling down the price-rigidity term $\rho_\delta(\rho_\delta + \rho)$ by $\frac{(1-\tilde{\theta})(1-\theta)}{(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi} < 1$. Otherwise, the systems are the same.

Aggregate Equilibrium System

In the model, all foreign regions are treated symmetrically. Since the region dynamic system above is the same for the foreign regions after relabeling, to get union-wide outcomes we simply take a weighted average of the home region and foreign regions with weights μ and $1-\mu$. After rearranging and recalling that $\hat{\sigma}^{-1}/(1+\alpha(\omega-1)) = \frac{1-\mathcal{G}}{\sigma}$, we get:

$$\dot{c}_t^* = \hat{\sigma}_*^{-1}(r_t^* - \pi_t^* - \rho) \tag{2.5}$$

$$\pi_t^* = \rho\pi_t^* - \chi(\theta)\kappa(c_t^* + (1-\xi)g_t^*), \tag{2.6}$$

where $\lambda = \rho_\delta(\rho_\delta + \rho)$, $\kappa = \lambda(\hat{\sigma} + \phi)$, $\xi = \frac{\hat{\sigma}}{\hat{\sigma}+\phi}$, $\chi(\theta) \equiv \frac{(1-\tilde{\theta})(1-\theta)}{(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi}$, and $\hat{\sigma}_* \equiv \frac{\sigma}{1-\mathcal{G}}$. As with the region-wide system, at the currency level, the introduction of trade in intermediates between regions serves only to flatten out the NKPC by a factor $\frac{(1-\tilde{\theta})(1-\theta)}{(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi}$. As an implication, the ZLB fiscal multiplier for the currency union as a whole is lower with roundabout production, since θ serves to flatten out the aggregate NKPC.[14]

Home-Region Local and Spillover Multipliers

For the purposes of defining state-level relative multipliers I consider deviations of $\{g_t, g_t^M\}$ around a symmetric steady state with zero inflation in which $c_t^* = 0$ for all $t \geq 0$. This is the model analog to estimating relative multipliers (either direct or spillover) relying upon cross-sectional variation—the dependence upon c_t^* of state (region-wide) outcomes is absorbed by the time fixed effects.

At the state-level, I consider three separate experiments. In each experiment, I let $\mu \to 0$ so that the size of each region is small relative to the union. This simplifies the exposition by implying that union-wide consumption deviations are always zero. (i.e., $c_t^* = 0$ for all $t \geq 0$).

Experiment 1: State Spending Only In the first experiment, I set $g_t^M = 0$ for all $t \geq 0$, so that the only forcing variable in the home-state system is home-state government spending. Coupled with the initial condition $\tilde{c}_0 = c_0^* = 0$, the path of consumption of home-produced goods is given by

$$\tilde{c}_t = \int_{-t}^{\infty} \alpha_s^{d,t} g_{t+s} ds, \tag{2.7}$$

where, as an implication of Proposition 3 in Farhi and Werning (2016), $\alpha_s^{d,t} \leq 0$. The exact formula for each $\alpha_s^{d,t}$ is provided in Appendix A.2. In the model, the relative effect of home-state government spending on consumption of home-produced goods is negative. This force is what generates the analytic result, in this model, that the region-wide, cumulative output multiplier is a lower bound on the aggregate multiplier at the ZLB. The coefficients $\{\alpha_s^{d,t}\}$ themselves are determined by the eigenvalues of the system. These are defined formally in Appendix A.2.

Empirically, however, there is scant evidence that the relative effect on consumption is negative. Chodorow-Reich (2019) reviews the local multiplier literature and finds that most papers tend to find local (relative) multipliers greater than one, suggesting that local government spending shocks tend to have a positive, relative effect on consumption. In the context of the Recovery Act, Dupor, Karabarbounis, et al. (2018) estimate a positive elasticity of local consumption to a local spending shock.

Theoretically, there are a number of ways to generate a positive relative effect on consumption in a currency union model akin to the one developed here, including complemen-

[14]Proposition 2 in Farhi and Werning (2016) states the result formally that the aggregate fiscal multiplier at the ZLB is decreasing in price rigidity.

tarities between consumption and labor, introducing hand-to-mouth households, or through modifying the extensive margin decisions of firm hiring and entry.[15] In general, such channels tend to amplify the aggregate multiplier at the ZLB. Thus, the logic underlying the lower bound result in the baseline model tends to carry over to these more complicated extensions of the model.

Experiment 2: Region Spending Only In the second experiment, I suppose that there is no state-level government spending ($g_t = 0$ for all $t \geq 0$) but that there is government spending in the rest of the region in which the home-state is located.

To determine the spillover effects of g_t^M on consumption of home-produced goods, one must account not only for its effect through the production of intermediate goods needed to furnish the government with the goods it has demanded but also the effect upon home-state consumption mediated by the change in region-wide consumption. Formally, there are two forcing variables in the state-level system that depend upon the path of region-wide government spending: g_t^M and $\tilde{c}_t^M$.

Again, with the initial condition $\tilde{c}_0 = 0$ and because the model is linear, we can write:

$$\tilde{c}_t = \underbrace{\int_{-t}^{\infty} \alpha_s^{s,t} g_{t+s}^M ds}_{\text{Spillover from } g_t^M} + \underbrace{\int_{-t}^{\infty} \alpha_k^{c,t}}_{\text{Loading on } \tilde{c}_{t+k}^M} \underbrace{\int_{-(t+k)}^{\infty} \alpha_s^{M,t+k} g_{t+k+s}^M ds \, dk}_{\equiv \tilde{c}_{t+k}^M} \tag{2.8}$$

Also by Proposition 3 in Farhi and Werning (2016), the coefficients $\{\alpha_s^{s,t}, \alpha_s^{M,t}\}$ are all weakly negative. $\alpha_s^{s,t}$ represents the first channel of spillovers through the trade in intermediates: the production of intermediates by the home state to meet the government demand for goods produced by the rest of the region. The coefficients $\{\alpha_k^{c,t}\}$ determine the loading of consumption of home-produced goods on the path of consumption of home-region produced goods.

In the previous expression, I've replaced $\tilde{c}_{t+k}^M$ with the effect of region-level spending on region-level production of privately consumed goods, which is pinned down by the coefficients $\{\alpha_s^{M,t}\}$. Specifically, $\tilde{c}_t^M = \int_{-t}^{\infty} \alpha_s^{M,t} g_{t+s}^M ds$. The coefficients are defined in terms of primitives in Appendix A.2.

Experiment 3: Equal State and Region Spending The final experiment I consider is one in which government spending in the home state is the same for the typical state in the rest of the region. In particular, suppose that $g_t = g_t^M$ for all $t \geq 0$. Coupled again with the initial condition that $\tilde{c}_0 = 0$, the path of private consumption of home-produced goods is simply given by summing together the previous two expressions:

$$\tilde{c}_t = \int_{-t}^{\infty} (\alpha_s^{d,t} + \alpha_s^{s,t}) g_{t+s}^M ds + \int_{-t}^{\infty} \alpha_k^{c,t} \int_{-(t+k)}^{\infty} \alpha_s^{M,t+k} g_{t+k+s}^M ds dk = \int_{-t}^{\infty} \alpha_s^{M,t} g_{t+s}^M ds. \tag{2.9}$$

[15] See, e.g., Nakamura and Steinsson (2014), Farhi and Werning (2016), and Auerbach, Gorodnichenko, and Murphy (2019).

The latter equality follows from the fact that, by symmetry, $g_t = g_t^M \implies \tilde{c}_t = \tilde{c}_t^M$ for all $t \geq 0$.

Cumulative State Multipliers Define the home-state cumulative consumption multipliers for direct spending in the home state (local consumption multiplier) and spending in the rest of the home region (spillover consumption multiplier) as

$$\underbrace{\mathcal{M}_{c,g} \equiv \frac{\int_0^\infty \tilde{c}_s ds}{\int_0^\infty g_s ds}}_{\text{Local Consumption Multiplier}} \qquad \text{and} \qquad \underbrace{\mathcal{M}_{c,g^M} \equiv \frac{\int_0^\infty \tilde{c}_s ds}{\int_0^\infty g_s^M ds}}_{\text{Spillover Consumption Multiplier}} \tag{2.10}$$

The local consumption multiplier represents the (undiscounted) cumulative, relative multiplier of a particular path of government spending on private consumption of home-state produced goods. For a fixed total value of the denominator, the effect on consumption will vary as a function of the particular path of spending. This is clearly the case since the instantaneous multiplier coefficients $\{\alpha_s^{d,t}\}$ are functions both of both t and s.

Similarly, the spillover consumption multiplier represents the (undiscounted) cumulative, relative multiplier of a particular path of government spending in the region on private consumption of home-state produced goods. This cumulative multiplier will in general also be a function of the particular path of region-level spending.

The relation between the local and spillover consumption multipliers and cumulative consumption multipliers at the region level and for the currency union as a whole is discussed below. I also defer until then a discussion of the relationship between consumption multipliers, output multipliers, and value-added multipliers.[16]

The Region-Wide Open Economy Relative Multiplier

Region-Wide Relative Multiplier Consider an arbitrary sequence of government spending $\{g_t^M\}$ for $t \geq 0$ allocated equally to all states within the home region. Given the equivalence between the region-level dynamic system with trade in intermediates and equations (5) and (6) in Farhi and Werning (2016), the initial condition $\tilde{c}_0^M = 0$ implies that the path of consumption of region produced goods is given by:

$$\tilde{c}_t^M = \int_{-t}^{\infty} \alpha_s^{M,t} g_{t+s}^M ds, \tag{2.11}$$

where $\alpha_s^{M,t} \leq 0$. This expression is the same one that appears above in Equation (2.8).

The cumulative consumption multiplier is defined similarly to multipliers at the state level:

[16]Given the trade in intermediates between states, consumption (both public and private) of home-produced goods is not necessarily the same as home state value added.

$$\mathcal{M}_{c^M,g^M} \equiv \frac{\int_0^\infty \tilde{c}_s^M ds}{\int_0^\infty g_s^M ds} \tag{2.12}$$

This multiplier represents the (undiscounted) cumulative, relative multiplier of a particular path of government spending for the region as a whole on private consumption of region-produced goods.

Because all factors of production required for region-produced goods are contained within the region, the natural region-wide output multiplier is simply $1 + \mathcal{M}_{c^M,g^M}$. This multiplier represents the cumulative output multiplier of one dollar of government spending relative to other regions contained within the currency union.

When the path of government spending in the home state is proportional to government spending in the rest of the region for all $t \geq 0$, the local and spillover consumption multipliers sum to the region-wide cumulative consumption multiplier. This result is stated formally in the following proposition (proof in Appendix A.2):

Proposition 1. *If $g_t = \kappa_g g_t^M$ for all $t \geq 0$ and $|\kappa_g| > 0$, then*

$$\mathcal{M}_{c,g} + \mathcal{M}_{c,g^M} = \mathcal{M}_{c^M,g^M}$$

One implication of Proposition 1 is that whenever home-state and region-wide spending are $AR(1)$ with a common decay factor, the sum of the state-level local and spillover cumulative consumption multipliers will be equal to the region-wide cumulative consumption multiplier. Empirically, this condition appears to hold approximately for the Recovery Act spending series, as portrayed in Figure 3.3.

In Chapter 3, I estimate output multipliers rather than multipliers on private consumption of home-produced goods. As a result of Proposition 1, it is nevertheless straightforward to show that the local and spillover *output* multipliers (based on local value-added) together sum to the region-wide output multiplier.[17]

2.3 Conclusion

This chapter proposes a novel and economically important mechanism—trade in intermediate goods among labor markets—by which local government spending shocks propagate through the economy and ultimately determine the local, spillover, and aggregate effects of fiscal policy. To illustrate this mechanism formally, I generalize a benchmark currency union model to incorporate roundabout production among local labor markets within the currency union. In this setting, there is a local relative multiplier and a spillover relative multiplier of government spending, the sum of which yields an approximate lower bound on the aggregate, ZLB multiplier.

While suggested by the theory developed above, the economic significance of this mechanism of trade in intermediates is an empirical question. Thus, in the following chapter, I

[17] I show this equivalence in terms of local value-added multipliers in Appendix A.3.

use geographic variation in government spending under the Recovery Act to recover both the local and spillover multipliers.[18]

[18]Because the literature already focuses considerable attention on estimating the local multiplier, I focus my discussion primarily on estimating the spillover multiplier.

Chapter 3

Empirical Evidence of Fiscal Policy Spillovers Via Trade in Intermediates

3.1 Introduction

Guided by the theory developed in the previous chapter, in this chapter I use state-level data to estimate the local and spillover multipliers arising from the American Recovery and Reinvestment Act (ARRA; Recovery Act) of 2009.[1] I follow the literature in estimating the local multiplier. To estimate the spillover multiplier, I use pre-recession trade-linkages from the 2007 Commodity Flow Survey (CFS) to calculate the extent to which each U.S. state was differentially exposed to Recovery Act spending elsewhere in the country.[2] I calculate spillover exposure for each state as the weighted sum of ARRA spending in the rest of the country, where bilateral weights are determined by import shares from the spillover-exposed state. I then use local projection methods to estimate the extent to which this exposure affected state-level economic outcomes such as output, employment, and unemployment. I have three key empirical findings.[3]

My first finding is an estimate of the spillover multiplier: all else equal, each additional \$1 of government spending in a given state increased output in the rest of the country by \$1.33 (SE: 0.16) over two years. As far as I know, this is the first paper to document cross-state output spillovers of fiscal policy arising from the Recovery Act and the first to emphasize the importance of trade in intermediate goods as the underlying mechanism of such spillovers.

Given that my estimate of the local multiplier is \$1.46 (SE: 0.43) over two years, the

[1] The Recovery Act had an estimated budgetary impact of \$830 b illion (see `https://www.cbo.gov/publication/45122`).

[2] The CFS is useful for thinking about this channel since, as reported in Hillberry and Hummels (2003), most shipments between states are between manufacturers and wholesalers.

[3] Identifying the spillover effects of the Recovery Act requires that policymakers did not select, intentionally or unintentionally, a *distribution* of spending in response to current or anticipated economic conditions among states indirectly exposed to such spending. Since a large portion of ARRA spending was allocated through pre-recession formulary rules, this seems to be a reasonable assumption. Nevertheless, I present evidence in support of this identifying assumption with an event-study style specification in Appendix B.2.

implied approximate lower bound on the aggregate multiplier from the Recovery Act was roughly 2.80 (SE: 0.48). That is, accounting for trade in intermediate inputs between U.S. states roughly *doubles* the implied lower bound on the aggregate, ZLB fiscal multiplier relative to relying upon the local multiplier estimate alone.

My second set of findings relate to the labor market. Again using local projection methods, I find that 6.7 (SE: 0.92) job-years were created/saved in the rest of the country over two years for every $1 million of Recovery Act spending. I extend this analysis to unemployment and find a quantitatively similar drop in unemployment relative to the rise in employment. My estimated spillover effects in the labor market are comparable to the direct effects previously estimated in the literature (see Chodorow-Reich (2019)). This provides further evidence that, at least in the case of the Recovery Act, local multipliers understate the aggregate effect of government spending.

Third, I decompose the spillover effects on output by broad industry grouping. I find that the composition of direct and spillover effects differ from one another in ways remarkably consistent with the spillover effects being mediated through the trade in intermediate goods between manufacturers and wholesalers. For example, the bulk of the spillover effect is concentrated in the manufacturing sector, with no discernible local effect on manufacturing.

Taken together, my results have clear policy implications, both in terms of the local effects and the aggregate effects of government spending. Most immediately, to the extent that the sum of the local and spillover multipliers represents a rough lower bound on the aggregate, liquidity-trap fiscal multiplier, then my estimates imply a near doubling of the corresponding aggregate multiplier. When monetary policy is constrained, fiscal policy appears to be effective in combatting economic downturns in the aggregate.

Second, the trade in intermediate goods between labor markets is a source of leakage by which local interventions may possibly fail to have predominantly local effects. If the goal of policymakers is to improve economic conditions in labor markets in distress, then spending in industries with a high share of value-added production from other labor markets may ultimately undermine that objective. Conversely, if the goal is to support a broad-based economic recovery, a sensible strategy may be to spend in sectors with a high share of intermediate inputs sourced from elsewhere in the country.

Related Literature

This and the previous chapter are most closely connected to the local fiscal multiplier literature, which uses plausibly exogenous geographic variation to study the effects of government spending on the economy. In the case of the Recovery Act of 2009, researchers have typically relied upon the institutional details for how aid was allocated to identify its local effects.[4] Extending this literature, I document that government spending under the Recovery Act had large, positive spillovers that extended well beyond the labor markets in which the spending

[4]See, for example, Chodorow-Reich et al. (2012), Wilson (2012), Dube et al. (2018), and Feyrer and Sacerdote (2012). Dupor and McCrory (2018) documents spillover effects from the Recovery Act between counties rather than between labor markets propagated by trade in intermediates. See Chodorow-Reich (2019) for a broader review of the literature, including non-ARRA papers.

took place.

A subset of this local multiplier literature uses cross-sectional variation in government spending and economic outcomes to further discipline general equilibrium models, which can then be used to study the aggregate fiscal multiplier. For example, Nakamura and Steinsson (2014) estimate the relative output multiplier on government spending, identified from differential state-level exposure to military build-ups and drawdowns. Dupor, Karabarbounis, et al. (2018) perform a similar exercise in the context of the Recovery Act, using instead the local county-level consumption multiplier to discipline their model. In both instances—and this is typical of this literature—in the empirical analysis, there is considerable trade in intermediate goods between geographic units. As I show in both theory and application below, it is important to account for trade in intermediates between regions when mapping estimates to the implied aggregate multiplier.[5]

While informative, credibly identified local fiscal multipliers differ conceptually from the policy-relevant, aggregate, closed economy multiplier. The second literature to which my results relate is that studying the aggregate effects of fiscal policy in a closed economy.[6] This literature has experienced a resurgence in the last decade in the wake of the global recession and the fact that many countries turned to fiscal policy in order to stimulate their weakened economies. Ramey (2019) reviews this literature and argues that the deficit-financed fiscal multiplier on output tends to be between 0.6 and 1.[7] While valuable for studying the typical response of the economy to government spending, this literature tends to lack sufficient statistical power to estimate the aggregate fiscal multiplier when monetary policy is constrained by the ZLB or when there excess capacity in the economy.

Yet, such circumstances are exactly when policymakers tend to be most interested in implementing countercyclical fiscal policy. My finding of large, positive fiscal policy spillovers between U.S. states implies that counteryclical fiscal policy is effective in stimulating the economy when monetary policy is constrained by the ZLB.

A third literature to which my results are related is work on the effects of fiscal policy in an open economy. (e.g. Auerbach and Gorodnichenko (2013), Ilzetzki, Mendoza, and Végh (2013), Gali and Monacelli (2008), Nakamura and Steinsson (2014) and Farhi and Werning (2016)). Empirically, Ilzetzki, Mendoza, and Végh (2013) show that the estimated effects of

[5]Dupor, Karabarbounis, et al. (2018) allow for trade in intermediates between regions but rely upon within-state, between-county variation to estimate the local consumption response. The model, calibrated to the estimated positive county-level consumption response, yields an aggregate consumption multiplier twice that of the county-level estimate—consistent with my finding of a large spillover multiplier. In contrast to their paper, I illustrate the importance of such roundabout spillovers between, rather than within, labor markets.

[6]See, for example, O. Blanchard and Perotti (2002), Hall (2009), Mountford and Uhlig (2009), and Ramey and Zubairy (2018).

[7]Of course, the multiplier is not a universal constant. It varies depending, for example, upon the composition of government spending (e.g. consumption versus infrastructure spending), how it is financed, the responsiveness of monetary policy, and the differential impact the spending has on households of varying levels of financial constraints. In general, frictionless dynamic general equilibrium models tend to predict output multipliers lower than 1 (See, for example, Baxter and King (1993)).

fiscal policy shocks are larger among closed economies than among open economies.[8] Wilson (2012), an early paper studying the effects of the Recovery Act, takes this as evidence that the estimated local multiplier may indeed be a lower bound on the aggregate multiplier, writing: “To the extent that subnational regions within the United States are more open than the national economy, this result suggests that the local multiplier estimated for these regions may indeed be a lower bound for the national multiplier” (p. 253). As far as I know, my paper is the first to present empirical evidence in support of this claim that local multipliers understate the aggregate multiplier.

Finally, my paper is related to the rapidly growing production network literature, which emphasizes the role that trade in intermediate goods has in propagating and amplifying idiosyncratic shocks.[9] This is relevant, since Hillberry and Hummels (2003) present evidence that the flows between states reported in the Commodity Flow Survey are predominantly between manufacturers and wholesalers. This trade in intermediate goods suggests parallels with the production network literature.[10]

3.2 Data

Commodity Flow Survey

To empirically investigate the spillover effects of fiscal policy that operate through trade linkages between states, I first construct a regional import/export matrix using data from the 2007 Commodity Flow Survey (CFS). This survey is taken every five years by the Census Bureau and the Bureau of Transportation Statistics to determine the characteristics of commodities shipped between regions within the United States.

For the purposes of this study, the CFS provides the dollar value of goods shipped between all pairs of states j and i in 2007 for the mining, manufacturing, wholesale, and selected retail and services trade industries. The CFS defines a shipment as the “single movement of goods, commodities, or products from an establishment to a single customer or to another establishment owned or operated by the same company as the originating establishment (e.g., a warehouse, distribution center, or retail or wholesale outlet).”[11] Thus, the reported values in the CFS correspond to the total value of final and intermediate goods shipped between states in 2007 for the subset of industries specified above. However, shipments between states

[8]Sheremirov and Spirovska (2019) similarly find multipliers are larger in relatively closed economies and smaller among more open economies.

[9]See, for example, Hulten (1978), Acemoglu, Carvalho, et al. (2012), Baqaee (2018), and Baqaee and Emmanuel Farhi (2019). Stumpner (2019) uses the CFS to study the geographic spread of the housing boom-bust cycle in the lead up to the Great Recession.

[10]For example, in a stylized production network model, Acemoglu, Akcigit, and Kerr (2016) show that, with limited relative price changes, the upstream propagation of demand shocks is larger than the downstream effects. Consistent with this prediction, I also show there are limited spillover effects from the recipient state to those states to which it ships goods.

[11]See https://www.census.gov/programs-surveys/cfs.html for more details about the CFS methodology and the specific implementation details.

are primarily between manufacturers and wholesalers, suggesting that these flows capture primarily the shipment of intermediate goods (see Hillberry and Hummels (2003)). Note that the CFS also includes shipments between establishments within each state.

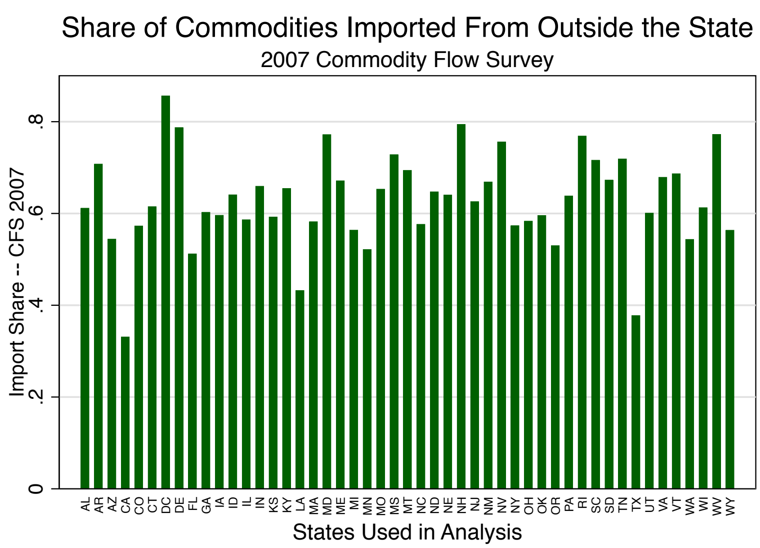
With these data I construct import shares for every pair of states i and j. Specifically, I calculate

$$w_{i,j} = \frac{imports_{j \leftarrow i}}{Inbound\text{-}Shipments_j}$$

where $w_{i,j}$ measures the share of commodities imported by state j from state i as a share of all commodities shipped to state j.[12] These import shares will be combined with data on government spending to construct a *spillover treatment* variable for each state. As a reminder, relative to the model, the states from which each state imports intermediate goods are viewed collectively as regions.

In the benchmark specification, I set $w_{i,i}$ to be equal to zero. I denote the full matrix of these weights by $\mathbf{W}$.

Figure 3.1: Share of Imported Goods from Outside the State



- This figure reports the share of shipments reported in the 2007 Commodity Flow Survey imported from the rest of the country.
- Each bar plot is the column sum of $\mathbf{W}$, which has typical element $w_{i,j} = \frac{imports_{j \leftarrow i}}{\sum_k imports_{j \leftarrow k}}$ and $w_{i,i} = 0$.

The column sums of $\mathbf{W}$ are equal to the proportion of inbound shipments of goods

[12] The commodity flow survey also reports commodities shipped between locations within the same state. I include these shipments in the denominator of $w_{i,j}$. Thus, $Inbound\text{-}Shipments_j \equiv \sum_k imports_{j \leftarrow k}$.

imported from outside the state. Letting $\bar{\omega}_j$ indicate the sum of the elements in the j^{th} column:

$$\bar{\omega}_j \equiv \frac{\sum_{k \neq j} imports_{j \leftarrow k}}{Inbound\text{-}Shipments_j}$$

The average value of $\bar{\omega}_j$ is 0.63, which means that on average states imported approximately 63% of the goods reported in the CFS 2007 from the rest of the country. California has the smallest value of 0.33, which implies that, as a share of all goods reported as being shipped to California in the CFS, only a third came from states other than California. On the opposite end of the spectrum, unsurprisingly, the largest is Washington D.C. with an import share of 0.86. Of the value of goods reported as being shipped to D.C., 86% come from the rest of the country.

Recovery Act Data

Data on the state-level spending component of the Recovery Act come from Wilson (2012). Every agency administering funds made available through the ARRA was required to provide a weekly detailed report, entitled the Financial and Activity Report, in which the value of obligations and payments to each state were specified. Under the ARRA, funds were made available to various Federal agencies. These agencies then determined—through discretion and formula—how much of such funds would be designated to each state. The bulk of such funds designated for each state were then announced as available to applicants.

When funds were obligated to a particular contractor or recipient—whether previously announced or unannounced—they were classified in the weekly Financial and Activity Reports as "obligations." For example, Wilson (2012) writes:

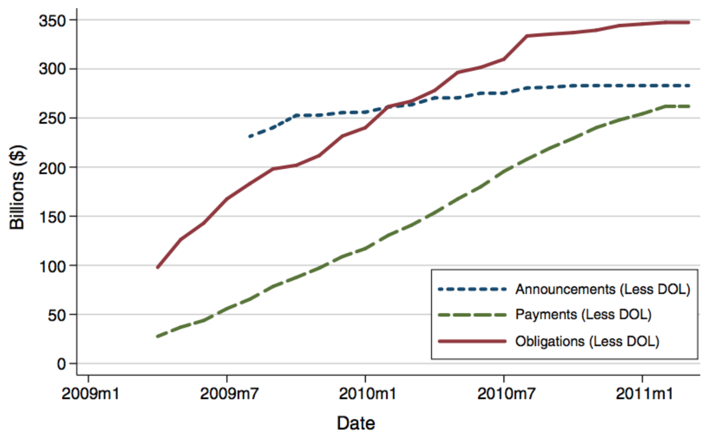
> The Department of Transportation (DOT) might award a contract to a construction firm or municipal agency at which point the DOT is said to have obligated those funds to that recipient. Finally, when recipients satisfy the terms of their contracts, the agency actually pays out the funds.

Payments, also reported in the weekly reports, correspond to when funds were actually transferred between the government and the recipient.

I use the state-level obligations series constructed by Wilson (2012). Reported in Figure 3.2 are three measures of ARRA spending over time, from April 2009 through March 2011.[13] As compiled by Wilson (2012), the spending component (i.e. obligations) of the Recovery Act totaled $418 billion in all fifty U.S. states and Washington D.C. Four agencies represent the majority of Recovery Act spending: Health and Human Services (27%), Department of

[13] This is Figure 2 in Wilson (2012).

Figure 3.2: ARRA Spending Measures over Time From Wilson (2012)



Education (22%), Department of Labor (15%), and Department of Transportation (10%).[14]

I use $ARRA^D_{i,t}$ to indicate the cumulative dollar value of Recovery Act obligations directly made to recipients in state i through quarter t. This variable is, by construction, set equal to zero prior to 2009Q2. Let $ARRA^D_t$ be the vector of obligations recorded for all states in quarter t.

Recovery Act Exposure Variable

I construct the extent to which state j was exposed to spending in all other states using the matrix of weights $\mathbf{W}$ and the vector of obligations $ARRA^D_t$:

$$ARRA^S_t = \mathbf{W} \times ARRA^D_t \tag{3.1}$$

where $ARRA^S_t$ records the cumulative dollar value of Recovery Act obligations each state was exposed to through quarter t. Specifically, each state's exposure is a weighted sum of spending elsewhere in the country:

$$ARRA^S_{i,t} = \mathbf{w}_i \cdot ARRA_t = \sum_{j \neq i} w_{i,j} ARRA_{j,t}$$

where $\mathbf{w}_i = (w_{i,1}, \ldots, w_{i,i-1}, 0, w_{i,i+1}, \ldots, w_{i,49})'$. I will often refer to this variable as a trade-

[14] Wilson (2012) excludes Department of Labor obligations since there is "virtually no source of exogenous variation to use as an instrument for [DOL funding]". The results presented below are robust to the exclusion or inclusion of this series in the construction of the cumulative value of obligations to which a state was exposed. Thus, for completeness, I include DOL obligations when I calculate how much each state was exposed to spending elsewhere in the country. Appendix Table B.3 reports results from using the cumulative obligation series net of DOL agency obligations.

weighted or import-weighted spillover ARRA.

There are 49 weights because I do not include Alaska or Hawaii in the benchmark analysis but I do include Washington DC. In what follows, references made to the collection of states used in the analysis refer to the 48 continental states plus DC. This vector would be equal to zeros if no state imported commodities from state i.

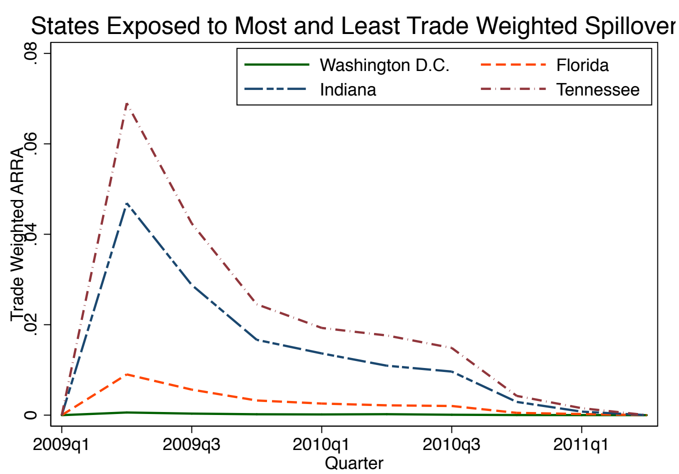
As explained below in Section 3.3, the variable of interest is

$$\frac{\Delta ARRA_{i,t}^{S}}{GSP_{i,t-1}} = \frac{ARRA_{i,t}^{S} - ARRA_{i,t-1}^{S}}{GSP_{i,t-1}},$$

which is the value of *additional* import-weighted obligations to which state i was exposed in quarter t relative to real Gross State Product (GSP) in the prior quarter.

Figure 3.3 plots the time series of this variable for the two states most exposed to trade-weighted obligations in 2009Q2 relative to output (Tennessee and Indiana) and the two states least exposed (Florida and Washington D.C.). In all cases, this trade-weighted spillover variable attains its maximum in 2009Q2, when the bulk of Recovery Act obligations were designated. Subsequently, the series all decline monotonically towards zero. Although all series exhibit similar patterns of dynamic exposure, it is clear that these states were differentially exposed to government spending that occurred in the rest of the country. It is this variation in exposure to spending elsewhere, geographic and temporal, that is used to estimate the spillover effects of fiscal policy.

Figure 3.3: Differences in the Path of $\frac{ARRA_{i,t}^{S} - ARRA_{i,t-1}^{S}}{GSP_{i,t-1}}$ for States with Highest and Lowest Values in 2009Q2



In the following section, I describe my empirical specification and present visual evidence that is consistent with my identifying assumption that the distribution of ARRA spending, coupled with the structure of trade flows between states, induced variation in exposure to spending elsewhere that was uncorrelated with contemporary or anticipated relative economic conditions in U.S. states. I then present my results, showing that there were large spillover effects of the Recovery Act mediated by trade linkages between states.

Other Data Sources

I consider three outcome variables: state-level output, employment, and unemployment. The quarterly real Gross State Product (GSP) series is from the Bureau of Economic Activity Regional Economic Accounts database.[15] Seasonally adjusted employment and unemployment data for each state were acquired from the Bureau of Labor Statistics.

3.3 Estimation and Results

Empirical Specification

The principal object I seek to estimate is the spillover multiplier of fiscal policy between U.S. states. Relative to the model presented in Section 2.2, each state's trading partners are analogous to regions in the model. Thus, I estimate how an additional dollar of government spending elsewhere in the country affects relative economic outcomes in each state.

To determine the spillover effect of fiscal policy upon an outcome variable Y, I estimate a series of Jordà (2005) local projections for horizons $h = 0, \ldots, 11$. The benchmark set of equations that I estimate on the panel data are of the following form:

$$\frac{Y_{i,t+h} - Y_{i,t-1}}{GSP_{i,t-1}} = \theta_{i,h} + \eta_{t,h} + \beta_h^Y \frac{\Delta ARRA_{i,t}^S}{GSP_{i,t-1}} + \alpha_h^Y \frac{\Delta ARRA_{i,t}^D}{GSP_{i,t-1}} + X_{i,t}\Gamma_h + \varepsilon_{i,h,t}, \tag{3.2}$$

where $GSP_{i,t}$ is the gross state product in state i in quarter t, $ARRA_{i,t}^D$ is the cumulative value of Recovery Act obligations to state i through quarter t, and $ARRA_{i,t}^S$ is the spillover treatment to which state i was exposed to in quarter t (see construction above), with Δ indicating the time difference of each variable. These equations include horizon-specific time fixed effects ($\eta_{h,t}$) and state fixed effects ($\theta_{i,h}$). $X_{i,t}$ is a vector of control variables. In the main analysis, the control variables comprising $X_{i,t}$ are four lags of $\frac{\Delta Y_{i,t}}{GSP_{i,t-1}}$, four lags of $\frac{\Delta ARRA_{i,t}^S}{GSP_{i,t-1}}$, and four lags of $\frac{\Delta ARRA_{i,t}^D}{GSP_{i,t-1}}$.[16]

The coefficients of interest are $\{\beta_h^Y\}_{h=0}^{11}$, each of which provides an estimate of the change in the outcome variable over h quarters in response to one-million dollars of import-weighted ARRA obligations elsewhere to which a state was exposed. I also report a cumulative exposure multiplier, which is scaled to incorporate the cumulative government spending

[15] The GSP series are in chained 2009 dollars and are seasonally adjusted by the BEA.

[16] This specification mirrors quite closely that of Auerbach and Gorodnichenko (2013). I have also estimated the model with regional fixed effects and varying the number of lag-lengths. Such changes have immaterial effects upon the estimated parameters.

shock. Specifically, the K-quarter cumulative exposure multiplier is given by:

$$\phi_K^{S,Y} \equiv \frac{\sum_{h=0}^{K-1} \beta_h^Y}{\sum_{h=0}^{K-1} \beta_h^{ARRA^S}}$$

where $\beta_h^{ARRA^S}$ is the cumulative impulse response of the spillover measure of K quarters, which I also estimate according to equation (3.2) with the cumulative spillover exposure as the dependent variable.

The interpretation of $\phi_K^{S,Y}$ is as follows: It is the cumulative effect on the outcome variable Y over K quarters for each dollar of Recovery Act aid a state was exposed to over the same K-quarter period. As discussed in Ramey and Zubairy (2018), one can succinctly estimate this statistic by estimating the model in a single step, replacing the left hand side of equation (3.2) with the accumulated change in the outcome variable of the relevant horizon and similarly replacing $\frac{\Delta ARRA_{i,t}^S}{GSP_{i,t-1}}$ with the cumulative increase in obligations over the same period.

Similarly, I will report the cumulative direct output multiplier over K quarters:

$$\phi_K^{D,Y} \equiv \frac{\sum_{h=0}^{K-1} \alpha_h^Y}{\sum_{h=0}^{K-1} \alpha_h^{ARRA^D}}$$

When presenting my results below, I directly estimate $\phi_K^{S,Y}$ and $\phi_K^{D,Y}$ by running the following specification:

$$\begin{aligned}\sum_{h=0}^{K-1} \frac{Y_{i,t+h} - Y_{i,t-1}}{GSP_{i,t-1}} =& \phi_K^{S,Y} \sum_{h=0}^{K-1} \left(\frac{\Delta ARRA_{i,t+h}^S}{GSP_{i,t-1}} \right) \mathbf{1}(t \geq 2009Q2) \\ &+ \phi_K^{D,Y} \sum_{h=0}^{K-1} \left(\frac{\Delta ARRA_{i,t+h}^D}{GSP_{i,t-1}} \right) \mathbf{1}(t \geq 2009Q2) \\ &+ X_{i,t}\Gamma_K + \epsilon_{i,K,t}\end{aligned} \tag{3.3}$$

where $\mathbf{1}(t \geq 2009Q2)$ is an indicator for whether the quarter is at or beyond 2009Q2 and $X_{i,t}$ is a vector of controls described in the previous equation. The purpose of specifying the model in this way is so that the cumulative exposure multiplier is identified solely from variation in output growth following the passage of the Act. Estimating the impulse response at all horizons jointly for both output and spillover ARRA exposure and combining estimates yields quantitatively similar results as estimating Equation (3.3) in a single step.

I estimate the model using data from 2006Q2 to 2015Q1. The benchmark tables report

[17] In my case, Driscoll and Kraay (1998) standard errors tend to be smaller relative to those constructed with heteroskedasticity consistent standard errors clustered by state. Table B.1 and Table B.2 report the counterparts to Tables 3.1 and 3.2 with the heteroskedasticity consistent standard errors clustered by state. Running a Pesaran (2004) cross-sectional dependence test on the residuals from Equation (3.2) strongly rejects the null hypothesis that the residuals are cross-sectionally uncorrelated. See Hoechle (2007) for more details on implementing this test.

Driscoll and Kraay (1998) standard errors, which allow for general forms of spatial and temporal dependence of the error terms $\varepsilon_{i,t,h}$.[17]

Summary statistics as of 2009Q1 of the variables used to estimate equations (3.2) and (3.3) are reported in Appendix Table B.9. Specifically, this table records the change and accumulated change in output, employment, unemployment, $ARRA^D$, and $ARRA^S$ over one and two years, scaled by lagged GSP.

Assessing the Identifying Assumption

Pre-Recession Growth of High and Low Spillover States

In estimating the direct effects of fiscal policy, one must overcome the omitted variable bias that arises because policymakers (typically) do not randomly assign treatment. More to the point, during a recession, the goal of countercyclical fiscal intervention is to stimulate economic activity and provide assistance to those local labor markets most severely affected by the downturn. Indeed, this was the stated purpose of the Recovery Act. To the extent that this endogenous allocation of Recovery Act aid occurred, then the estimates of $\{\alpha_h^Y\}_{h=0}^{11}$ from Equation (3.2)—the estimates of the direct effect of Fiscal Aid—will be biased downwards.

However, this study is concerned principally with the spillover effects of fiscal policy. As discussed Appendix B.3, there was only weak correlation between the initial severity of the downturn, prior to the passage of the Recovery Act, and the value of spillover aid to which a state was exposed. Similarly, there was limited correlation between the pre-Recovery Act severity of the recession and the centrality of a state in the network constructed from imports and exports between states.

Even if policymakers allocated funds according to the weakness of the local economy, it is unlikely that funds were allocated in order to affect the economic conditions of those states from which the recipient state imported goods.[18] For example, Colorado imports the bulk of its out-of-state commodities from California. Of all the commodities imported by Colorado, 7.5% originated in California. If the ultimate goal was to improve economic conditions in California, obligating funds to Colorado would presumably be an inefficient way to do so.[19]

Nevertheless, there may still be unobserved factors that introduce bias into the estimates of $\{\beta_h^Y\}_{h=0}^{11}$. As a further check on my identifying assumption, I look at the pre-treatment and post-treatment path of state GSP for states receiving high versus low spillover exposure. I view the results of this exercise as illustrative of both my identifying assumption and of the striking evidence of large spillover effects of the Recovery Act.

To construct relevant treatment and control groups, I first calculate the cumulative value

[18] Boone, Dube, and Kaplan (2014) provide evidence that the allocation of ARRA expenditure was generally uncorrelated with the severity of the economic downturn, strengthening this line of reasoning. Dube et al. (2018) also find that the amount of stimulus a county received was only weakly correlated with the downturn, as measured by the unemployment rate.

[19] The correlation between cumulative spillover exposure $\sum_{h=0}^{7} \Delta ARRA^S_{i,2009Q2+h}/GSP_{i,2009Q1}$ and direct aid $\sum_{h=0}^{7} \Delta ARRA^D_{i,2009Q2+h}/GSP_{i,2009Q1}$ over the two years following the passage of the Recovery Act is essentially 0. Even if direct aid were systematically correlated with local economic conditions, it appears unlikely the spillover exposure was.

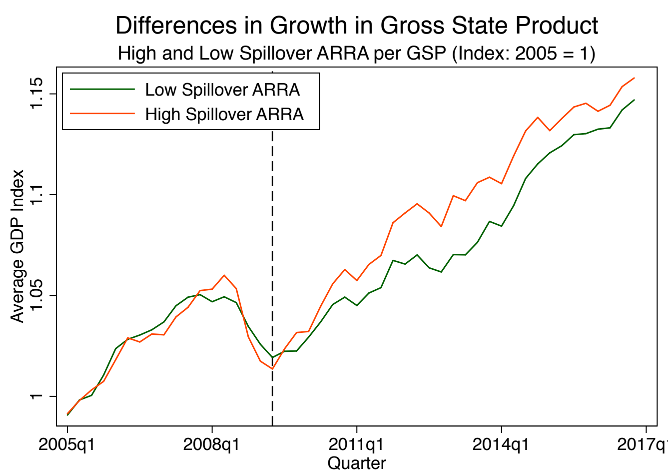
of import-weighted obligations to which each state was exposed relative to the state's pre-recession level of output, observed in 2005: $Z_i = \frac{ARRA^S_{i,2011Q2}}{GSP_{2005}}$.[20] The "control" group is designated as the set of states for which the accumulated import-weighted obligation series relative to state GSP was below the median:

$$\text{Control Group} = \{i \in States : Z_i \leq median(\{Z_i\}_{i=1}^{49})\}$$

The "treatment" group is the remaining set of states whose exposure to import-weighted obligations relative to GSP was above the median for the entire sample.

I then re-index the value of each state's level of output to be relative to the level of output in 2005Q1. For each of these groups I take the average value of this GSP index. The time-series of the average values of these indices are reported in Figure 3.4.

Figure 3.4: Differences in Gross State Product Growth since 2005Q1: High versus Low Values of Spillover ARRA Aid



- Low spillover states are those for which $Z_i \leq median(\{Z_i\}_{i=1}^{49})$, where $Z_i \equiv \frac{ARRA^S_{i,2011Q2}}{GSP_{2005}}$. High spillover states are the remaining states.
- Each line corresponds to the average within-group average of real GSP, after re-indexing each state's GSP to its level as of 2005Q1.

The reason for choosing 2005Q1 as the base quarter is to highlight two facts: First, in the two years prior to the passage of the Recovery Act the growth path of output in these two groups was comparable prior to and during the early stages of the recession; Second, both groups reached the nadir of output in 2009Q2—the quarter in which the effects of the Recovery Act likely began—but the subsequent growth in the treatment group was

[20] Recall that cumulative obligations are observed only through 2011Q2.

considerably faster than that in the control group. The common pre-trends in state-level output in the two years prior to the act is further evidence that the identifying assumption holds.[21]

The top panel of Figure 3.5 follows this line of reasoning a bit further by plotting the accumulated change in output between 2009Q2 and 2011Q2 against the accumulated value of ARRA spending in the rest of the country to which a state was exposed, relative to its lagged level of gross state product. Despite not conditioning on any set of controls, there is a clear upward sloping relationship between the value of import-weighted obligations to which a state was exposed and its output growth in the first two years of the recovery from the recession. In the second panel of Figure 3.5, I change the horizon over which output growth changes are accumulated: between 2007Q2 and 2009Q2. If anything, the states ultimately disproportionately exposed to spending elsewhere in the country experienced relatively lower output growth in the two years prior to the passage of the Recovery Act.

Effects on Output and Import Weighted Obligations

In this subsection I discuss the estimated impulse responses of output and the exposure series itself to a \$1 innovation to $ARRA^S_{i,t}$, the central empirical findings of my paper. The estimating equations are given by Equation (3.2), reprinted here for convenience:

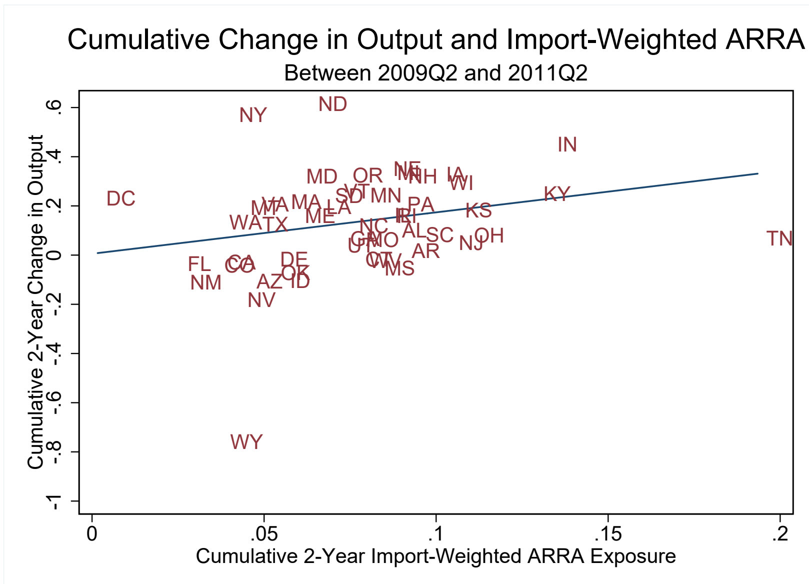
$$\frac{Y_{i,t+h} - Y_{i,t-1}}{GSP_{i,t-1}} = \theta_{i,h} + \eta_{t,h} + \beta^Y_h \frac{\Delta ARRA^S_{i,t}}{GSP_{i,t-1}} + \alpha^Y_h \frac{\Delta ARRA^D_{i,t}}{GSP_{i,t-1}} + X_{i,t,h}\Gamma_h + \varepsilon_{i,h,t}$$

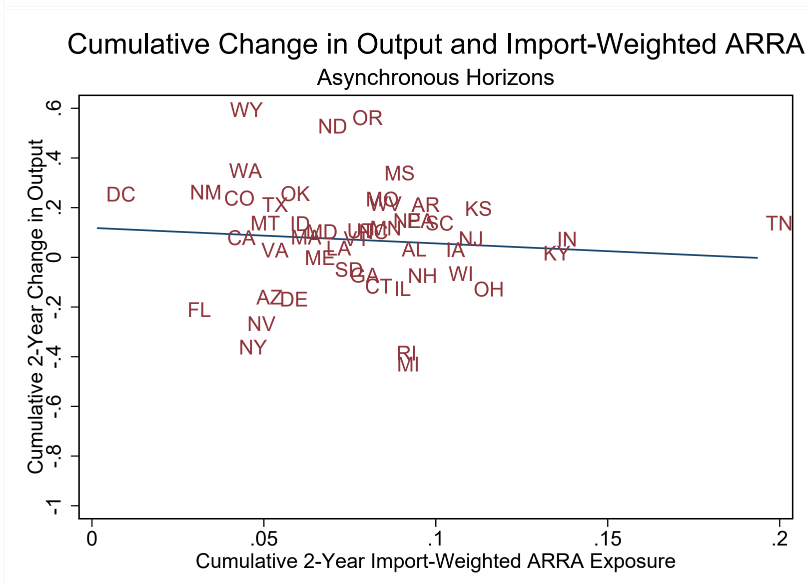
Figure 3.6a plots the estimated effect on output (GSP) of a \$1 innovation to import-weighted obligations over 12 quarters, including the impact quarter: $\{\hat{\beta}^{GSP}_h\}^{11}_{h=0}$. As seen in the figure, output increases on impact, rising by approximately 0.16 (SE: 0.05). Recall, that this has the interpretation that real output rose \$0.16 for every \$1 of import-weighted ARRA obligations to which a state was exposed. By quarter four, the estimates stabilize at close to 1, where they remain for the subsequent 8 quarters. Taking the integral of this impulse response over eight quarters ($h = 0, \ldots, 7$) yields the 2-year cumulative effect on output of a \$1 innovation to $ARRA^S_{i,t}$. This value is 5.68, which has the interpretation that the cumulative increase in output over two years was \$5.68 following a \$1 innovation to $ARRA^S_{i,t}$.

However, to properly scale this effect on output, we need to know the persistence of innovations to import-weighted obligations. Figure 3.3 suggests that import-weighted obligations, $ARRA^S_{i,t}$, have a strong auto-regressive component, even after controlling for other factors. Indeed, Figure 3.6b reports the impulse response of import-weighted obligations, $ARRA^S_{i,t}$ to a one dollar innovation to $ARRA^S_{i,t}$. Specifically, it plots the estimated coeffi-

[21] Consistent with evidence of local hysteresis in labor markets presented in Yagan (2019), this plot suggests that the spillover effects were extremely long-lived. Over longer horizons, one may suspect that factor reallocation of capital and labor may produce persistent relative differences in output (see O. J. Blanchard and Katz (1992)). Investigating whether the long-run relative differences in outcomes arising from spillovers are due to local employment hysteresis or factor reallocation is beyond the scope of this chapter.

Figure 3.5: Scatter Plots of Cumulative Output Change and Cumulative ARRA Exposure Over 2 Years Following Passage of Recovery Act





cients $\{\hat{\beta}_h^{ARRA^S}\}_{h=0}^{11}$ from Equation (3.2). By construction, this IRF is equal to 1 on impact. The IRF then exhibits a near geometric decay, declining to 0.56 (SE: 0.04) in the quarter following impact and to 0.30 (SE: 0.05) the quarter after. Eventually, the IRF of the import-weighted obligation series becomes statistically indistinguishable from zero after 5 quarters. The integral of this IRF through the fifth quarter following the innovation is 2.4.

Taken together, this implies that the 2-year cumulative effect on output of being exposed to one dollar of import-weighted ARRA obligations over the same 2-year window, ϕ_8^S, is approximately \$2.37 (= 5.68/2.4).

In Table 3.1 I report the estimates of $\hat{\phi}_8^S$ when estimating the model in a single step according to Equation (3.3). The benchmark specification corresponds to the column entitled "All Controls". This specification includes the following control variables: state and time fixed effects, four lags of $\frac{\Delta ARRA_{i,t}^S}{GSP_{i,t-1}}$, four lags of $\frac{ARRA_{i,t}^D}{GSP_{i,t-1}}$, and four lags of $\frac{\Delta GSP_{i,t}}{GSP_{i,t-1}}$. The point estimate is \$2.12 (SE: 0.25). Recall that this has the interpretation that output increased by \$2.12 over two years for each one dollar of ARRA obligations to which a state was exposed, over the same two year horizon.

In the first four columns of Table 3.1, I consider various restrictions to the benchmark specification. In the left-most column I report the most restrictive model, the bivariate regression estimate in which I exclude state fixed effects, time fixed effects and all other controls from the benchmark model. The point estimate is \$1.88 (SE: 0.75). In the second through the fourth columns, I sequentially introduce additional controls: state fixed effects, time fixed effects, the two-year ahead cumulative value of directly received aid, and lags. In all cases, the point estimates are quantitatively similar to the benchmark estimate of \$2.12.

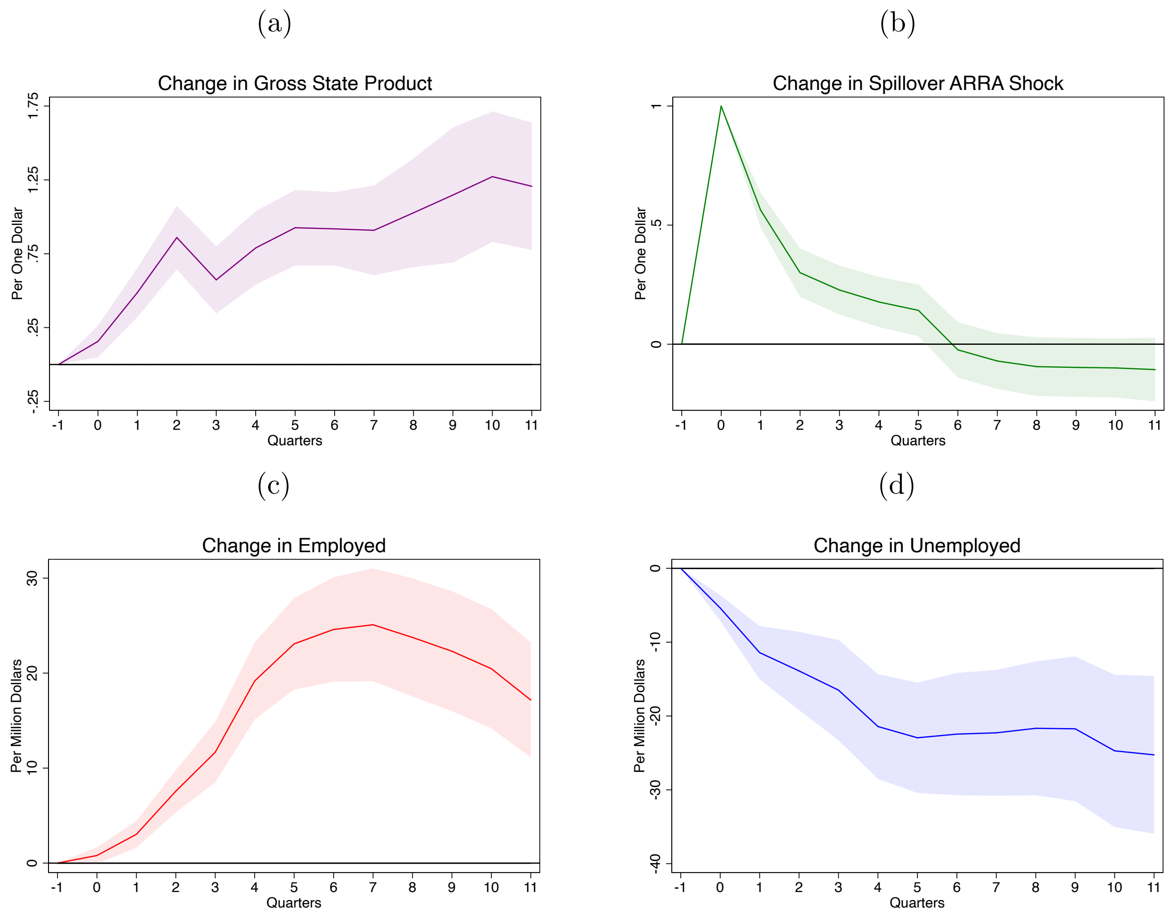
Table 3.1: Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Gross State Product: Varying Controls

	Bivariate	+ State FEs	+ Quarter FEs	+ Direct ARRA	All Controls
	b/se	b/se	b/se	b/se	b/se
8-Qtr Ahead	1.88**	2.03**	2.80***	2.82***	2.12***
Spill. ARRA	(0.75)	(0.82)	(0.38)	(0.40)	(0.25)
8-Qtr Ahead				2.30***	1.46***
ARRA				(0.52)	(0.43)
No. Obs.	1764	1764	1764	1764	1764
R-Squared	0.018	0.236	0.456	0.461	0.474
State FEs	No	Yes	Yes	Yes	Yes
Quarter FEs	No	No	Yes	Yes	Yes
Lagged Variable	No	No	No	No	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 8 quarters) is set to zero in quarters prior to 2009Q2.
- The controls in column (5) represent the benchmark specification.
- On average, each \$1 of directly received ARRA aid is associated with \$0.63 of import-weighted exposure. To convert to a spillover multiplier, multiply the coefficients in the top line by 0.63.

I convert this number to the appropriate spillover effect per \$1 of funding in the following way. First, I calculate the average import share across all states: $\bar{\omega} \equiv \mathbb{E}_i\left[\bar{\omega}_i\right]$, which is 0.63.

Figure 3.6: Impulse Response of Output, Employment, Unemployment, and Import-Weighted ARRA Obligations to Innovation to Import-Weighted ARRA Obligation



- Figures report 95% confidence intervals constructed from Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- Effects on GSP and $ARRA^S$ scaled to be the effect per \$1 of $ARRA^S$. Employment and unemployment figures scaled to be effect per \$1 million of $ARRA^S$.

This means that, in the context of my empirical specification in Equation (3.2), on average each \$1 of ARRA obligations is associated with \$0.63 of spillover obligations, distributed among other states in the country. For example, each additional \$1 of $ARRA^D_{i,t}$ corresponds to, on average, \$0.63 of $\sum_{j\neq i} ARRA^S_{j,t}$.[22]

Thus, we can calculate the 2-year cumulative effect on other states from one dollar of direct aid by multiplying the coefficient $\hat{\phi}^S_8$ by 0.63, yielding \$1.33 (SE: 0.16). All else equal, for each \$1 of Recovery Act aid allocated to a given state over two years, output increased

[22] Intuitively, the rescaling is necessary because the theoretical analog of the spillover exposure I construct is $\int_0^\infty \bar{\omega} g^M_s ds$, where spending in the rest of the region is scaled down by $\bar{\omega}$.

elsewhere in the country by an additional \$1.33.

Table 3.1 also reports the 2-year cumulative effect on output of directly allocated ARRA funding. Relative to the literature studying the Recovery Act, I do not instrument for this measure of local fiscal aid because I am primarily interested in estimating the spillover effects of the Recovery Act. Nevertheless, my estimate of the 2-year cumulative output multiplier of directly received ARRA obligations, \$1.46 (SE: 0.43), is consistent with the existing literature: Chodorow-Reich (2019) using only cross-sectional variation in government spending under the Recovery Act and a battery of instruments, estimates a 2-year multiplier of 1.53 (SE: 1.19). The trade-mediate spillover effects of fiscal policy that I estimate is quantitatively significant, representing approximately 90% of the estimated local effect.

Combining both the local multiplier and the spillover multiplier estimates, the implied region-wide relative multiplier from Recovery Act spending was approximately 2.80 (SE: 0.48). Absent other forces, this would suggest an implied, rough lower bound on the aggregate multiplier of the Recovery Act of approximately 2.80, since monetary policy was constrained by the ZLB.

Effects on the Labor Market

To what extent were the spillover effects of fiscal policy, identified in the previous section, also manifested in the labor market? To answer this question, I investigate the spillover effects of the Recovery Act aid on employment and unemployment. The estimated effects on employment yield a measure of the extensive margin spillover effect of fiscal policy, as compared to the intensive margin effect upon hours worked by already-employed workers.[23]

Complementing the results for employment, I also estimate the spillover effect on the number of people unemployed. These unemployment effects should be of comparable magnitude and opposite sign if the increase in employment is primarily due to people moving from unemployment to employment, as opposed to moving from non-participation in the labor force directly to employment.[24]

Consider first the effects of import-weighted ARRA obligations in all other states on a particular state's employment. Figure 3.6c plots the estimated parameters, $\{\hat{\beta}_h^{EMP}\}_0^{11}$, in an identical fashion to the output estimates. In these regressions, $ARRA_{i,t}^S$ is normalized to be per million dollars of obligations. In response to a million dollars of import-weighted government spending, the number of people employed in a particular state increases slowly at first, increases sharply by the end of the first year following the intervention, eventually attains a maximum value of 28 jobs in quarter 7, and then declines slightly.

As with the output estimates, we can calculate the integral of this figure to calculate the cumulative employment effect for every \$1 million innovation to import-weighted ARRA obligations. Over two years, the cumulative effect is 28.75 job-years created or saved. Di-

[23]Dupor and Mehkari (2016) present evidence that this intensive margin adjustment is quantitatively important.

[24]An alternative interpretation is that the higher employment is due to fewer job losses. In the counterfactual world of no spillover exposure and increased job losses, the previously employed workers would be moving primarily into unemployment.

viding through by 2.4, the cumulative value of trade weighted exposure over the same two years and multiplying by 0.63 yields the 2-year spillover employment multipler of approximately 7.5 job-years created or saved in all other states other than the state receiving the million dollars of fiscal stimulus. The implied cost per spillover job-year created is thus approximately \$133K.

The unemployment estimates, $\{\hat{\beta}_h^{UR}\}_{h=0}^{11}$, are reported in Figure 3.6d. Each coefficient represents the reduction in the number of unemployed persons at horzion h for every million dollars of ARRA aid to which the state was exposed. The dynamic spillover effect of fiscal aid exhibits a similar, though opposite, pattern to that upon employment. The decline in unemployment stabilizes after approximately five quarters. Integrating over two years and appropriately annualizing yields a 2-year cumulative reduction in unemployment by 34 job-years for every million dollars of aid to which a state was exposed. Multiplying this by 0.63 and dividing by 2.4 yields a reduction in unemployment in all other states by 8.9 job-years for every \$1 million of ARRA aid.

Taking Stock: Cumulative One & Two Year Trade Exposure Multipliers

In this section I summarize the findings of the previous two subsections by tabulating the cumulative one and two year trade exposure multipliers of Recovery Act aid. I do so by estimating equation (3.3) for $K = 4$ and $K = 8$ for output, employment, and unemployment. The coefficients for the employment and unemployment regressions have been scaled so as to represent the cumulative annualized effect of \$1 million of import-weighted ARRA exposure.[25] As before, I include four lags of the quarterly change of the outcome variable scaled by lagged GSP, four lags of the quarterly change in the ARRA exposure variable, state fixed effects, and time fixed effects.

Table 3.2 reports the results. Over one year, for each \$1 million of ARRA obligations to which a state was exposed: output increased by \$0.97 million (SE: 0.14), employment increased by 2.79 (SE: 0.53) job-years, and unemployment fell by 5.40 (SE: 0.98) job-years. Over two years, the effects are even more pronounced. Output increased by \$2.12 million (SE: 0.25) for every million of ARRA obligation exposure, with employment rising by 10.54 job-years (SE: 1.44) and unemployment falling by 12.61 (SE: 2.25) job-years.

These results suggest the aggregate effect (direct plus spillover) of the Recovery Act was large. Each \$1 of Recovery Act aid increased output by \$1.46 (SE: 0.43) in the recipient state and increased output elsewhere in the country by \$1.33 (SE: 0.16). Absent any other offsetting forces, the implied aggregate multiplier of the Recovery Act was 2.80 (SE: 0.48).

Each \$1 million of Recovery Act aid increased employment by 10.56 (SE: 1.87) job-years in the recipient state and increased employment by 6.63 (SE: 0.91) job-years elsewhere in the country. The combined employment effect was thus 17.20 (SE: 2.62) job-years per \$1 million of ARRA aid. The implied cost of creating a job lasting one year in the local state

[25] As above, annualizing the employment effects means dividing through by 4 since the model is estimated with quarterly data.

Table 3.2: Benchmark One and Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Gross State Product, Employment, and Unemployment

	4-Quarter Effect			8-Quarter Effect		
			Unemployed			Unemployed
	Output	Job-Years	-Years	Output	Job-Years	-Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead	0.97***	2.79***	-5.40***			
Spill. ARRA	(0.14)	(0.53)	(0.98)			
4-Qtr Ahead	0.27	3.53***	-2.35			
ARRA	(0.27)	(0.70)	(1.67)			
8-Qtr Ahead				2.12***	10.54***	-12.61***
Spill. ARRA				(0.25)	(1.44)	(2.25)
8-Qtr Ahead				1.46***	10.56***	-6.14**
ARRA				(0.43)	(1.87)	(2.53)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.417	0.722	0.799	0.474	0.698	0.823
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.
- On average, each $1 of directly received ARRA aid is associated with $0.63 of import-weighted exposure. To convert to a spillover multiplier, multiply the coefficients in the first and third lines by 0.63.

economy was $95K and $150K elsewhere in the country. The combined cost of creating a job anywhere in the country was $58K.

Decomposing Output Effects By Sector

In this subsection I decompose the cumulative direct and spillover output effects over one and two years by sector. Specifically, I estimate Equation (3.3) with K equal to 4 and 8 and change the left hand side variable to represent various broad sectors, indexed with s, of the economy:

$$
\begin{aligned}
\sum_{h=0}^{K-1} \frac{Y_{i,s,t+h} - Y_{i,s,t-1}}{GSP_{i,t-1}} =& \phi_{s,K}^{S,Y} \sum_{h=0}^{K-1} \left(\frac{\Delta ARRA_{i,t+h}^{S}}{GSP_{i,t-1}} \right) \mathbf{1}(t \geq 2009Q2) \\
&+ \phi_{s,K}^{D,Y} \sum_{h=0}^{K-1} \left(\Delta \frac{ARRA_{i,t+h}^{D}}{GSP_{i,t-1}} \right) \mathbf{1}(t \geq 2009Q2) \\
&+ X_{i,s,t}\Gamma + \epsilon_{i,s,t}
\end{aligned}
$$

As before, $\phi_{s,K}^{S,Y}$ has the interpretation of the cumulative K-quarter effect on the outcome variable $Y_{i,s,t}$ for each \$1 of ARRA aid to which the state was exposed over the same K quarters. $X_{i,t}$ includes four lags of the outcome variable and the exposure variable, as well as the cumulative value of ARRA aid received by the state over the same K quarters.[26] The analogue coefficient for directly received ARRA obligations, which I will refer to as $\phi_{s,K}^{D}$, has the interpretation of the cumulative effect over K quarters for each \$1 of ARRA aid a state directly received over the same K quarters.

Table 3.3 reports the estimated coefficients $\hat{\phi}_{s,K}^{S,Y}$ and $\hat{\phi}_{s,K}^{D,Y}$ for eight broad sectors of the economy: construction, non-durable manufacturing, durable manufacturing, retail trade, wholesale trade, transportation and warehousing, all other private sectors, and the government sector. Each panel of table records the effects by sector. The first row of the column reports the cumulative one year effect of being exposed to one additional dollar of Recovery Act spending in the rest of the country (column 1) and the one year effect of directly receiving one additional dollar of aid (column 2).

At neither the one nor the two year horizon is there a statistically significant effect on construction of spending elsewhere in the country; however, at the two year horizon, each \$1 of ARRA obligations led to an additional \$0.16 (SE: 0.06) of construction output in the local economy. Approximately 10% of the ARRA obligations in my sample were apportioned to states through the Department of Transportation, the bulk of which was designated for highway construction. It may thus be unsurprising that there is a direct effect on construction output but no indirect effect through trade linkages between states.

The differential effects by direct and indirect exposure to the Recovery Act, as detailed in Table 3.3, are consistent with the spillover effects being mediated by the trade in goods, particularly intermediate goods. For example, wholesale trade activity between wholesalers and retailers occurs primarily within state borders. As such, one would expect any effect on wholesale trade to be concentrated in the state receiving aid, with little or no spillover effect on wholesale trade elsewhere. This is exactly what I find. Over two years, roughly one tenth of the direct effect of government spending is through an increase in wholesale trade activity: each \$1 of directly received aid over two years led to increased wholesale trade production of \$0.19 (SE: 0.07). There is no discernible spillover effect on wholesale trade activity.

Perhaps more convincingly, the spillover exposure effect is principally concentrated in

[26]Recall that $\hat{\phi}_{s,K}^{S}$ should be rescaled by 0.63, since on average only \$0.63 of every dollar of aid is used to construct $ARRA_t^S$.

Table 3.3: One and Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Sectoral Output

	Cumulative Spillover ARRA b/se	Cumulative Direct ARRA b/se
Construction Effects		
Over One Year	0.03	0.02
	(0.02)	(0.02)
Over Two Years	0.03	0.16**
	(0.05)	(0.06)
Manufacturing Effects		
Over One Year	0.52***	-0.01
	(0.08)	(0.10)
Over Two Years	1.47***	0.11
	(0.16)	(0.19)
Retail Trade Effects		
Over One Year	0.02**	0.05**
	(0.01)	(0.01)
Over Two Years	0.06***	0.12***
	(0.01)	(0.02)
Wholesale Trade Effects		
Over One Year	-0.01	0.05*
	(0.02)	(0.02)
Over Two Years	-0.00	0.19**
	(0.03)	(0.07)
Transportation and Warehousing Effects		
Over One Year	0.05***	0.01
	(0.01)	(0.01)
Over Two Years	0.14***	0.04
	(0.02)	(0.05)
Private All Other Effects		
Over One Year	0.51***	0.22
	(0.05)	(0.17)
Over Two Years	0.82***	0.92***
	(0.09)	(0.25)
Government Effects		
Over One Year	-0.06*	-0.03
	(0.03)	(0.03)
Over Two Years	-0.16*	0.01
	(0.06)	(0.06)

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 8 quarters) is set to zero in quarters prior to 2009Q2.
- On average, each $1 of directly received ARRA aid is associated with $0.63 of import-weighted exposure. To convert to a spillover multiplier, multiply the coefficients in the first column by 0.63.

two sectors most associated with the production and shipment of intermediate goods: manufacturing and transportation/warehousing activity. Over two years, each additional $1 of aid to which a state was inderectly exposed led to an increase in manufacturing output of $1.47 (0.16). In contrast, there was no statistically discernible effect on local manufacturing of directly received aid. The same holds for the effect on transportation and warehousing, which only manifests itself in the spillover effect of government spending.

Robustness Exercises

In Section B.2 of the Appendix I show that my baseline estimates are robust to various concerns. First, I do an outlier analysis and show that no single state or pair of states are driving my results. Next, I address the concern that I have imposed the normalization that $w_{ii} = 0$. To do this, I include own-spending multiplied by self-import shares in the construction of my spillover exposure regressor. The point estimate on direct spending falls and the spillover exposure estimate rises. No longer needing to rescale the spillover exposure estimate, I find that the sum of the two coefficients is approximately 3, consistent with my baseline results.

In the third exercise, I address the concern that maybe states disproportionately exposed to spending elsewhere in the country recovered more rapidly simply because such states load more heavily on the aggregate business cycle and, in turn, the general recovery that began around the passage of the Recovery Act. My results are robust to explicitly controlling for state-level excess cyclicality.

The fourth exercise is a type of placebo test. I construct a new measure of spillover exposure by taking the transpose of $\mathbf{W}$ and assess whether there are additional spillovers propagating downstream from recipient states to states to which they tend to export. I find no evidence of downstream propagation of Recovery Act spending.

Fifth, I address the concern raised by Ramey (2019) that with heterogeneous treatment effects the unweighted regressions will tend not to yield estimates of the policy relevant closed economy multiplier. When weighting my results by state population, I find that that the direct effect on output rises to $2.50 and the indirect estimate falls to $1.29. Larger states source more intermediate goods internally within the state, so this result is unsurprising. Again, the implied aggregate multiplier from combining both the direct and indirect effect is in line with my baseline findings.

As a sixth robustness exercise, I assess whether my results differ when explicitly incorporating higher order linkages between states when determining how much a particular state was exposed to spending elsewhere in the country. Specifically, I use trade flows from the CFS to construct a Leontief Inverse weighting matrix that calculates for each state the total implied demand for local factors of production, such as labor, mediated by the trade in intermediate goods. The results of this exercise confirm the benchmark findings, suggesting that the first order linkages between states capture the bulk of indirect exposure.

Finally, to allay remaining concerns, I estimate an event study style specification to determine whether states disproportionately exposed to total spending elsewhere fared better or worse economically leading up to and following the passage of the Recovery Act, relative to states receiving less aid. This exercise formalizes the results already presented in Figures 3.4 and 3.5 (bottom panel). I find no evidence of a pre-trend in output growth among states more indirectly exposed to spending elsewhere in the country. Following the passage of the Recovery Act, there is a sharp effect on output growth, again consistent with the benchmark results.

3.4 Conclusion

In the context of the Great Recession, I empirically estimate quantitatively large spillover effects of the Recovery Act mediated through the trade in intermediate goods between U.S. states. Using the spending component of the Recovery Act of 2009, I construct a measure of how much each state was exposed to spending in other parts of the country. The regional and time-series variation in this exposure allows me to identify the spillover relative multiplier. My estimate of the local relative multiplier is in line with the extant literature and the spillover relative multiplier is of a comparable magnitude.

In my preferred specification, for every dollar of Recovery Act aid to a recipient state over two years there is a corresponding increase in output of \$1.33 elsewhere in the country. Coupled with the estimated direct effect of \$1.46 and the theoretical results developed in Chapter 2, this implies an approximate lower bound on the aggregate fiscal multiplier when monetary policy is constrained by the ZLB—as it was during the Great Recession—of around 2.8. Taken together, this evidence implies that the Recovery Act had large effects on aggregate output and, more generally, that fiscal policy is an effective policy tool for stabilizing economies in distress when monetary policy interventions have been exhausted.

Chapter 4

Unemployment Effects of Stay-At-Home Orders: Evidence from High Frequency Claims Data[†]

Joint with ChaeWon Baek, Todd Messer, and Preston Mui

4.1 Introduction

The previous two chapters study the local, spillover, and aggregate effects of government spending. The theoretical framework developed in the second chapter is used as an interpretive lens for understanding the cross-sectional (relative) multipliers estimated in the third chapter.

In this chapter, we use a similar methodological approach—coupling causally identified cross-sectional moments with a benchmark currency union model—to study the relative and aggregate economic effects of a prominent policy intervention implemented in the early months of the COVID-19 pandemic to stop the spread of the coronavirus. Our approach has many parallels with the previous two chapters of this dissertation as well as with the broader local fiscal multiplier literature.

To limit the spread and severity of the COVID-19 pandemic, officials around the globe turned to non-pharmaceutical interventions (NPIs), such as shutting down schools, restricting economic activities to those deemed essential, and requiring people to remain at home whenever possible. In mid-March 2020, Ferguson et al. (2020) issued a report projecting that, in the absence of the effective implementation of NPI mitigation strategies, more than 2 million Americans were potentially at risk of death from the COVID-19 respiratory disease, with many more facing uncertain medical complications in the near- and long-run.

[†]Except for the first two paragraphs and portions of the conclusion, this chapter is a reprint (with permission) of "Unemployment Effects of Stay-at-Home Orders: Evidence from High Frequency Claims Data," coauthored with ChaeWon Baek, Todd Messer, and Preston Mui. It has been accepted for publication in the *Review of Economics and Statistics*, which retains first publication credit,

Soon after, state and local officials in the United States began announcing Stay-at-Home (SAH) orders, which restricted residents from leaving their homes except for essential activities. The earliest SAH order was implemented in the Bay Area, California on March 16th, 2020. Three days later, the governor of California issued a state-wide SAH order. By March 24th, more than 50% of the U.S. population was under a SAH order (see Figure 4.1). By April 4th, 95% of the U.S. population was under a state or local SAH order, likely substantially reducing the supply of and demand for locally produced goods and services.

At the same time, there was mounting evidence of substantial disruption to labor markets in the United States. For the week ending March 21st, 2020, the Department of Labor (DOL) reported that more than 3.3 million individuals filed for unemployment benefits.[1] In the subsequent weeks ending March 28th and April 4th, initial claims for unemployment once again hit unprecedented highs of more than 6.9 million claims and 6.7 million claims, respectively. Taken together, total unemployment insurance (UI) claims over this three week period was almost 17 million.

How much of the initially observed increase in UI claims was attributable to the newly implemented SAH orders? This is not a straightforward question to answer since the increase in unemployment claims could plausibly be attributed to a multitude of factors other than SAH orders that occurred at the same time. For example, consumer and business sentiment both declined and economic uncertainty rose as the pandemic worsened. One stark example of this economic uncertainty was the swift drop in the value of the S&P 500 stock market index, which lost roughly 30% of its value between February 20 and March 16, the first day a SAH order was announced in the United States.

In this paper, we disentangle the local effects of SAH orders from the broader economic disruption brought on by the COVID-19 pandemic and other factors affecting all states equally. We do so by providing evidence of a direct causal link between the implementation of SAH orders and the observed increase in UI claims. To the best of our knowledge, this paper is the first systematic study of the causal link between SAH orders and UI claims in the United States. This is our main contribution.

We show that the decentralized implementation of SAH orders across the U.S. induced high-frequency regional variation as to when and to what degree local economies were subject to such orders. We leverage the cross-sectional variation in the length of time that states were exposed to such orders to estimate its effect on UI claims.[2,3]

We find that an additional week of exposure to SAH orders increased UI claims by approximately 1.9% of a state's employment level, relative to unexposed states. The effect

[1] For comparison, in this week one year prior, there were just over 200 thousand initial claims for unemployment insurance. This was also the first time since the DOL began issuing these reports that the flow into unemployment insurance exceeded the number of individuals with continuing claims.

[2] Our variable of interest pertains to the *government* implementation of SAH orders. Our design does not aim to capture the effects of, for example, social distancing behaviors that may have taken place in the absence of a government order.

[3] In this paper, we principally focus on UI claims for three reasons: (1) UI claims are among the highest frequency indicators of real economic activity—especially as it relates to the labor market; (2) These data are consistently reported at a subnational level; (3) The data are publicly and readily available.

is precisely estimated and robust to the inclusion of a battery of controls one might suspect are correlated with both local labor market disruption and SAH implementation, lending it a causal interpretation. The set of controls we consider include the severity of the local exposure to the coronavirus pandemic, state-level political economy factors, and each state's industry composition.

We use our cross-sectional estimate to calculate the implied aggregate effect of SAH orders on the number of new unemployment claims. This exercise yields an estimate of approximately 4 million UI claims attributable to SAH orders through April 4, comprising roughly 24% of total claims over the time period. We refer to this calculation as the relative-implied aggregate estimate of employment losses from SAH orders.

It is well known that cross-sectional research designs, such as the one employed in our paper, hold constant general equilibrium effects as well as other aggregate factors. Simply scaling up our cross-sectional estimate may therefore give a biased impression of the aggregate effect of SAH orders on UI claims in the United States.

To understand the nature of these general equilibrium forces, we present a simplified currency union model to provide conditions under which the relative-implied estimate represents an upper or lower bound on aggregate employment losses. When the SAH shock is viewed primarily as a technology shock—and in the empirically relevant case with flexible prices—our estimate represents an *upper bound* on the aggregate effect. However, when SAH orders are treated as a local demand shock, the interpretation is a bit more subtle and depends upon the persistence of the shock and degree of price flexibility. Across all combinations of price rigidity, persistence and nature of the SAH shock, we find that our back-of-the-envelope estimate, at most, understates aggregate employment losses by a factor of approximately two. With sticky prices and a zero-persistence shock, the relative-implied estimate associated with the SAH-induced local demand shock understates aggregate employment losses by 12%.

Taken together, the model results then imply a (non-binding) *upper bound* on UI claims from SAH orders through April 4, 2020 of approximately 8 million. Thus, relative to the total rise of around 16.5 million, at most around 50% of the total rise in UI claims over this period can be attributed to SAH orders.

Finally, we document the robustness of our empirical results by considering an alternative research design relying upon county-level data. Specifically, we estimate county-level specifications which allow us to control for unobserved state-level factors, such as each state's ability to respond to and process unprecedented numbers of unemployment claims. We find similar results in this case. Appendix C.1 documents the robustness of our headline result to alternative research designs and empirical specifications.

Related Literature

Our paper relates most obviously to the rapidly growing economic literature studying the COVID-19 pandemic, its economic implications, and the policies used to address the simultaneous public health and economic crises. The epidemiology literature has focused on

[4] The six countries are China, South Korea, Italy, Iran, France, and the United States.

the health effects of NPIs. In a notable study, Hsiang et al. (2020) estimate that, in six major countries, NPI interventions prevented or delayed over 62 million COVID-19 cases.[4] Our focus is, instead, on the macroeconomic effects of the coronavirus pandemic. Broadly speaking, the macroeconomic literature on COVID-19 has split into two distinct yet highly related strands. Here we provide a representative, albeit not exhaustive, review.

The first strand of research focuses on the relationship between macroeconomic activity, policy, and the unfolding pandemic. Gourinchas (2020) and Atkeson (2020) are early summaries of how the public health crisis and associated policy interventions interact with the economy. Both emphasize the trade-off between flattening the pandemic curve while steepening the recession curve. Similarly, Faria–e–Castro (2020) studies the effect of a pandemic-like event in a quantitative DSGE model in order to assess the economic damage associated with the pandemic along with the fiscal interventions employed in the U.S. to attempt to flatten the recession curve. Eichenbaum, Rebelo, and Trabandt (2020) derive an extension of the standard Susceptible-Infected-Recovered (SIR) epidemiological model to incorporate macroeconomic effects, formalizing the relationship between the flattening the pandemic curve and amplifying the recession curve. We view our paper as providing causally identified, empirical support for the claim that flattening the pandemic curve requires steepening the recession curve.

The second strand of research uses high-frequency data to understand the economic fallout wrought by the COVID-19 pandemic. Our paper aligns more closely with this strand of the literature. Baker et al. (2020) show that economic uncertainty measured by stock market volatility, newspaper-based economic uncertainty, and subjective uncertainty in business expectation surveys rose sharply as the pandemic worsened. Lewis, Mertens, and Stock (2020) derive a weekly national economic activity index and show that the COVID-19 outbreak had already had a substantial negative effect on the United States economy in the early weeks of the crisis. Hassan et al. (2020) use firm earnings calls to quantify the risks to firms as a result of the COVID-19 crisis. Coibion, Gorodnichenko, and M. Weber (2020a) examine how the pandemic affected the labor market in general. Using a repeated large-scale household survey, they show that by April 6th, 2020, 20 millions jobs were lost and the labor market participation rate had fallen sharply.

Our paper also relates to empirical work studying the effect of lockdown policies more specifically. For example, Hartl, Wälde, and E. Weber (2020) study the effect of lockdowns in Germany on the spread of the COVID-19. In contrast to these papers, we use geographic variation to understand the effect of COVID-19 on economic activity. In that respect, our paper can be thought of a high frequency version of Correia, Luck, Verner, et al. (2020), who find that over the long term, NPI policies implemented in response to the 1918 Influenza Pandemic ultimately resulted in faster growth during the recovery following the pandemic.

Other papers employing geographic variation in NPI implementation to understand their contribution to the economic fallout associated with COVID-19 pandemic include the following: Kong and Prinz (2020) use high-frequency Google search data as a proxy for UI claim activity to study the labor market effects of various NPIs; Coibion, Gorodnichenko, and M. Weber (2020b) study the effect of lockdowns on employment and macroeconomic expectations; L. B. Kahn, Lange, and Wiczer (2020) document broad declines job market openings in mid-March prior to implementation of SAH orders; Kudlyak and Wolcott (2020) provide evidence that the bulk of UI claims over this period were classified as temporary, suggesting that the long-run costs of lockdowns may be mitigated, so long as worker-firm matches persist until the recovery; and, Sauvagnat, Barrot, and Grassi (2020) document regional lockdowns depressed the market value of affected firms.

A closely related paper is Friedson et al. (2020), which uses the state-wide SAH order implementation in California along with high frequency data on confirmed COVID-19 cases and deaths to estimate the effect of this policy on flattening the pandemic curve. Unlike our approach, however, the authors in this paper use a synthetic control research design to identify the causal effects on this policy. The authors argue that the SAH order in California reduced the number of cases by 150K over three weeks; the authors perform a back-of-the-envelope calculation to calculate roughly 2-4 jobs lost over a three week period in California per case saved. In contrast to Friedson et al. (2020), we are able to directly estimate the causal effect of SAH orders on UI claims. Taking their benchmark number of cases saved over three weeks, we find that a SAH order implemented over three weeks in California would increase UI claims by 6.4 per case saved.

4.2 Data

State-Level Stay-at-Home Exposure

We construct a county-level dataset of SAH order implementation based on reporting by the *New York Times*. On March 24th, 2020, the *New York Times* began tracking all cities, counties, and states in the United States that had issued SAH orders and the dates that those orders became effective.[5]

We calculate the number of weeks that each county c in the U.S. had been under a SAH order between day $t-k$ and day t (and counting the day that the policy became effective).[6] We denote this variable with $SAH_{c,s,t,t-k}$, where s indicates the state in which the county is located. Except when explicitly stated, we drop the $t-k$ subscript and set k to be large enough so that this variable records the total number of weeks of SAH implementation in county c through time t.

As an example, consider Alameda County, California. Alameda County was among the first counties to be under a SAH order when one was issued on March 16th, 2020. Here,

[5]The most recent version of this page is available at `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html`. In a few instances, states implemented the closure of non-essential businesses prior to broader SAH orders that affected businesses and households alike. We show that our results are qualitatively and quantitatively robust to accounting for this occasional discrepancy in timing in Appendix C.1. We choose to rely upon the *New York Times* reporting since it provides sub-state variation. Over time, the *New York Times* stopped separately reporting sub-state orders when a state-wide SAH order was issued. We used the *Internet Archive* to verify the timing and location of SAH orders as reported in the *New York Times*.

[6]When a city implements a SAH order, we assign that date to all counties in which that city is located—unless of course the county had already issued a SAH order.

$SAH_{Alameda,CA,Mar.28} = 13/7$, as Alameda County had been under Stay-at-Home policies for thirteen days. Los Angeles County, California, on the other hand, did not issue a SAH order before the State of California did so. We therefore set $SAH_{LosAngeles,CA,Mar.28} = 10/7$ since the state-wide order was issued in California on March 19th, 2020.

The previous two examples illustrate how, in some instances, county officials took action before the state in which they were located did. Unfortunately, however, our main outcome of interest, new unemployment claims, is available to us only at the state-level.[7]

To aggregate county-level SAH orders to the state level, we construct a state-level measure of the duration of exposure to SAH orders by taking an employment-weighted average across counties in a given state. Formally, we calculate:

$$SAH_{s,t} \equiv \sum_{c \in s} \frac{Emp_{c,s}}{Emp_s} \times SAH_{c,s,t} \tag{4.1}$$

Employment for each county is the average level of employment in 2018 as reported by the BLS in the Quarterly Census of Employment and Wages (QCEW).[8] One can think of $SAH_{s,t}$ as the average number of weeks a worker in state s was subject to SAH orders by time t.

Figure 4.2 reports $SAH_{s,Apr.4}$ for each state in the U.S. and the District of Columbia. California had the highest exposure to SAH orders at 2.5, indicating that Californian workers were on average subject to SAH orders for two and a half weeks. Conversely, five states (Arkansas, Iowa, Nebraska, Northa Dakota, and South Dakota) had no counties under SAH orders by April 4. The average value across all states of $SAH_{s,Apr.4}$ is 1.2.

Main Outcome Variable: State Initial Claims for Unemployment Insurance

Our main outcome of interest is initial unemployment insurance claims. Initial UI claims is among the highest-frequency real economic activity indicators available. As discussed in the introduction, initial claims for unemployment insurance for the week ending March 21st, 2020 were unprecedented, with more than 3 million workers claiming benefits. By the end of that week, very few states or counties had issued SAH orders. Figure 4.1 shows that by March 21st, only around 20% of the U.S. population was under such directives. This suggests that a substantial portion of the initial economic disruption associated with the COVID-19 crisis may have occurred in the absence of SAH orders.

Let $UI_{s,t}$ indicate new unemployment insurance claims for state s at time t and UI_{s,t_0,t_1} denote cumulative unemployment claims for state s from time t_0 to t_1. In our baseline specification, we consider the effect of SAH orders on cumulative weekly unemployment

[7]While we lack sufficient data to estimate county-level effects on UI claims, in Section 4.6 we consider county-level regressions in which we estimate the March to April change in log employment and the unemployment rate using data published by the Bureau of Labor Statistics. We find quantitatively similar results even after conditioning on state-level fixed effects. In Appendix C.1 we use this county-level variation to study the impact of SAH orders on retail and workplace mobility, as measured by the Google mobility index.

[8]The annual averages by county in 2019 were, at the time of writing, not yet publicly available.

insurance claims by state from March 14th, 2020 to April 4th, 2020:

$$UI_{s,Mar.21,Apr.4} = UI_{s,Mar.21} + UI_{s,Mar.28} + UI_{s,Apr.4} \tag{4.2}$$

We then normalize this variable by employment for each state, as reported in the 2018 QCEW, to construct our outcome variable of interest:

$$\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} \tag{4.3}$$

Our choice of April 4th, 2020 as the end date for this regressions is driven by the observation that, by April 4th, 2020, approximately 95% of the U.S. population was under a SAH order. In Section 4.6, we consider 2-week and 4-week horizon specifications and find quantitatively similar results.

4.3 Empirical Specification

We now turn to our research design. Our main design is a state-level, cross-sectional regression:

$$\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s \tag{4.4}$$

where α is a constant, β_C is the coefficient on state-level exposure to SAH orders, X_s is a vector of controls with associated vector of coefficients Γ, and ϵ_s represents the error term in this equation.

To illustrate the motivation for our empirical design, in Figure 4.3 we compare the evolution of UI claims to state employment of "early adopters," defined as those states being in the top quartile of SAH exposure through April 4, 2020, to that of "late adopters," defined as those states being in the bottom quartile.[9] This figure provides *prima facie* graphical evidence of the main result of our paper: in the first few weeks, early adopters initially had a higher rise in unemployment claims relative to late adopters. By the week ending April 4th, 2020, the relative effect of adopting SAH orders early largely disappears, reflecting the fact that by this point approximately 95% of the U.S. population was under a SAH order, with most having been under the order for the full week ending April 4th.

This figure also suggests that SAH orders alone likely do not account for all of the rise in unemployment claims.[10] In the early weeks, late adopters also experienced historically unprecedented levels of UI claims even though early adopters had higher claims on average.

[9] The upper and lower edges of the boxes denote the interquartile range of each group, with the horizontal line denoting the median. As is standard, the "whiskers" denote the value representing 1.5 times the interquartile range boundaries.

[10] We thank an anonymous referee for pointing out that this could have the alternative interpretation that local SAH order implementation had substantial negative spillover effects on the rest of the country. See Section 4.5 for a model-driven discussion of such potential spillover effects between states.

For example, consider the week ending March 28. Here the difference between the median value of the two groups was approximately 1% of state employment; in that week, the median value of initial claims to employment for late adopters was roughly 3%, despite close to zero SAH exposure by this point. By April 4th, this difference almost completely disappears. Late adopters, who were under SAH orders for a much shorter period of time (or not at all, in some cases), converged to similar levels of unemployment claims relative to employment.

Confounding Factors

In order for our estimate $\hat{\beta}_C$ to have a causal interpretation, it must be the case that the timing of SAH orders implemented at the state and sub-state-level be orthogonal with unobserved factors affecting reported state-level UI claims.[11]

We provide further support for our causal interpretation by testing the magnitude and significance of the estimate $\hat{\beta}_C$ against the inclusion of three sets of important controls. The first set of controls considers the impact that the COVID-19 outbreak itself had on local labor markets. States that chose to implement SAH orders earlier may have done so simply because of the intensity, perceived or otherwise, of the local outbreak. In most macro-SIR models, a larger real outbreak would directly result in a larger drop in consumption due to a higher risk of contracting the virus associated with consumption activity (e.g. Eichenbaum, Rebelo, and Trabandt (2020)). To account for this concern, we control for the number of excess deaths, as reported by the Centers for Disease Control and Prevention (CDC), relative to population. We also include the share of the population over 60, as this demographic was more at risk of serious health complications arising from contracting COVID-19.

Additionally, one may be concerned that consumers' perceptions of the outbreak differed from its actual severity. During this time period, the reported number of new confirmed cases was an important statistic reported by the media. This statistic, which suffers from differential testing capability and definitions across states, differs from the measure of excess deaths as it focuses on how local labor markets may have interpreted the severity of the outbreak.[12] We therefore also include the total confirmed cases relative to population.[13]

[11] An additional reason for preferring April 4th is that over longer horizons, there is greater risk of omitted variable bias (i.e. $\mathbb{C}ov[\epsilon_s SAH_{s,Apr.4}] \neq 0$). A salient example is the rollout of the Paycheck Protection Program (PPP) on April 3rd. (The PPP was a central component of the CARES Act, a two trillion fiscal relief package signed into law on March 27, 2020. The PPP authorized $350 billion dollars in potentially forgivable SBA guaranteed loans.) This program provided forgivable loans to small businesses affected by the economic fallout of the pandemic, so long as those loans were used to retain workers. On the margin, PPP incentivizes firms to not lay off their workers, which would tend to lower UI claims for the week after April 4th. Depending upon how this interacts with the differential timing of SAH implementation, the bias could go in either direction.

[12] Evidence from Fetzer et al. (2020) suggests that the arrival of confirmed COVID-19 cases leads to a sharp rise in measures of economic anxiety, which would have an effect on real economic activity through the change in household and firm beliefs about the future state of the economy.

[13] We rely upon confirmed COVID-19 cases as compiled at the county-by-day frequency by USAFacts. USAFacts is a non-profit organization that compiles these data from publicly available sources, typically from daily reports issued by state and local officials. See `https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/` for more details.

Note that the severity of the outbreak would lead to an upward bias in our estimate $\hat{\beta}_C$ if states were more likely to enact SAH orders when the local outbreak was worse or perceived to have been worse, which may itself have led to labor market disruptions.[14]

The second set of controls we consider relates to the political economy of the state government. Some states may have had more generous social safety nets that led workers to separate from firms earlier than in states with less generous policies. Moreover, states with generous policies may also have been more likely to respond earlier to the pandemic, thereby generating bias. To account for this concern, we consider two political economy controls. First, we include the average UI replacement rate in 2019, as reported by the Department of Labor's Employment and Training Administration.[15] Second, we include the Republican vote share in the 2016 presidential election.[16] The first measure is designed to capture the generosity of the social safety net, while the latter is meant to capture political constraints on state and local officials to implement various public health NPIs.

Finally, our last set of controls is intended to address the concern that the timing of SAH implementation may be related to the sectoral composition within each state, and therefore the magnitude of job losses experienced by that state irrespective of SAH orders. To address this concern, we use a measure of predicted state-level UI claims as determined by industry composition within each state and the monthly change in jobs as reported in the national jobs report in March by the BLS. These numbers are based on a survey reference period that concluded on March 14th, 2020—fortuitously for us, two days before any SAH order was announced. Specifically we construct a Bartik-style control:

$$B_s = \sum_i \Delta \ln Emp_{i,March} \times \omega_{i,s} \tag{4.5}$$

where $\Delta \ln Emp_{i,March}$ is the monthly percentage change in employment in industry i (3-digit NAICs) for the month of March. $\omega_{i,s}$ is the share of employment in industry i in the state, as reported in the QCEW for 2018.

We also control for the extent of work-at-home capacity at the state-level. Dingel and Neiman (2020) construct an index denoting the share of jobs that can be done at home by cities, industries, and countries. We construct a state-level index by taking an state employment-weighted average of the Dingel and Neiman (2020) industry-level (2-digit NAICS) work-at-home index. It may be the case that states with a higher capacity to work from home may have been willing to implement SAH orders earlier if the labor market disruption of such policies was perceived to be lower when more workers are able to work from home. If this index is correlated with the number of initial UI claims received by the state in

[14]Our controls for excess deaths and confirmed cases are taken as cumulative sums as of the end of the sample period, which is April 4th in the benchmark analysis. We experimented with using lagged values of these measures as pre-period controls, and they had no effect on the magnitude or significance of our coefficient of interest. These results are available upon request.

[15]See `https://oui.doleta.gov/unemploy/ui_replacement_rates.asp` for more details.

[16]As reported by the *New York Times* at `https://www.nytimes.com/elections/2016/results/president`.

the absence of implementing SAH orders, then failing to include this control would introduce bias.[17]

Causal interpretations aside, the cross-sectional framework is nevertheless constrained in only answering the following question: By how much did UI claims increase in a state that implemented SAH orders *relative* to a state that did not? The constant term absorbs, for example, the general equilibrium effects of stay-at-home orders which would affect all states within the U.S.—not just those implementing SAH orders. To the extent that other states' labor markets were affected in any way by the local imposition of SAH orders, then $\hat{\beta}_C$ will fail to capture the *entire* effect of such policies. We postpone discussion of the mapping between the relative effect of SAH orders and their aggregate effect until after presenting our cross-sectional results.

4.4 Results

Effects of SAH Orders on State-Level UI Claims

In Table 4.1, we present results from estimating Equation (4.4). Column (1) shows the univariate specification, with no controls. The point estimate of approximately 1.9% (SE: 0.67%) implies that a one-week increase in exposure to SAH orders raises the number of claims as a share of state employment by 1.9% relative to states that did not implement SAH orders. Figure 4.4 displays this result graphically. The bubbles are shaded according to the intensity of the confirmed COVID-19 cases per thousand people and the size of the bubbles are proportional to state population.

In Column (2), we control for the number of confirmed COVID-19 cases per one thousand people, excess deaths by state, and the share of state population over the age of 60. As discussed, these are intended to control for factors related to the pandemic that might simultaneously affect both the timing of SAH implementation and the severity of state labor market disruptions. The change in the coefficient is immaterial—economically and statistically. In Column (3) we control for political economy factors: the state's UI replacement rate in 2019 and the 2016 Trump vote share. Our estimate $\hat{\beta}_C$ falls only slightly to 1.8%. In Column (4) we include controls for each state's sectoral composition (and in turn its sensitivity to both the pandemic-induced crisis and timing of SAH implementation). Our point estimate is again largely unchanged.

Finally, in column (5), we select a parsimonious specification that captures dimensions of each set of controls. We control for confirmed cases, excess deaths, the UI replacement rate, and the WAH index (the only significant variable). In this specification, which is our

[17] In unreported regressions, we study whether the effect of SAH orders differentially depends upon the value of the work-at-home index; we find no evidence that this is the case.

preferred specification, the estimate of β_C is still 1.9%.[18,19]

Our results support the idea that policies that work to flatten the pandemic curve also imply a steepening of the recession curve (Gourinchas 2020). To quantify this steepning of the recession curve, we use our point estimate of the relative effect on state-level UI claims of SAH orders to calculate a back-of-the-envelope estimate of the total implied number of UI claims between March 14 and April 4 attributable to SAH orders. We calculate the relative-implied estimate as follows:[20]

$$\text{Relative-Implied-Aggregate-Claims} = \sum_{s} \hat{\beta}_C \times SAH_{s,Apr.4} \times Emp_s \tag{4.6}$$

where s indexes a particular state. This is a back-of-the-envelope calculation as it simply scales up the cross-sectional coefficient $\hat{\beta}_C$ according to each state's SAH exposure through April 4, 2020 and each state's level of employment.

This back-of-the-envelope calculation yields an estimate of 4 million UI claims attributable to SAH orders through April 4. Ignoring cross-regional spillovers, this relative-implied estimate suggests that approximately 24% of total claims through April 4, 2020 were attributable to such orders.

This calculation does not incorporate general equilibrium effects or spillovers that may have arisen as a result of local SAH implementation. As we discuss in Section 4.5, when the SAH order is interpreted as a local productivity shock, this represents an upper bound on aggregate employment losses; when, however, the SAH implementation is treated as a local demand shock, the analysis is a bit subtler. Yet, even in this case, we find that at most the relative-implied aggregate multiplier understates true employment aggregate employment losses by a factor of 2. Through the lens of the model, this provides an upper bound on total employment losses attributable SAH orders: 8 million UI claims through April 4, or approximately half of the overall spike in claims during the initial weeks of the economic crisis induced by the COVID-19 pandemic.

An alternative back-of-the-envelope calculation to assess the magnitude of our estimate is to instead focus the relative contribution of SAH orders in terms of typical cross-sectional variation in UI claims in our sample. Our estimates imply that a state which implemented SAH orders one week earlier saw an increase in UI claims by 1.9% of its 2018 employment level relative to a state one week later, which is slightly less than 50% of the cross-sectional

[18] In the appendix, we consider three additional robustness exercises at the state-level. We alternate the horizon over which the model is estimated (2 and 4 weeks), estimate the model by weighted least squares, and re-estimate the model dropping one state at a time. The results are quantitatively and qualitatively similar.

[19] In unreported regressions, we find that, when including all regressors, $\hat{\beta}_C$ is somewhat attenuated—albeit statistically indistinguishable from our baseline estimate; however, this attenuation is largely driven by the parametric assumption of linearity on the share of votes for Trump in 2016, which places substantial leverage on Wyoming and West Virginia. Dropping these states from the full specification with all control variables yields a point estimate of 1.8% (SE: 0.75%). These regressions are available upon request.

[20] We use the terminology "relative-implied" because in the cross-section we are only able to identify effects of SAH orders relative to states not implementing SAH orders. We discuss this issue at greater length in Section 4.5.

standard deviation of employment-normalized claims between weeks ending March 21 and April 4.[21]

4.5 Aggregate Versus Relative Effects

Our empirical strategy relies on cross-sectional variation in the timing and location of SAH orders to identify the relative effect such policies had on labor markets during the initial weeks of the COVID-19 outbreak in the United States. In this section, we discuss in greater detail the sorts of spillovers that are likely to be relevant and the conditions under which the relative-implied aggregate estimate (see equation (4.6)) represents a lower or upper bound on the aggregate effects of SAH orders on UI claims. This is important for how one should interpret our back-of-the-envelope calculation that in the early period of the crisis, approximately only 24% of UI claims through April 4, 2020 were related to SAH orders.

To the extent that there are cross-regional (either positive or negative) spillovers of SAH orders, our estimate will not capture the *aggregate* effect of SAH orders. This limitation is related to the stable unit value (SUTVA) assumption in the causal inference literature, which requires that potential outcomes be independent of the treatment status of other observational units. Because of considerable trade between U.S. states, SUTVA is likely to be violated in our setting.[22]

To guide our discussion, we use a benchmark currency-union model to study the effects of SAH orders on the local economy, the rest of the currency union, and the entire economy as a whole. We present results for an economy characterized either by sticky prices or flexible prices, with SAH orders modeled as either a pure local demand shock or a pure local productivity/supply shock; the evidence from Appendix C.1 suggests that both channels were operative.[23] We then briefly summarize other important cross-regional spillovers not well-captured by the currency model we study. The most salient of these spillovers relate to the *informational* effect of early SAH implementation in some parts of the country.

Currency Union Model: Supply and Demand Shock Implications of SAH Orders

In this section, we consider the implications of local demand or supply shocks in a benchmark currency union model under either sticky or flexible prices. The model we consider is a simpler version of the baseline, separable utility, complete markets model presented in Nakamura and Steinsson (2014), modified to incorporate productivity shocks and discount

[21] We thank an anonymous referee for this particular recommendation.

[22] SUTVA violations are likely to be more salient in the cross-section when the model is estimated over longer horizons. This is, in part, why we choose as our baseline the 3-week horizon specification.

[23] Additionally, as is discussed in Brinca, Duarte, and Faria-e-Castro (2020), it is appropriate to view the COVID-19 pandemic (and associated policy responses) as some combination of demand and supply shocks. We consider pure demand and supply shocks to illustrate the economic implications of each in isolation.

rate shocks (to model negative local supply and demand shocks, respectively).[24] We follow Nakamura and Steinsson (2014) in calibrating the model to the U.S. setting. The full model specification is relegated to the Appendix; here we present only those aspects of the model modified to study the effects of SAH orders.

Modeling SAH Orders

Our first model experiment is to treat the implementation of SAH orders as a pure local demand shock. To incorporate this into the model, we introduce a consumption preference shock, δ_t. This preference shock causes home region households to prefer, all else equal, delaying consumption into the future. This may be a reasonable way to model the SAH shock for a variety of reasons. First, to the extent that the drop in retail mobility, as shown in Appendix C.1, represents a decline in goods consumption, households may simply be delaying such purchases until temporarily closed stores reopen. Second, the inability to purchase locally furnished goods and services may lead households to temporarily save more than they might otherwise choose to do, which would be observationally equivalent to a discount rate shock only to consumption.

Households in the home region maximize the present discounted value of expected utility over current and future consumption C_t and labor supply N_t.

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\delta_t \frac{(C_t)^{1-\sigma}}{1-\sigma} - \chi \frac{(N_t)^{1+\psi}}{1+\psi} \right],$$

where β is the rate of time discounting, σ is the inverse intertemporal elasticity of substitution, ψ is the inverse Frisch elasticity of labor supply, and χ is the weight on labor supply. The discount rate shock process follows

$$\log \delta_t = \rho^{\delta} \log \delta_{t-1} + \epsilon_t^{\delta}. \tag{4.7}$$

We close the household side of the model by assuming preferences for varieties are constant elasticity of substitution (CES), which gives rise to the standard CES demand curve via cost minimization.

Alternatively, the SAH orders may be modeled as a local productivity shock. Even if demand for locally produced goods is unchanged, firms may be constrained in supplying the goods and services demanded by local households or by the rest of the currency union. We model this interpretation as a region-level productivity shock for intermediate-goods-

[24] Implications from a model with different preference structures (e.g. Greenwood, Hercowitz, and Huffman (1988) preference) and with incomplete market are qualitatively the same. Unlike the original focus of Nakamura and Steinsson (2014), the model we consider does not incorporate government spending shocks, as that is not our focus in this paper.

producing firms. A firm i in the home region faces the following production function

$$y_{h,t}(i) = A_t N_{h,t}(i)^{\alpha},$$

where $y_{h,t}(i)$ is the output of a firm i, $N_{h,t}(i)$ is the amount of labor input hired by the firm, and A_t is region-wide technology in the home region. α is the returns to scale parameter on labor. The aggregate supply shock A_t evolves according to the following process:

$$\log A_t = \rho^A \log A_{t-1} + \epsilon_t^A. \tag{4.8}$$

Firms maximize profits subject to demand by households. Nominal rigidities are specified à la Calvo (1983) with associated price-reset parameter θ.

Finally, we close the model by assuming bond markets are complete, labor markets are perfectly competitive, and, when prices are sticky, the monetary authority follows a union-wide Taylor rule. A full derivation is available in the Appendix.

Model Results: Modeling SAH Order Shocks under Flexible and Sticky Prices

We model the implementation of SAH orders as a one-time negative shock with either $\epsilon_t^\delta = -1$ (for local demand shocks) or $\epsilon_t^A = -1$ (for local supply shocks). We choose zero decay parameters on the shock series to illustrate the dynamics of the model in settings in which the shock induced by the SAH order is temporary. Specifically, we set $\rho^A = \rho^\delta = 0$. For the purposes of mapping the relative-implied employment losses to aggregate employment losses, this is without loss for the results for the technology shock but not without loss with respect to the demand shock with sticky prices. Below, we discuss what happens when the demand shock exhibits some persistence.

We calibrate the remaining parameter values according to Nakamura and Steinsson (2014) (see their Section III.D.). When working with the sticky price model, we set the Calvo parameter $\theta = 0.75$. In the flexible price model, we set $\theta = 0$.

We consider each of the two types of shocks in isolation under either sticky prices or fully flexible prices. In each of the four scenarios, we calculate the on-impact responses of home region employment, foreign region employment, and aggregate employment to the local shock. Because the model is calibrated to a quarterly frequency and because our empirical design estimates the relative effect over a short horizon (3-weeks), the relevant horizon for mapping the model to the cross-section is the *on-impact* relative effect between employment in the shocked home region and the non-shocked foreign region.

The results from these exercises are reported in Figure 4.5 and Table 4.2. Figure 4.5 shows the *on-impact* responses of employment in a home region (blue circles) and a foreign region (red crosses), and aggregate employment (black squares) under the four different scenarios. Table 4.2 then compares the relative-implied aggregate employment calculated from the

[25] Formally, the relative-implied estimate in the model is calculated as $n(\ell_t - \ell_t^*)$, where ℓ_t and ℓ_t^* represent log deviations from steady state of home and foreign region per-capita employment respectively. n is the size of the home-region. This is exactly the model-analog of the relative-implied estimate reported in equation (4.6).

differences between the responses of home and foreign employment and the responses of aggregate employment under different scenarios.[25]

In the model, only three of the four stylized scenarios we consider produce relative effects of SAH orders that are consistent with the positive coefficient we estimate in the data. When the SAH orders are modeled as local productivity shocks, only the flexible price equilibrium produces an immediate, relative decline in employment in the home region subject to the shock. When the SAH orders are instead modeled as local demand shocks, both the sticky price and flexible price economies produce a steeper decline in the shocked home region's employment relative to the rest of the economy, as suggested by the cross-sectional evidence presented above.

When SAH orders are modeled as negative productivity shocks with fully flexible prices, the immediate, relative effect of SAH orders is an *upper bound* on the aggregate employment effect over the same horizon. This is because the decline in local employment arising from the SAH order is offset by an increase in employment in the rest of the economy. The mechanism is that in the flexible price case, the negative productivity shock in the home region translates into an improvement in the foreign region's terms of trade. This, in turn, increases labor demand in the foreign region, which increases employment in the foreign region.

In contrast, when prices are fully flexible in response to an SAH-induced home-region demand shock, the relative-implied estimate represents a *lower* bound on aggregate employment losses. This is because employment in both the home and foreign regions fall in response to the shock. With prices being fully flexible, the negative preference shock in the home region leads to a decline in prices for home goods relative to foreign goods, making foreign consumption more expensive. This, in turn, decreases demand for foreign goods, resulting in a decline in foreign employment, which is necessary for market clearing. When prices are fully flexible and the effect of SAH orders is a pure local demand shock, aggregating the relative employment losses understates the aggregate employment losses by a factor of about two (see Table 4.2, Row 1, Column 3).

The case with sticky prices and SAH orders modeled as a pure local demand shock lies in between the previous two scenarios. When the local demand shock is sufficiently persistent, the immediate, relative effect of SAH orders could potentially *overstate* the aggregate employment effect. This is because employment in the foreign region increases on impact. Meanwhile, when the demand shock has essentially no persistence, so that it only affects demand in the home region for a single quarter, employment in the foreign region also falls on impact, implying that the (aggregated) relative employment effect again understates aggregate employment losses, in the quarter of the shock (See Figure 4.5). Regardless, the degree to which this on-impact effect understates aggregate employment losses is bounded above by the response under flexible prices to a local demand shock.

The evidence presented in Appendix C.1 suggests that SAH orders represented a shock to both the supply of and demand for locally produced goods. This on its own implies that the flexible price, preference shock scenario provides a non-binding upper bound on aggregate employment losses. Specifically, in this scenario the relative-implied aggregate estimate would understate employment losses by roughly a factor of two. The distance from

Table 4.1: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment for Weeks Ending March 21 thru April 4, 2020

	(1) Bivariate	(2) Covid	(3) Pol. Econ.	(4) Sectoral	(5) All
SAH Exposure thru Apr. 4	0.0194***	0.0192**	0.0178**	0.0209***	0.0187**
	(0.00664)	(0.00742)	(0.00818)	(0.00637)	(0.00714)
COVID-19 Cases per 1K		-0.00213			0.00194
		(0.00621)			(0.00676)
Excess Deaths per 1K		0.0446			0.0480
		(0.109)			(0.113)
Share Age 60+		0.237			
		(0.281)			
Avg. UI Replacement Rate			0.0719		0.0726
			(0.0794)		(0.0787)
2016 Trump Vote Share			-0.0225		
			(0.0508)		
Work at Home Index				-0.331^{+}	-0.388^{+}
				(0.192)	(0.229)
Bartik-Predicted Job Loss				-2.401	
				(7.528)	
Constant	0.0815***	0.0357	0.0621	0.181**	0.182**
	(0.00848)	(0.0543)	(0.0481)	(0.0742)	(0.0821)
Adj. R-Square	0.0829	0.0434	0.0618	0.0966	0.0763
No. Obs.	51	51	51	51	51

Table 4.2: On-Impact Response of Union-Wide Employment and Relative-Implied Aggregate Employment to a Local SAH-induced: (i) Preference Shock with Flexible Prices, (ii) Preference Shock with Sticky Prices, (iii) Technology Shock with Flexible Prices, and (iv) Technology Shock with Sticky Prices

	Flexible				Sticky		
	Total	Implied	Factor		Total	Implied	Factor
Preference Shock	-0.047	-0.021	2.21	$\rho^\delta = 0.9$	-0.032	-0.075	0.43
				$\rho^\delta = 0.0$	-0.093	-0.083	1.12
Technology Shock	0.003	-0.021	-0.16		0.1642	0.1398	1.18

this upper bound increases, moreover, with price rigidity and the persistence of the SAH shock. In the baseline calibration, when prices are sticky and the demand shock has no persistence, the relative-implied job losses understates aggregate employment losses by 12%.

Other Cross-Regional Spillovers

The benchmark currency-union model presented in the previous section illustrates how locally implemented SAH orders would affect the local economy, other regions in the currency union and the entire economy as a whole. The spillover forces in the model work through the trade in goods between regions and associated price and expenditure switching effects. However, there may be other important cross-regional spillovers that are not well-captured by the model, but may nevertheless be important for interpreting our empirical results in light of the aggregate effects of SAH orders.

An important example is an *informational effect* of early SAH implementation in some parts of the economy. For example, the early imposition of SAH orders in some regions may signal to the rest of the country that a SAH order is likely to be imposed some time in the near future. This informational channel can be incorporated into the model by assuming that the foreign region learns, on-impact, that a SAH order will be imposed in the foreign region in the subsequent period. We experimented with this specific informational channel of local SAH order implementation and found that the upper and lower bounds provided in the previous subsection continued to hold.[26]

A more subtle informational effect of SAH implementation relates to the credible signal it sends about the severity of the COVID-19 pandemic and the potential economic disruptions it is likely to induce, even in the absence of any additional SAH orders. In this interpretation, the SAH orders have spillover effects on the rest of the economy through the changes they induce to beliefs held by households and firms about the future path of the economy. As opposed to other signals conveyed by public officials about the severity of the pandemic, SAH implementation is a credible signal because it imposes non-trivial costs on the economy. This could, in turn, lead to a reduction in demand as a result of increased economic anxiety and fear of exposure to the COVID-19.

If this second informational effect of local SAH implementation ultimately led to job losses throughout the rest of the country, then our relative-implied estimate would understate the aggregate job losses attributable to SAH orders. Neither the model nor the empirical design takes this particular spillover mechanism into account. We view understanding the role of SAH orders as credibly communicating the severity of the pandemic as an important and interesting avenue for future research.[27]

Another important example is spillovers through firm networks—internal and external.[28]

[26]These results are available upon request.

[27]Coibion, Gorodnichenko, and M. Weber (2020b) provide evidence that local SAH orders led households in the affected regions to hold more pessimistic views of the future path of the economy. This is a separate, though related, channel than the *aggregate* change in beliefs that may have occurred following the early imposition of SAH orders.

[28]We thank an anonymous referee for pointing this out.

For example, complex supply chains may cause economic activity to decline in parts of the country where SAH orders are not yet enacted if the sourcing of intermediate inputs is affected. Alternatively, national chains may close establishments located in regions without SAH orders due to losses in other major markets with SAH orders. Arguably, these sorts of spillovers would lead our relative-implied estimate of job losses to understate true aggregate employment losses. However, we believe these channels are minor, as the adjustments would need to occur over a very short period time. The horizon of our empirical specifications is three weeks, during which time existing inventories were likely to be sufficient for production.[29]

4.6 Alternative Specification: County-Level Employment and Unemployment Effects

A major concern with the estimates of Equation (4.4) is that states may have experienced substantial difficulty in scaling up their systems to process the historically unprecedented numbers of unemployment claims. For example, it is well known that some states' unemployment insurance systems rely on archaic computer programming languages.[30] Thus, it is reasonable to be worried that states with more cumbersome systems may systematically report lower UI claims numbers relative to those states with more efficient systems.

A priori, the induced omitted variable bias could go in either direction. On the one hand, states with stronger UI systems may have also been more inclined to respond aggressively to the COVID-19 pandemic with SAH orders, generating an upward bias in our estimates. On the other hand, the severity of labor market disruptions from the COVID-19 pandemic may have both made it more difficult for states to process new claims *and* made them more likely to impose SAH orders earlier—thus, generating a downward bias. While we have already controlled for measures of COVID-19 in our estimates of Equation (4.4), in this subsection we present an alternative design at the county-level using employment and unemployment as outcomes, albeit at a lower frequency. Using total employment, rather than unemployment insurance claims, allows us to sidestep the issue of whether states could meet demand for UI claims. This design also allows for the inclusion of state fixed effects to identify the relative effect of SAH orders using within-state variation in the timing of SAH implementation.

We analyze the effects of SAH orders at the county-level relying upon local area unemployment and employment statistics constructed by the Bureau of Labor Statistics (BLS). The downside is that this data is constructed at the monthly frequency, rather than the

[29]It is a well known observation that inventories generally adjust more slowly to changes in sales, consistent with the claim that this particular source of bias is most relevant at lower frequencies and longer horizons. (See Ramey and West 1999; Bils and J. A. Kahn 2000).

[30]See, for example, "'COBOL Cowboys' Aim To Rescue Sluggish State Unemployment Systems" by NPR (`https://www.npr.org/2020/04/22/841682627/cobol-cowboys-aim-to-rescue-sluggish-state-unemployment-systems`).

weekly frequency in our main specification.[31] The BLS primarily relies upon the Current Population Survey (CPS) as the primary input into constructing estimates of county-level employment and unemployment.[32] Fortunately, the survey reference periods for the CPS aligns quite nicely with measuring household employment and unemployment just prior to the broad implementation of SAH orders and one month hence. The reference week for the CPS for March 2020 was March 8th through March 14th and the reference week for April was April 12th through April 18th.

We estimate analogs of our state-level regression at the county-level, using as our outcome variable either the log change in employment or the change in the unemployment rate between March 2020 and April 2020. County-level treatment is the weekly SAH exposure through April 15, 2020. Formally, we estimate the following regression by ordinary least squares:

$$\Delta y_{c,s,April} = \alpha_s + \beta^{y}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s} \tag{4.9}$$

where $y_{c,s,April}$ indicates the monthly change between March and April in either log employment or the unemployment rate. α_s are state-level fixed effects which control for all state-level policies implemented between mid-March and mid-April that may have been systematically related to observed UI claims during that period. We also report results when constraining $\alpha_s = \alpha$ to provide a natural benchmark against our state-level regression. We also control for the number of confirmed COVID-19 cases per thousand people and the WAH index, which are our only controls available at the county-level.[33]

Because the first outcome variable we consider at the county-level is the log change in county employment, we expect that the estimated relative effect of SAH orders on local employment, $\hat{\beta}^{emp}_{C,county}$, will be comparable to our estimate of the same parameter at the state-level.[34] If the timing of the decentralized implementation of SAH orders was orthogonal to state-level economic conditions and if there were negligible spillovers from treated counties to untreated counties within the same state, then we would expect to see a relatively stable coefficient regardless of whether we include state fixed effects, α_s, or not.

Table 4.3 provides the results for the effects of SAH orders on employment. The first column shows the results restricting $\alpha_s = \alpha$ (e.g., no state fixed effects). The point estimate suggests that the relative effect of SAH exposure on employment at the county-level is to reduce employment by of -1.8% (SE: .57%). That we use a different outcome variable and different level of disaggregation yet obtain a coefficient of similar magnitude is encouraging.

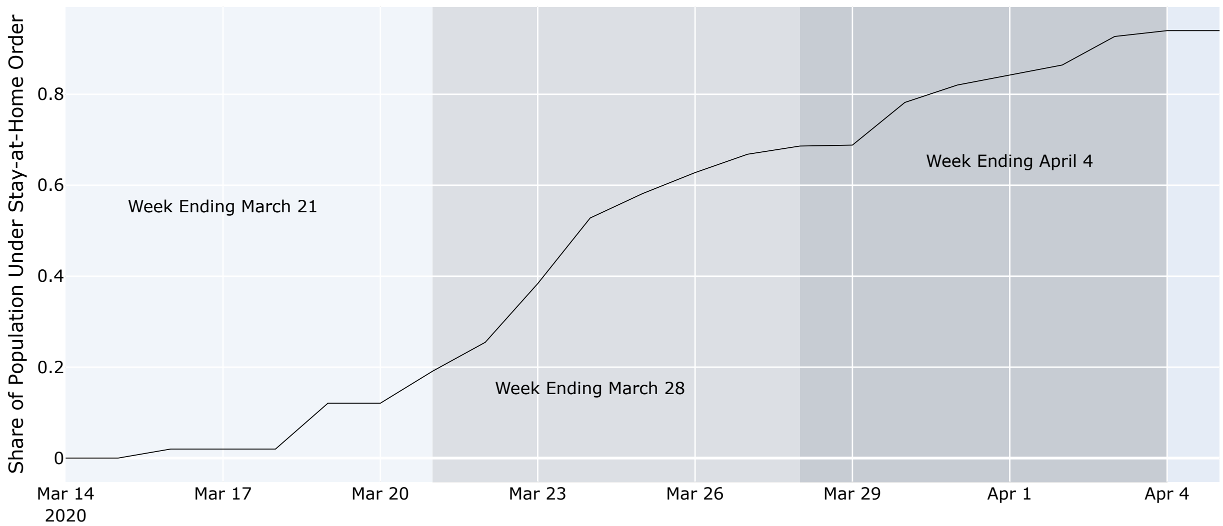
[31] In Appendix C.1 we estimate event study specifications using high frequency employment statistics at the county-level for a subset of counties in the U.S. for which these data exist. We find no evidence of differential changes in county-level employment prior to SAH implementation while at the same time finding that SAH orders lowered employment on average by 1.9% after one week.

[32] For additional details on the methodology employed by the Bureau of Labor Statistics, see `https://www.bls.gov/lau/laumthd.htm`.

[33] We control for the number of confirmed COVID-19 cases through April 15th to align with the timing of the surveys used by the BLS to construct county-level employment and unemployment statistics.

[34] Note that because we use the 2018 QCEW to normalize UI claims at the state-level, we should expect the county-level estimates to be slightly lower in magnitude since the state-level regressions calculates the percent change off of a smaller base value.

Figure 4.1: Cumulative Share of Population under Stay-at-Home Orders in the U.S.



	(1) $\Delta \ln Emp$	(2) $\Delta \ln Emp$	(3) $\Delta \ln Emp$	(4) $\Delta \ln Emp$
SAH Exposure thru Apr. 15	-0.0176***	-0.0124**	-0.0129**	-0.00905**
	(0.00568)	(0.00464)	(0.00453)	(0.00397)
Covid-19 Cases per 1K Emp			-0.0000280	-0.000116
			(0.0000348)	(0.000121)
Work at Home Index			0.0549	0.0547
			(0.0457)	(0.0537)
Constant	-0.0824***	-0.113***	-0.129***	-0.135***
	(0.0147)	(0.00900)	(0.0157)	(0.0139)
Dep Mean	-0.12	-0.14	-0.14	-0.14
States	51.00	12.00	12.00	12.00
State FE	No	Yes	Yes	Yes
CZ FE	No	No	No	Yes
Adj. R-Square	0.10	0.62	0.63	0.74
No. Obs.	3141.00	1116.00	1116.00	453.00

Table 4.3: County-Level Specification: Effect of Stay-at-Home Orders on Local Employment Growth

Columns (2) and (3) focus on the 12 states for which there is variation across counties in the timing of SAH orders. The magnitude of the estimate falls by about one third, regardless of whether we include controls—although this difference is not statistically significant. If, as we argue above, the timing of SAH implementation was orthogonal to policies and economic conditions at the state-level[35], then the decline in the point estimate is suggestive evidence of negative spillovers between treated and untreated counties. While this may be the appropriate interpretation, it appears that the bulk of employment losses were nevertheless concentrated within the labor markets in which SAH orders were implemented.

Finally, in the last column, we include commuting zone fixed effects and find that the coefficient is roughly a third of the effect estimated in column (3). Following a similar logic as in the previous paragraph, this would suggest that not only were the bulk of employment losses concentrated within the labor market, they were moreover concentrated within the specific counties in which the SAH orders were implemented.

Table 4.4 provides the results for the effects of SAH orders on the change in the county-level unemployment rate. As with the employment specification, the first column does not include state fixed effects. In columns (2) and (3) we include state fixed effects; in the final column, we condition further on commuting zone fixed effects. Consider the result reported in column (3), the state fixed effects specification with controls for local COVID-19 pandemic and capacity for the local labor force to work from home: the point estimate is 1.5 (SE: 0.331), implying that each week of SAH exposure at the county-level increased the local unemployment rate by 1.5.

In sum, we view the the county-level results as corroborating evidence of the main result in this paper: that the cross-sectional effect of SAH orders had real costs to the labor markets in the early weeks of the crisis, but that such costs were likely dwarfed by other factors in the early weeks of the crisis. While not inconsistent with our state-level analysis, broadly the county-level design yields somewhat lower point estimates than in our benchmark specification. In this respect, relative to a null that all observed UI claims were attributable to SAH orders, the state-level specification yields the most conservative estimate of the relative effect of such orders on local labor markets. Through the lens of our theoretical model, these cross-sectional estimates imply, at most, a non-binding upper bound of half of total UI claims through April 4, 2020 being attributable to SAH orders.

4.7 Conclusion

While non-pharmaceutical interventions (NPIs) are necessary to slow the spread of viruses such as COVID-19, they likely steepen the recession curve. But to what extent? We provide estimates of how much one prominent NPI disrupted local labor markets in the short run in the U.S. in the early weeks of the coronavirus pandemic.

In particular, we investigate the effect of Stay-at-Home (SAH) orders on new unemployment claims in order to quantify the causal effect of this severe NPI (i.e., flattening the pan-

[35] And the average treatment effect among counties in the twelve states appearing in columns (2)-(4) is the same as for counties.

demic curve) on economic activity (i.e., steepening the recession curve). The decentralized implementation of SAH orders in the U.S. induced both geographic and temporal variation in when regions were subject to restrictions on economic and social mobility. Between March 14th and April 4th, the share of workers under such orders rose from 0% to almost 95%. This rise was gradual but steady, with new areas implementing SAH orders on a daily basis. We couple this variation in SAH implementation with high-frequency unemployment claims data to quantify the resulting economic disruption.

We find that a one-week increase in stay-at-home orders raised unemployment claims by 1.9% of state-level employment. This estimate is robust to a battery of controls, including the severity of the local COVID-19 pandemic, the local political economy response, and the industry mix of the local economy. A back-of-the-envelope calculation using our estimate implies that SAH orders resulted in a rise of 4 million unemployment insurance claims, about a quarter of the total unemployment insurance claims during this period.

A stylized currency union model suggests that in some empirically relevant cases, this back-of-the-envelope estimate can be seen as an upper bound on aggregate job losses. When it instead represents a lower bound, it at most understates job losses by a factor of two. The methodological approach we take in this paper—coupling a causally identified cross-sectional moment with a benchmark currency union model—is analogous to the approach developed in the previous two chapters of this dissertation.

While it is beyond the scope of this paper to uncover all determinants of the unprecedented initial rise in unemployment during the COVID-19 pandemic, there is evidence that the economic downturn was already under way by the time that SAH orders were implemented. Even before the national emergency was announced by President Trump on March 13, 2020, households were reallocating their spending away from in-person goods and services.[36] Consistent with this evidence, our estimates imply that a sizeable share of the increase in unemployment in the early weeks of the COVID-19 crisis was due to other channels, such as decreased consumer sentiment, stock market disruptions, and social distancing that would have occurred in the absence of government orders.

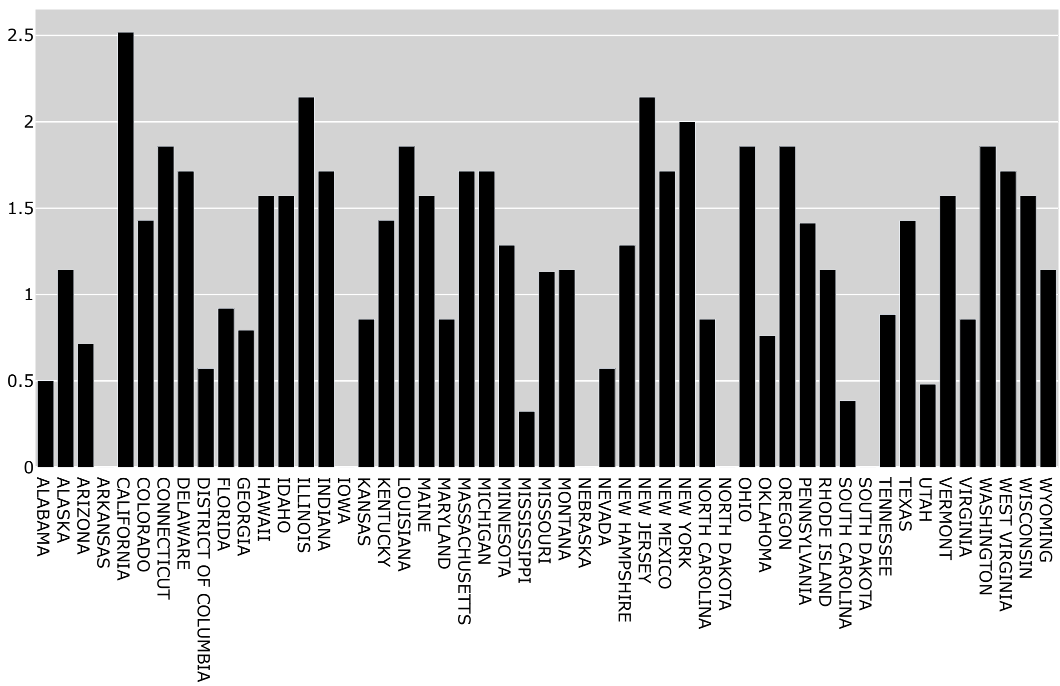
Nevertheless, despite representing a minority share of the overall increase in unemployment in the initial three weeks of the crisis, our estimates suggest that over longer horizons SAH orders played a much larger role. Performing an out-of-sample forecast through April 25 of the relative-implied aggregate effect of SAH orders is illustrative: An additional 7.5 million UI claims between April 4 and April 25 are due to SAH orders, little more than half of the additional overall increase in UI claims nationally during that time.[37]

[36]By March 13, grocery spending was up 44%, restaurant spending was down 10%, and entertainment and recreation spending was down 23%, all relative to their respective levels in January 2020. At about the same time—and preceding any reported SAH orders—both national consumer spending and small business revenue began their precipitous declines. Statistics calculated from data available at `https://tracktherecovery.org/`.

[37]This helps to reconcile our estimates with Coibion, Gorodnichenko, and M. Weber (2020b) who find a larger contribution of SAH orders to job losses throughout April than we do. In this exercise, we adjust for whether a state reopened before April 25; not adjusting increases the out-of-sample forecast to 7.6 million claims. See `https://www.nytimes.com/interactive/2020/us/states-reopen-map-coronavirus.html` for state reopening dates.

In sum, we see our paper as providing evidence that undoing SAH orders may relieve only a fraction of the economic disruption arising from the COVID-19 pandemic while at the same time exacerbating the public health crisis. This implies that the economic downturn may persist at least until the pandemic itself is resolved. At the same time, we document a large elasticity of unemployment with respect to such lockdown measures, suggesting that the costs of SAH orders are non-trivial in the long-run.

Figure 4.2: Employment-Weighted State Exposure to Stay-at-Home Policies Through Week Ending April 4



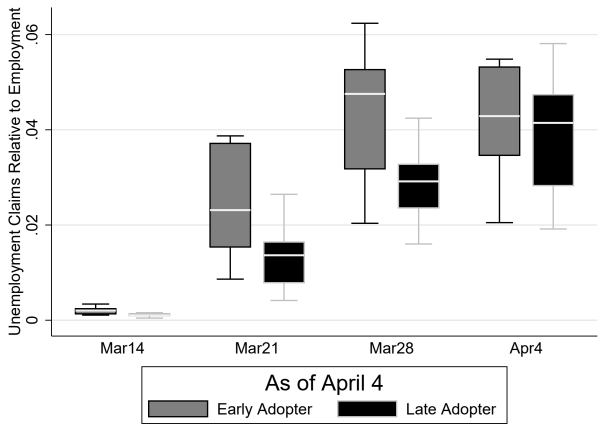
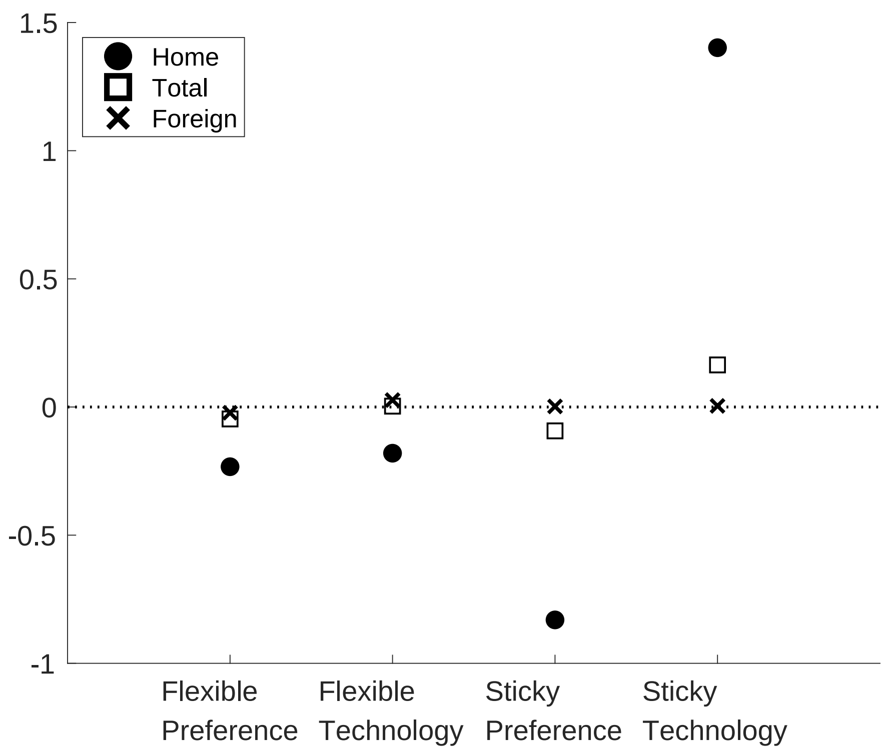


Figure 4.3: Box Plots by Week of Initial UI Claims Relative to Employment for Early and Late Adopters of SAH orders

Figure 4.4: Scatterplot of SAH Exposure to Cumulative Initial Weekly Claims for Weeks Ending March 21 thru April 4

Figure 4.5: On-Impact Response of Home Employment, Foreign Employment, and Union-Wide Employment to a Local SAH-induced: (i) Technology Shock with Flexible Prices, (ii) Technology Shock with Sticky Prices, (iii) Preference Shock with Flexible Prices, and (iv) Preference Shock with Sticky Prices



	(1)	(2)	(3)	(4)
	ΔUR	ΔUR	ΔUR	ΔUR
SAH Exposure thru Apr. 15	1.574***	1.382***	1.570***	0.944***
	(0.400)	(0.331)	(0.331)	(0.216)
Covid-19 Cases per 1K Emp			-0.000239	0.0110
			(0.00468)	(0.00806)
Work at Home Index			-12.29**	-5.437
			(5.336)	(5.089)
Constant	4.114***	4.425***	7.922***	6.689***
	(0.888)	(0.642)	(2.005)	(1.863)
Dep Mean	7.69	7.11	7.11	7.32
States	51.00	12.00	12.00	12.00
State FE	No	Yes	Yes	Yes
CZ FE	No	No	No	Yes
Adj. R-Square	0.13	0.39	0.40	0.59
No. Obs.	3141.00	1116.00	1116.00	453.00

Table 4.4: County-Level Specification: Effect of Stay-at-Home Orders on Local Unemployment Rate

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Appendix A

Appendix to Chapter 2

A.1 Solving for Log-Linearized Differential Equations

For a typical variable Z_t, I use the notation $\check{z}_t$ to refer to the log-deviation in Z_t around its steady state value. As defined in the main text, this implies that $\check{c}_t(1-\mathcal{G}) = c_t$ and $\check{g}_t\mathcal{G} = g_t$. Deviations in consumption of home-state produced goods scaled by steady state consumption is denoted by $\tilde{c}_t$. Deviations in terms of gross production are defined as $\check{\tilde{y}}_t$. Variables for states in the rest of the currency union are defined similarly.

Defining Various Price Indices, Terms of Trade, and Real Exchange Rates

Because states in the home-region are treated symmetrically and because all other states are treated symmetrically, the currency-wide price index

$$P_t^* = \left(\mu {P_t^M}^{1-\gamma} + (1-\mu){P_t^{-M}}^{1-\gamma}\right)^{\frac{1}{1-\gamma}},$$

where P_t^M is the PPI for the typical home-region state (not the home state):

$$P_t^M = \left(\int_0^1 P_t^M(j)^{1-\epsilon} dj\right)^{\frac{1}{1-\epsilon}}.$$

The price index associated with the typical state in the foreign region is similarly defined. The home state consumer price index (CPI) is

$$P_t = \left[(1-\alpha)P_{H,t}^{1-\eta} + \alpha {P_t^*}^{1-\eta}\right]^{\frac{1}{1-\eta}},$$

with the home state PPI given by

$$P_{H,t} = \left(\int_0^1 P_{H,t}(j)^{1-\epsilon} dj \right)^{\frac{1}{1-\epsilon}}.$$

The CPI for the home region M is similarly given by:

$$P_{M,t} = \left[(1-\alpha) P_t^{M\,1-\eta} + \alpha P_t^{*\,1-\eta} \right]^{\frac{1}{1-\eta}}.$$

The CPI for the foreign regions $-M$ are defined in the same way.

The introduction of roundabout production within regions of the currency union implies that we need to keep track of multiple terms of trade and real exchange rates. Define the home-state terms of trade (for a foreign import) as

$$S_t = \frac{P_t^*}{P_{H,t}},$$

the home-state terms of trade (for a region-wide import) as

$$S_t^M = \frac{P_t^M}{P_{H,t}},$$

the union-wide real exchange rate as

$$\mathcal{Q}_t = \frac{P^*}{P_t},$$

and the real exchange rate with the region in which the home-state is located:

$$Q_t^M = \frac{P_{M,t}}{P_t}$$

Useful Steady State Ratios In the symmetric, no inflation steady state, we have for the typical firm in state j :

$$\tilde{\theta} = \frac{P^* M(j)}{P^* \tilde{Y}(j)}$$

Integrating over all home-state firms, we get:

$$\tilde{\theta}(Y + X^M) = \tilde{X}$$

where $\tilde{X}$ is the home-state demand for intermediates and X^M is home-production of intermediates. In the symmetric steady state, the home price is the same as the price in the rest of the region. Thus, we have that the ratio of final value added produced to gross production in the home (and typical) state is as in the closed economy case:

$$\frac{Y}{\tilde{Y}} = (1 - \tilde{\theta})$$

And similarly

$$\frac{\tilde{X}}{\tilde{Y}} = \tilde{\theta}$$

Exchange Rates Note:

$$P_t = ((1-\alpha)P_{H,t}^{1-\eta} + \alpha P_t^{*1-\eta})^{\frac{1}{1-\eta}}$$

Around steady state:

$$\check{p}_t = (1-\alpha)\check{p}_{H,t} + \alpha \check{p}_t^* \implies \check{p}_t^* - \check{p}_t = (1-\alpha)(\check{p}_t^* - \check{p}_{H,t})$$

So that

$$\check{q}_t = (1-\alpha)\check{s}_t$$

We can also decompose the currency wide price index

$$\check{p}_t^* = \mu \check{p}_t^M + (1-\mu)\check{p}_t^{-M}$$

Market Clearing Condition As a matter of accounting, total consumption of home-state produced goods is given by:

$$\begin{aligned}\tilde{C}_t = (1-\alpha)&\left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t \\ &+ \alpha\left[\mu\left(\frac{P_t^*}{P_{M,t}}\right)^{-\eta}\left(\frac{P_{H,t}}{P_t^*}\right)^{-\gamma} C_t^M + (1-\mu)\left(\frac{P_t^*}{P_{-M,t}}\right)^{-\eta}\left(\frac{P_{H,t}}{P_t^*}\right)^{-\gamma} C_t^{-M}\right].\end{aligned}$$

Log-linearizing this market-clearing condition for total consumption of home produced goods, we get

$$\begin{aligned}\tilde{c}_t = (1-\alpha)&(c_t + \alpha\eta(1-\mathcal{G})\check{s}_t) + \alpha\gamma(1-\mathcal{G})\check{s}_t \\ &+ \alpha\eta(1-\mathcal{G})(\mu(\check{p}_{M,t} - \check{p}_t^*) + (1-\mu)(\check{p}_{-M,t} - \check{p}_t^*)) + \alpha(\mu c_t^M + (1-\mu)c_t^{-M})\end{aligned}$$

which implies, since $\mu\check{c}_t^M + (1-\mu)\check{c}_t^{-M} = \check{c}_t^*$ and $\check{p}_t^* = \mu\check{p}_t^M + (1-\mu)\check{p}_t^{-M}$,

$$\tilde{c}_t = (1-\alpha)c_t + \alpha((1-\alpha)\eta + \gamma)(1-\mathcal{G})\check{s}_t + \alpha c_t^*$$

Time differentiating:

$$\dot{\tilde{c}}_t = (1-\alpha)\dot{c}_t + \alpha((1-\alpha)\eta + \gamma)(1-\mathcal{G})(\pi_t^* - \pi_{H,t}) + \alpha\dot{c}_t^*$$

Next, log-linearize the Backus-Smith condition:

$$\tilde{c}_t = \mu c_t^M + (1-\mu)c_t^{-M} + \frac{1}{\sigma}(1-\mathcal{G})(\check{p}_t^* - \check{p}_t) = c_t^* + \frac{1}{\sigma}(1-\mathcal{G})(1-\alpha)\check{s}_t$$

Time differentiating the Backus-Smith condition:

$$\dot{c}_t = \dot{c}_t^* + \frac{1}{\sigma}(1-\alpha)(1-\mathcal{G})(\pi_t^* - \pi_{H,t})$$

Solving for $(1-\mathcal{G})(\pi_t^* - \pi_{H,t})$ yields

$$(1-\mathcal{G})(\pi_t^* - \pi_{H,t}) = \frac{\sigma}{1-\alpha}\dot{c}_t - \frac{\sigma}{1-\alpha}\dot{c}_t^*$$

Plugging into the previous expression:

$$\dot{\tilde{c}}_t = (1-\alpha)\dot{c}_t + \frac{\alpha((1-\alpha)\eta+\gamma)\sigma}{1-\alpha}\dot{c}_t - \frac{\alpha((1-\alpha)\eta+\gamma)\sigma}{1-\alpha}\dot{c}_t^* + \alpha\dot{c}_t^*$$

yielding

$$\dot{\tilde{c}}_t = \dot{c}_t + \alpha\left[\frac{((1-\alpha)\eta+\gamma)\sigma}{1-\alpha} - 1\right](\dot{c}_t - \dot{c}_t^*)$$

Rearranging, we get the following:

$$\dot{\tilde{c}}_t = \dot{c}_t + \alpha\left[\frac{(1-\alpha)\eta\sigma+\gamma\sigma-1+\alpha}{1-\alpha}\right](\dot{c}_t - \dot{c}_t^*)$$

which simplifies with $\omega \equiv \sigma\gamma + (1-\alpha)(\sigma\eta - 1)$ to

$$\dot{\tilde{c}}_t = \frac{1-\alpha+\alpha\omega}{1-\alpha}\dot{c}_t - \frac{\alpha\omega}{1-\alpha}\dot{c}_t^*$$

Plugging in the Euler equation then yields

$$\dot{\tilde{c}}_t = \frac{1-\alpha+\alpha\omega}{1-\alpha}\frac{1-\mathcal{G}}{\sigma}(i_t^* - \pi_t - \rho) - \frac{\alpha\omega}{1-\alpha}\dot{c}_t^*$$

Observing that $\pi_t = (1-\alpha)\pi_{H,t} + \alpha\pi_t^*$:

$$\dot{\tilde{c}}_t = \hat{\sigma}^{-1}\frac{1}{1-\alpha}((1-\alpha)i_t^* - (1-\alpha)\pi_{H,t} - (1-\alpha)\rho) + \hat{\sigma}^{-1}\frac{\alpha}{1-\alpha}(i_t^* - \pi_t^* - \rho) - \frac{\alpha\omega}{1-\alpha}\dot{c}_t^*$$

with $\hat{\sigma} \equiv \frac{\sigma}{(1-\alpha+\alpha\omega)(1-\mathcal{G})}$.

Simplifying, using the Euler equation for the currency union, we have

$$\dot{\tilde{c}}_t = \hat{\sigma}^{-1}(i_t^* - \pi_{H,t} - \rho) + \frac{\alpha(1-\alpha+\alpha\omega)}{1-\alpha}\dot{c}_t^* - \frac{\alpha\omega}{1-\alpha}\dot{c}_t^*$$

which further simplifies to

$$\dot{\tilde{c}}_t = \hat{\sigma}^{-1}(i_t^* - \pi_{H,t} - \rho) + \alpha \left[\frac{1-\alpha+\alpha\omega-\omega}{1-\alpha}\right] \dot{c}_t^*$$

and finally

$$\dot{\tilde{c}}_t = \hat{\sigma}^{-1}(i_t^* - \pi_{H,t} - \rho) - \alpha(\omega-1)\dot{c}_t^*$$

which is equation (6) in Farhi and Werning (2016).

Relating Consumption of Home Produced Goods, Union Consumption, and Terms of Trade Recalling the market clearing condition for consumption of home-produced goods:

$$\tilde{c}_t = (1-\alpha)c_t + \alpha((1-\alpha)\eta+\gamma)(1-\mathcal{G})\check{s}_t + \alpha c_t^*$$

Plug in for home consumption deviations with the Backus-Smith condition $c_t = c_t^* + \frac{1-\mathcal{G}}{\sigma}(1-\alpha)\check{s}_t$:

$$\tilde{c}_t = (1-\alpha)c_t^* + \frac{(1-\alpha)^2(1-\mathcal{G})}{\sigma}\check{s}_t + \alpha((1-\alpha)\eta+\gamma)(1-\mathcal{G})\check{s}_t + \alpha c_t^*$$

Simplifying:

$$\tilde{c}_t = c_t^* + (1-\mathcal{G})\frac{(1-\alpha)^2+\alpha((1-\alpha)\eta+\gamma)\sigma}{\sigma}\check{s}_t$$

Observe that

$$(1-\alpha)^2+\alpha((1-\alpha)\eta+\gamma)\sigma = (1-\alpha)^2+\alpha(1-\alpha)\eta\sigma+\alpha\gamma\sigma = (1-\alpha)(1-\alpha+\alpha\eta\sigma)+\alpha\gamma\sigma)$$

The RHS becomes $1-\alpha+\alpha\omega$ with, as a reminder, $\omega \equiv \sigma\gamma + (1-\alpha)(\sigma\eta-1)$. Thus, using again the definition that $\hat{\sigma} \equiv \frac{\sigma}{(1-\mathcal{G})(1-\alpha+\alpha\omega)}$, we have $\tilde{c}_t = c_t^* + \hat{\sigma}^{-1}\check{s}_t$, implying

$$\check{s}_t = \hat{\sigma}\tilde{c}_t - \hat{\sigma}c_t^*$$

This additionally implies that

$$\check{s}_{M,t} = \hat{\sigma}\tilde{c}_t^M - \hat{\sigma}c_t^*$$

where $\check{s}_{M,t} \equiv \check{p}_t^* - \check{p}_t^M$.

Together, these two expressions further imply

$$\check{p}_t^M - \check{p}_t = \hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) + \alpha\hat{\sigma}(c_t^* - \tilde{c}_t) \quad \text{and} \quad \check{p}_t^M - \check{p}_{H,t} = \hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M),$$

two expressions that ease the derivation of the NKPC.

Real Marginal Costs Real marginal costs for the firm deflated by home PPI, in log deviations,

$$MC_t \propto \frac{W_t^{1-\theta} P_t^{M\theta} P_t^{1-\theta}}{P_{H,t} P_t^{1-\theta}} \implies \check{mc}_t = (1-\theta)(\check{w}_t - \check{p}_t) + \theta(\check{p}_t^M - \check{p}_t) + \check{p}_t - \check{p}_{H,t}$$

simplifying slightly yields:

$$\check{mc}_t = (1-\theta)(\check{w}_t - \check{p}_t) + \theta\check{(}\check{p}_t^M - \check{p}_t) + \alpha\check{s}_t$$

Plugging in for $(\check{p}_t^M - \check{p}_t)$ and $\check{s}_t$ yields

$$\check{mc}_t = (1-\theta)(\check{w}_t - \check{p}_t) + \theta(\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) + \alpha\hat{\sigma}(c_t^* - \tilde{c}_t)) + \alpha\hat{\sigma}(\tilde{c}_t - c_t^*)$$

Combining like terms yields:

$$\check{mc}_t = (1-\theta)(\check{w}_t - \check{p}_t) + \theta\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) + (1-\theta)\alpha\hat{\sigma}(\tilde{c}_t - c_t^*)$$

Real Wage The household's intratemporal condition in the home state is unchanged:

$$\sigma\check{c}_t + \phi\check{n}_t = (\check{w}_t - \check{p}_t)$$

Log linearizing total labor demand:

$$\check{n}_t = \check{\check{y}}_t - \theta(\check{w}_t - \check{p}_t) + \theta(\check{p}_t^M - \check{p}_t)$$

which we get from adding and subtracting home CPI price-deviations. Plugging this into labor supply equation and solving for the real wage

$$\check{w}_t - \check{p}_t = \frac{\phi}{1+\theta\phi}\check{\check{y}}_t + \frac{\sigma}{1+\theta\phi}\check{c}_t + \frac{\theta\phi}{1+\theta\phi}\left[\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) + \alpha\hat{\sigma}(c_t^* - \tilde{c}_t)\right]$$

Substituting home consumption with union consumption from the Backus-Smith condition and substituting for $\check{s}_t$ yields

$$\begin{aligned}\check{w}_t - \check{p}_t = &\frac{\phi}{1+\theta\phi}\check{\check{y}}_t + \frac{\sigma}{1+\theta\phi}\left(\frac{1}{1-\mathcal{G}}c_t^* + \frac{1}{\sigma}(1-\alpha)\hat{\sigma}(\tilde{c}_t - c_t^*)\right)\\ &+ \frac{\theta\phi}{1+\theta\phi}\left[\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) + \alpha\hat{\sigma}(c_t^* - \tilde{c}_t)\right]\end{aligned}$$

Combining like terms:

$$\begin{aligned}\breve{w}_t - \breve{p}_t &= \frac{\phi}{1+\theta\phi}\breve{\tilde{y}}_t + \frac{\frac{\sigma}{(1-\mathcal{G})}}{1+\theta\phi}c_t^* \\ &+ \left(\frac{(1-\alpha)\hat{\sigma}}{1+\theta\phi} - \frac{\theta\phi\alpha\hat{\sigma}}{1+\theta\phi}\right)(\tilde{c}_t - c_t^*) \\ &+ \frac{\theta\phi\hat{\sigma}}{1+\theta\phi}(\tilde{c}_t - \tilde{c}_t^M)\end{aligned}$$

which can be simplified further to

$$\begin{aligned}\breve{w}_t - \breve{p}_t &= \frac{\phi}{1+\theta\phi}\breve{\tilde{y}}_t + \frac{\frac{\sigma}{(1-\mathcal{G})}}{1+\theta\phi}c_t^* \\ &+ \left(\frac{\hat{\sigma}}{1+\theta\phi} - \alpha\hat{\sigma}\right)(\tilde{c}_t - c_t^*) \\ &+ \frac{\theta\phi\hat{\sigma}}{1+\theta\phi}(\tilde{c}_t - \tilde{c}_t^M)\end{aligned}$$

Region-Wide Intermediates in Terms of Region Consumption and Union Government Spending Next, log-linearize the market clearing condition for gross output ($\tilde{Y}_t^M = C_t^M + G_t^M + X_t^M$):

$$\breve{\tilde{y}}_t^M = (1-\tilde{\theta})\breve{y}_t^M + \tilde{\theta}\breve{x}_t^M$$

Note that $\frac{P_t^M X_t^M}{P_t^M \tilde{Y}_t^M} = \theta MC_t^M \Delta_t^M$, where Δ_t^M is a measure of region-wide price dispersion which is approximately zero in log-deviations. This implies that $\breve{x}_t^M = \breve{\tilde{y}}_t^M + \breve{mc}_t^M$, further implying that $\breve{\tilde{y}}_t^M = \breve{y}_t^M + \frac{\tilde{\theta}}{1-\tilde{\theta}}\breve{mc}_t^M$. Implying that deviations in region-wide intermediates depends upon final consumption of region-wide goods adjusted by changes in region-wide marginal costs:

$$\breve{x}_t^M = \tilde{c}_t^M + g_t^M + \frac{1}{1-\tilde{\theta}}\breve{mc}_t^M$$

Simplifying Marginal Costs Gross production of home-produced goods is (see Steady State Section)

$$\breve{\tilde{y}}_t = (1-\tilde{\theta})\breve{y}_t + \tilde{\theta}\breve{x}_t$$

which, after plugging in for deviations in intermediates production, becomes

$$\breve{\tilde{y}}_t = (1-\tilde{\theta})(\tilde{c}_t + g_t) + \tilde{\theta}\Big[\tilde{c}_t^M + g_t^M + \frac{1}{1-\tilde{\theta}}\breve{mc}_t^M + \gamma(\breve{p}_t^M - \breve{p}_{H,t})\Big]$$

Plugging in for $(\breve{p}_t^M - \breve{p}_{H,t}) = \hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M)$, we have:

$$\breve{\tilde{y}}_t = (1-\tilde{\theta})(\tilde{c}_t + g_t) + \tilde{\theta}\Big[\tilde{c}_t^M + g_t^M + \frac{1}{1-\tilde{\theta}}\breve{mc}_t^M + \gamma\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M)\Big]$$

Plugging this into the real wage equation:

$$\begin{aligned}\breve{w}_t - \breve{p}_t = &\frac{\phi}{1+\theta\phi}\left[(1-\tilde{\theta})(\tilde{c}_t + g_t) + \tilde{\theta}\Big[\tilde{c}_t^M + g_t^M + \frac{1}{1-\tilde{\theta}}\breve{mc}_t^M + \gamma\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M)\Big]\right] \\ &+ \frac{\frac{\sigma}{(1-\mathcal{G})}}{1+\theta\phi}c_t^* \\ &+ \left(\frac{\hat{\sigma}}{1+\theta\phi} - \alpha\hat{\sigma}\right)(\tilde{c}_t - c_t^*) \\ &+ \frac{\theta\phi\hat{\sigma}}{1+\theta\phi}(\tilde{c}_t - \tilde{c}_t^M)\end{aligned}$$

Real Marginal Costs and Real Wage in Terms of Consumption of Home Produced, Home-Region Produced, and Union Wide Consumption Recall the definition for real marginal costs:

$$\breve{mc}_t = (1-\theta)(\breve{w}_t - \breve{p}_t) + \theta\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) + (1-\theta)\alpha\hat{\sigma}(\tilde{c}_t - c_t^*)$$

Together with the real wage expression, this implies

$$\begin{aligned}\frac{\breve{mc}_t}{1-\theta} = &\frac{\phi}{1+\theta\phi}\left[(1-\tilde{\theta})(\tilde{c}_t + g_t) + \tilde{\theta}\Big[\tilde{c}_t^M + g_t^M + \frac{1}{1-\tilde{\theta}}\breve{mc}_t^M + \gamma\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M)\Big]\right] \\ &+ \frac{\frac{\sigma}{(1-\mathcal{G})}}{1+\theta\phi}c_t^* \\ &+ \left(\frac{\hat{\sigma}}{1+\theta\phi} - \alpha\hat{\sigma}\right)(\tilde{c}_t - c_t^*) \\ &+ \frac{\theta\phi\hat{\sigma}}{1+\theta\phi}(\tilde{c}_t - \tilde{c}_t^M) \\ &+ \frac{\theta}{1-\theta}\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) \\ &+ \alpha\hat{\sigma}(\tilde{c}_t - c_t^*)\end{aligned}$$

Simplifying a bit:

$$\frac{\check{mc}_t}{1-\theta} = \frac{\phi}{1+\theta\phi}\Big[(1-\tilde{\theta})(\tilde{c}_t + g_t) + \tilde{\theta}\Big[\tilde{c}_t^M + g_t^M + \gamma\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M)\Big]\Big]$$
$$+ \frac{\frac{\sigma}{(1-\mathcal{G})}}{1+\theta\phi} c_t^*$$
$$+ \left(\frac{\hat{\sigma}}{1+\theta\phi}\right)(\tilde{c}_t - c_t^*)$$
$$\left[\frac{\theta\phi\hat{\sigma}}{1+\theta\phi} + \frac{\theta}{1-\theta}\hat{\sigma}\right](\tilde{c}_t - \tilde{c}_t^M)$$
$$+ \frac{\tilde{\theta}\phi}{(1+\theta\phi)(1-\tilde{\theta})}\check{mc}_t^M$$

Combining like terms:

$$\frac{\check{mc}_t}{1-\theta} = \left[\frac{\phi(1-\tilde{\theta}) + \phi\tilde{\theta}\gamma\hat{\sigma}}{1+\theta\phi} + \hat{\sigma} + \frac{\theta}{1-\theta}\hat{\sigma}\right]\tilde{c}_t$$
$$+ \frac{(1-\tilde{\theta})\phi}{1+\theta\phi} g_t$$
$$+ \frac{\tilde{\theta}\phi}{1+\theta\phi} g_t^M$$
$$+ \left[\frac{\frac{\sigma}{(1-\mathcal{G})} - \hat{\sigma}}{1+\theta\phi}\right] c_t^*$$
$$+ \left[\frac{\tilde{\theta}\phi - \tilde{\theta}\phi\gamma\hat{\sigma} - \theta\phi\hat{\sigma}}{1+\theta\phi} - \frac{\theta}{1-\theta}\hat{\sigma}\right]\tilde{c}_t^M$$
$$+ \frac{\tilde{\theta}\phi}{(1+\theta\phi)(1-\tilde{\theta})}\check{mc}_t^M$$

We now need to solve for region-wide marginal costs, $\check{mc}_t^M$. At the region-level we replace state-level variables with their region-level counterparts (i.e. $\tilde{c}_t = \tilde{c}_t^M$). Some terms cancel out so that we are left with

$$\begin{aligned}\frac{\check{mc}_t^M}{1-\theta} = & \frac{\phi+\hat{\sigma}}{1+\theta\phi}\tilde{c}_t^M \\ & + \frac{\phi}{1+\theta\phi}g_t^M \\ & + \left[\frac{\frac{\sigma}{(1-\mathcal{G})}-\hat{\sigma}}{1+\theta\phi}\right]c_t^* \\ & + \frac{\tilde{\theta}\phi}{(1+\theta\phi)(1-\tilde{\theta})}\check{mc}_t^M\end{aligned}$$

Next, we solve for marginal costs as a function of consumption of region-produced goods, union-wide consumption and the forcing variable of region-wide government spending:

$$\check{mc}_t^M = \left[1 - \frac{(1-\theta)\tilde{\theta}\phi}{(1+\theta\phi)(1-\tilde{\theta})}\right]^{-1} \frac{1-\theta}{1+\theta\phi}\left[(\phi+\hat{\sigma})\tilde{c}_t^M + \phi g_t^M + \left(\frac{\sigma}{1-\mathcal{G}} - \hat{\sigma}\right)c_t^*\right]$$

Which implies that

$$\check{mc}_t^M = \underbrace{\frac{(1-\tilde{\theta})(1-\theta)}{(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi}}_{\chi(\theta)}\left[(\phi+\hat{\sigma})\tilde{c}_t^M + \phi g_t^M + \left(\frac{\sigma}{1-\mathcal{G}} - \hat{\sigma}\right)c_t^*\right],$$

where $\chi(\theta)$ is a function of θ, holding fixed other primitives in the model. It is straightforward to show that $\chi(\theta) < 1$.[1]

[1] Consider the denominator. Because $\theta > \tilde{\theta}$, we have $(1+\theta\phi)(1-\tilde{\theta})-(1-\theta)\tilde{\theta}\phi > (1+\theta\phi)(1-\tilde{\theta})-(1-\tilde{\theta})\tilde{\theta}\phi = (1-\tilde{\theta})(1+\theta\phi-\tilde{\theta}\phi) = (1-\tilde{\theta})(1+\phi(\theta-\tilde{\theta})) > (1-\tilde{\theta})$. Thus, the ratio is less than 1.

Plugging this back into the expression for state-level marginal costs, yields:

$$
\begin{aligned}
\frac{\check{mc}_t}{1-\theta} = & \left[\frac{\phi(1-\tilde{\theta})+\phi\tilde{\theta}\gamma\hat{\sigma}}{1+\theta\phi} + \hat{\sigma} + \frac{\theta}{1-\theta}\hat{\sigma}\right]\tilde{c}_t \\
& + \frac{(1-\tilde{\theta})\phi}{1+\theta\phi} g_t \\
& + \left[\frac{\tilde{\theta}\phi}{1+\theta\phi} + + \frac{\tilde{\theta}\phi\chi(\theta)}{(1+\theta\phi)(1-\tilde{\theta})}\phi\right] g_t^M \\
& + \left[\frac{\frac{\sigma}{(1-\mathcal{G})} - \hat{\sigma}}{1+\theta\phi} + \frac{\tilde{\theta}\phi\chi(\theta)}{(1+\theta\phi)(1-\tilde{\theta})}\left(\frac{\sigma}{1-\mathcal{G}} - \hat{\sigma}\right)\right] c_t^* \\
& + \left[\frac{\tilde{\theta}\phi - \tilde{\theta}\phi\gamma\hat{\sigma} - \theta\phi\hat{\sigma}}{1+\theta\phi} - \frac{\theta}{1-\theta} + \frac{\tilde{\theta}\phi\chi(\theta)}{(1+\theta\phi)(1-\tilde{\theta})}(\phi+\hat{\sigma})\right]\tilde{c}_t^M
\end{aligned}
$$

Define the coefficients pre-multiplying consumption of home produced goods, government spending in the home state, government spending in the home region, union-wide consumption, and consumption in the home region as $\{\beta_{\tilde{c}}, \beta_g, \beta_{g^M}, \beta_{c^*}, \beta_{c^M}\}$. Combining with the log-linearized firm's problem yields the state NKPC.

A.2 Definitions of Instantaneous Multiplier Coefficients

In this section I define the instantaneous multiplier coefficients that appear in Section 2.2 for both the state-level and region-level dynamic systems. To ease exposition, I restate the dynamic systems for both.

Because the instantaneous multiplier coefficients at the state-level depend upon the consumption response at the region-level, I first define the region-level instantaneous multiplier coefficients before turning to the state-level system.

Region Multiplier Coefficients

The system of differential equations characterizing the path of consumption of home region produced goods and PPI in the home region is

$$\dot{\tilde{c}}_t^M = \hat{\sigma}^{-1}(i_t^* - \pi_{H,t} - \rho) - \alpha(\omega-1)\dot{c}_t^* \tag{A.1}$$

$$\dot{\pi}_t^M = \rho\pi_t^M - \rho_\delta(\rho_\delta+\rho)\chi(\theta)\left[(\hat{\sigma}+\phi)\,\tilde{c}_t^M + \phi g_t^M + \hat{\sigma}\alpha(\omega-1)c_t^*\right]. \tag{A.2}$$

As in the main body of the text, I let $\mu \to 0$. Thus, coupled with the initial condition that $\tilde{c}_0^M = c_0^* = 0$, the path of region-wide consumption in response to region-wide government spending is, again, given by

$$\tilde{c}_t^M = \int_{-t}^{\infty} \alpha_s^{M,t} g_{t+s}^M ds. \tag{A.3}$$

To define $\{\alpha_s^{M,t}\}$, it is helpful to first calculate the eigenvalues of the region-wide system:

$$\nu^M = \frac{\rho - \sqrt{\rho^2 + 4\kappa_M \hat{\sigma}^{-1}}}{2}$$
$$\bar{\nu}^M = \frac{\rho + \sqrt{\rho^2 + 4\kappa_M \hat{\sigma}^{-1}}}{2},$$

where $\kappa_M \equiv \rho_\delta(\rho_\delta + \rho)\chi(\theta)(\hat{\sigma} + \phi)$

Then, because the region-wide system is isomorphic to the model presented in Farhi and Werning (2016), we have:

$$\alpha_s^{M,t} = \begin{cases} -\hat{\sigma}^{-1}\kappa_M \frac{\phi}{\hat{\sigma}+\phi} e^{-\nu^M s} \frac{1-e^{(\nu^M - \bar{\nu}^M)(t+s)}}{\bar{\nu}^M - \nu^M} & s < 0, \\ -\hat{\sigma}^{-1}\kappa_M \frac{\phi}{\hat{\sigma}+\phi} e^{-\nu^M s} \frac{1-e^{-(\bar{\nu}^M - \nu^M)t}}{\bar{\nu}^M - \nu^M} & s \geq 0. \end{cases} \tag{A.4}$$

State Multiplier Coefficients

The system of differential equations characterizing the path of consumption of home produced goods and home PPI in the home state is

$$\dot{\tilde{c}}_t = \hat{\sigma}^{-1}(i_t^* - \pi_{H,t} - \rho) - \alpha(\omega - 1)\dot{c}_t^* \tag{A.5}$$

$$\dot{\pi}_{H,t} = \rho\pi_{H,t} - \rho_\delta(\rho_\delta + \rho)(1-\theta)\left[\beta_{\tilde{c}}\tilde{c}_t + \beta_g g_t + \beta_{g^M} g_t^M + \beta_{c^*} c_t^* + \beta_{c^M}\tilde{c}_t^M\right]. \tag{A.6}$$

Coupled with the initial condition that $\tilde{c}_0 = c_0^* = 0$, the path of consumption in response to a particular path of state-level government spending, $\{g_t\}$, and region-wide government spending, $\{g_t^M\}$, is, again, given by

$$\tilde{c}_t = \int_{-t}^{\infty} \alpha_s^{d,t} g_{t+s} ds + \int_{-t}^{\infty} \alpha_s^{s,t} g_{t+s}^M ds + \int_{-t}^{\infty} \alpha_k^{c,t} \int_{-t}^{\infty} \alpha_s^{M,t} g_{t+s}^M ds dk, \tag{A.7}$$

with $\{\alpha_s^{M,t}\}$ defined above.

The coefficients $\{\alpha_s^{d,t}, \alpha_s^{s,t}, \alpha_k^{c,t}\}$ rely upon the eigenvalues of the system:

$$\nu = \frac{\rho - \sqrt{\rho^2 + 4\kappa\hat{\sigma}^{-1}}}{2}$$
$$\bar{\nu} = \frac{\rho + \sqrt{\rho^2 + 4\kappa\hat{\sigma}^{-1}}}{2},$$

where $\kappa \equiv \rho_\delta(\rho_\delta + \rho)(1-\theta)\beta_{\tilde{c}}$.[2]

[2]Observe that when $\theta = 0$ that $\kappa_M = \kappa$.

To determine the coefficients $\{\alpha_s^{d,t}, \alpha_s^{s,t}, \alpha_k^{c,t}\}$ it is helpful to define the function

$$f(\beta) = \begin{cases} -\hat{\sigma}^{-1}\kappa\frac{\beta}{\beta_{\tilde{c}}}e^{-\nu^M s}\frac{1-e^{(\nu-\bar{\nu})(t+s)}}{\bar{\nu}-\nu} & s < 0, \\ -\hat{\sigma}^{-1}\kappa\frac{\beta}{\beta_{\tilde{c}}}e^{-\nu s}\frac{1-e^{-(\bar{\nu}-\nu)t}}{\bar{\nu}-\nu} & s \geq 0. \end{cases} \tag{A.8}$$

Then, we have

$$\begin{aligned} \alpha_s^{d,t} &= f(\beta_g) \\ \alpha_s^{s,t} &= f(\beta_{g^M}) \\ \alpha_s^{c,t} &= f(\beta_{c^M}). \end{aligned}$$

Proof of Proposition 1

Proposition 2. *If $g_t = \kappa_g g_t^M$ for all $t \geq 0$ and $|\kappa_g| > 0$, then*

$$\mathcal{M}_{c,g} + \mathcal{M}_{c,g^M} = \mathcal{M}_{c^M,g^M}$$

Proof.

$$\begin{aligned} \mathcal{M}_{c,g} + \mathcal{M}_{c,g^M} &= \frac{\int_0^\infty \tilde{c}_s ds}{\int_0^\infty g_s ds} + \frac{\int_0^\infty \tilde{c}_s ds}{\int_0^\infty g_s^M ds} \\ &= \frac{\kappa_g \int_0^\infty \int_{-s}^\infty \alpha_u^{d,s} g_{s+u}^M duds}{\kappa_g \int_0^\infty g_s^M ds} + \frac{\int_0^\infty \tilde{c}_s ds}{\int_0^\infty g_s^M ds} \\ &= \frac{\int_0^\infty \left[\int_{-s}^\infty (\alpha_u^{d,s} + \alpha_u^{s,s}) g_{s+u}^M du + \int_{-s}^\infty \alpha_k^{c,s} \int_{-(s+k)}^\infty \alpha_u^{M,s+k} g_{s+k+u}^M dudk\right] ds}{\int_0^\infty g_s^M ds} \\ &= \frac{\int_0^\infty \tilde{c}_s^M ds}{\int_0^\infty g_s^M ds} = \mathcal{M}_{c^M,g^M} \end{aligned}$$

The first equality follows by definition. The second by assumption of proportionality and by definition of $\{\tilde{c}_t\}$. The third follows again by definition. The final equality follows by symmetry whenever the home state and the rest of the region have the same level of spending for all $t \geq 0$. □

A.3 Consumption Versus Value-Added Multipliers

To align the model with the notation used in Farhi and Werning (2016) I have opted to describe the dynamic system in terms of consumption of home state produced goods. However, given how state output measures are constructed, empirically I estimate local and spillover value-added relative multipliers.

This is without loss when adding together the local and spillover multipliers. To see this, write out the PPI-deflated value-added (wage income and profits):

$$VA_t \equiv \tilde{Y}_t - \frac{P_t^M X_t}{P_{H,t}} = \tilde{Y}_t - \theta MC_t \tilde{Y}_t \Delta_t,$$

where Δ_t is a term reflecting dispersion in prices in the home-state.[3] In log-deviations, this is

$$\check{va}_t = (1-\tilde{\theta})(\tilde{c}_t + g_t) + \tilde{\theta}\left(c_t^M + g_t^M + \frac{1}{1-\tilde{\theta}}\check{mc}_t^M + \gamma\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M)\right) - \frac{\tilde{\theta}}{1-\tilde{\theta}}\check{mc}_t, \tag{A.9}$$

where I've used the fact that $\check{x}_t^M = c_t^M + g_t^M + \frac{1}{1-\tilde{\theta}} mc_t^M$. Rearranging:

$$\begin{aligned}
\check{va}_t &= (1-\tilde{\theta})\tilde{c}_t + \tilde{\theta}c_t^M && \text{(A.10)}\\
&+ (1-\tilde{\theta})g_t + \tilde{\theta}g_t^M && \text{(A.11)}\\
&+ \tilde{\theta}\gamma\hat{\sigma}(\tilde{c}_t - \tilde{c}_t^M) && \text{(A.12)}\\
&+ \frac{\tilde{\theta}}{1-\tilde{\theta}}(\check{mc}_t^M - \check{mc}_t) && \text{(A.13)}
\end{aligned}$$

Define

$$\mathcal{M}_{va,g} \equiv \frac{\int_0^\infty \check{va}_s ds}{\int_0^\infty g_s ds} \quad \text{and} \quad \mathcal{M}_{va,g^M} \equiv \frac{\int_0^\infty \check{va}_s ds}{\int_0^\infty g_s^M ds}$$

Thus, when the path of government spending is proportional in the home state and in the region in which the home state is located for all t, by symmetry we have $\mathcal{M}_{va,g} + M_{va,g^M} = 1 + \mathcal{M}_{c^M,g^M}$.

[3] This is a slight abuse of notation in that this term will be proportional to the term defined in the main-body of the paper, implying in log-deviations they are the same and equal to zero.

Appendix B

Appendix to Chapter 3

B.1 Variants of Main Tables

Table B.1: Benchmark One and Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Gross State Product, Employment, and Unemployment
(State-Clustered Standard Errors)

	Bivariate	+ State FEs	+ Quarter FEs	+ Direct ARRA	All Controls
	b/se	b/se	b/se	b/se	b/se
8-Qtr Ahead	2.01***	2.03***	2.80**	2.82**	2.12*
Spill. ARRA	(0.38)	(0.39)	(1.33)	(1.31)	(1.21)
8-Qtr Ahead				2.30**	1.46
ARRA				(1.09)	(1.04)
No. Obs.	1764	1764	1764	1764	1764
R-Squared	0.018	0.236	0.456	0.461	0.474
State FEs	No	Yes	Yes	Yes	Yes
Quarter FEs	No	No	Yes	Yes	Yes
Output Lags	No	No	No	No	Yes

- Tables report heteroskedasticity consistent standard errors, clustered by state.
- The spillover and direct measure of ARRA spending (over the subsequent 8 quarters) is set to zero in quarters prior to 2009Q2.
- The controls in column (5) represent the benchmark specification.
- On average, each $1 of directly received ARRA aid is associated with $0.63 of import-weighted exposure. To convert to a spillover multiplier, multiply the coefficients in the top line by 0.63.
- The estimate in the first column differs slightly from that reported in the first column of Table 3.1 because, without state fixed effects, the random effects GLS estimator is invoked.

Table B.2: Benchmark One and Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Gross State Product, Employment, and Unemployment
(State-Clustered Standard Errors)

	4-Quarter Effect			8-Quarter Effect		
	Output	Job-Years	Unemployed -Years	Output	Job-Years	Unemployed -Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead	0.97	2.79**	-5.40***			
Spill. ARRA	(0.59)	(1.29)	(0.95)			
4-Qtr Ahead	0.27	3.53**	-2.35*			
ARRA	(0.53)	(1.61)	(1.34)			
8-Qtr Ahead				2.12*	10.54***	-12.61***
Spill. ARRA				(1.21)	(3.62)	(2.24)
8-Qtr Ahead				1.46	10.56**	-6.14*
ARRA				(1.04)	(5.08)	(3.43)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.417	0.722	0.799	0.474	0.698	0.823
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report heteroskedasticity consistent standard errors, clustered by state.
- The spillover and direct measure of ARRA spending (over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.
- On average, each $1 of directly received ARRA aid is associated with $0.63 of import-weighted exposure. To convert to a spillover multiplier, multiply the coefficients in the first and third lines by 0.63.

Table B.3: Benchmark One and Two Year Cumulative Exposure Multipliers of Recovery Act Spending (Less DOL) on Gross State Product, Employment, and Unemployment

	4-Quarter Effect			8-Quarter Effect		
	Output	Job-Years	Unemployed -Years	Output	Job-Years	Unemployed -Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead Spill. ARRA (Less DOL)	1.16***	4.13***	-6.78***			
	(0.16)	(0.96)	(1.19)			
4-Qtr Ahead ARRA (Less DOL)	0.35*	3.95***	-2.07			
	(0.17)	(0.91)	(1.46)			
8-Qtr Ahead Spill. ARRA (Less DOL)				2.52***	13.80***	-15.27***
				(0.31)	(2.10)	(2.64)
8-Qtr Ahead ARRA (Less DOL)				1.52***	9.02**	-7.90***
				(0.49)	(4.29)	(2.77)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.414	0.724	0.799	0.473	0.698	0.824
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (less DOL; over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.
- On average, each \$1 of directly received ARRA aid is associated with \$0.63 of import-weighted exposure. To convert to a spillover multiplier, multiply the coefficients in the first and third lines by 0.63.

B.2 Robustness Exercises

Outlier Assessment

In this subsection I assess whether my estimates are driven by any one state, which is a concern when analyzing outcomes at the state level. To do so, I sequentially select each state from the sample and re-estimate Equation (3.3) using the benchmark set of controls with gross state product as the outcome variable. Only one state is dropped at a time.

As an example, the benchmark two-year cumulative output spillover estimate is \$2.12. When excluding Washington D.C. from the sample, the point estimate rises slightly to \$2.36 (SE: 0.24).

Figure B.1: Outlier Analysis: Estimated 2-Year Cumulative Spillover Output Multiplier from Dropping Each State

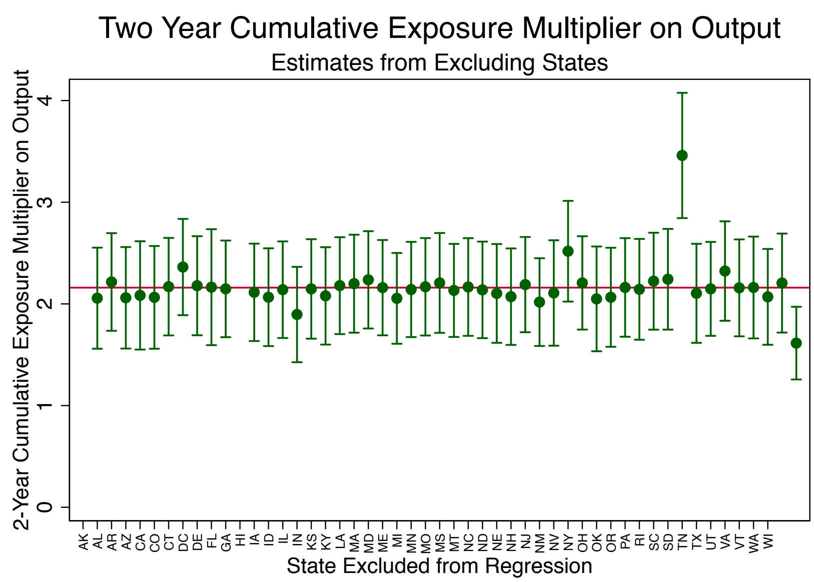


Figure B.1 reports the results from this exercise. The height of each bullet indicates the point estimate when excluding the given state from the sample. 90% confidence intervals are drawn around each bullet point.[1] The solid, red horizontal line indicates the benchmark point estimate of 2.12.

There are two takeaways from this exercise. First, in the vast majority of cases, dropping a state from the analysis does not matter: the point estimates cluster around the 2.12. Second, in only two cases does the point estimate change by more than one standard deviation relative to the benchmark: dropping Tennessee and Wyoming. When dropping Tennessee,

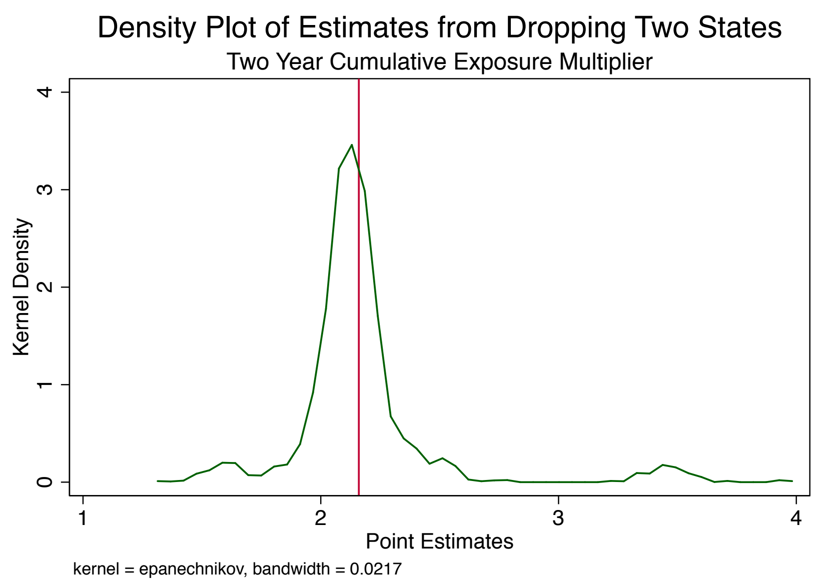
[1]Since Alaska and Hawaii are dropped from the analysis to begin with, there is no bullet point for these states.

the point estimate rises to 3.46 (SE: 0.31) but dropping Wyoming produces a point lower point estimate relative to the benchmark: 1.61 (SE: 0.18).

Multiplying by the scaling factor of 0.63 discussed above in Section 3.3 yields a range of estimates of the increase in output in all other states for every $1 of Recovery Act aid dispersed to a particular state: between 1.01 and 2.17. Although there is a considerable range in the implied output effect, the finding that fiscal policy has quantitatively large spillover effects is not driven by the experience of any particular state.

To allay any further concerns of this nature, I repeat the state exclusion exercise as described above, except that instead of dropping only a single state I drop two states at once from the analysis. Figure B.2 provides the kernel density plot of the estimated coefficients. As expected, the vast majority of the estimates are close to the benchmark estimate of 2.12.

Figure B.2: Outlier Analysis: Estimated 2-Year Cumulative Spillover Output Multiplier from Dropping Combinations of Two States



Including Own-Share in Weight Matrix

Motivated by the findings in Hillberry and Hummels (2003) that many shipments within state are between wholesalers and retailers, I do not include own-shipments in the calculation of the spillover exposure variable. The reason for this was to focus on a consistently defined measure of exposure to interventions in other states that are mediated by the trade in intermediate goods. However, by setting w_{ii} equal to zero I am implicitly forcing my estimates of the direct effect to include indirect own-state effects mediated through the trade channel studied above.

If the within-state effect is comparable to the cross-state effect, then including the own-share spillover in the construction of $ARRA^S_{i,t}$ should not alter my baseline findings. In particular, in what follows I set w_{ii} equal to the share of reported within-state shipments among all reported inbound shipments from the CFS. I then include $w_{ii} \times ARRA^D_{i,t}$ in the construction of $ARRA^S_{i,t}$, as implied by (3.1).

The following table reports the cumulative 2-year exposure multiplier when the own-share spillover effect is included. Since every $1 of direct ARRA is, in this analysis, associated with $1 of spillover aid, there is no need to rescale the coefficients as I did above. Looking at column six of Table B.4, we see that, all else equal, every $1 of direct aid led to $1.94 (SE: 0.33) of increased output over two years. This is quantitatively similar to our benchmark (rescaled) finding of $1.33. Under this specification, one cannot reject the null that $1.33 is the true effect.

Perhaps unsurprisingly, the coefficient on directly allocated ARRA obligations falls from $1.46 to $1.07 (SE: 0.41), suggesting that the direct effect estimated in Table 3.2 in part captures the indirect, local effect mediated by trade within the state.

Table B.4: One and Two Year Cumulative Exposure Multiplier of Recovery Act Spending—Self-Share Weight Included

	4-Quarter Effect			8-Quarter Effect		
	Output	Job-Years	Unemployed -Years	Output	Job-Years	Unemployed -Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead	0.86***	2.34***	-4.93***			
Spill. ARRA (Self-Share)	(0.16)	(0.41)	(1.10)			
4-Qtr Ahead	0.09	3.09***	-1.37			
ARRA	(0.29)	(0.65)	(1.60)			
8-Qtr Ahead				1.94***	8.76***	-12.74***
Spill. ARRA (Self-Share)				(0.33)	(1.36)	(2.40)
8-Qtr Ahead				1.07**	8.71***	-3.72
ARRA				(0.41)	(1.67)	(2.41)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.417	0.721	0.801	0.475	0.696	0.826
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.

Excess Cyclicality

In this subsection I assess the concern that states disproportionately exposed to spending elsewhere in the country through the trade in goods exhibit greater co-movement with the aggregate business cycle. States with business cycles that tend to co-move more strongly with the aggregate business cycle may have exhibited both a deeper decline in the early stages of the downturn relative to other states and a relatively stronger recovery in the years following the passage of the Recovery Act in exactly the pattern documented for the high and low spillover states described above in Section 3.3. If this is indeed the case, then my benchmark estimates would be upwardly biased, tending to overstate the spillover effects of the Recovery Act.

In what follows, I present evidence that this concern has some legitimacy: those states that were disproportionately exposed to spending elsehwere in the country tend to have business cycles that co-move more with the aggregate business cycle. However, when controlling for this co-movement directly in Equation (3.3), I find that my benchmark estimates are quantitatively unchanged, even though the co-movement regressor is significantly—statistically and quantitatively—correlated with accumulated changes in output, employment, and unemployment.

Using data from the BEA, I calculate annual real output growth rates for every state and the nation between 1977 and 2008. For each state, I then separately estimate the following regression:

$$\Delta ln(GSP_{i,t}) = \alpha_i + \psi_i \Delta ln(GDP_t) + \epsilon_{i,t} \tag{B.1}$$

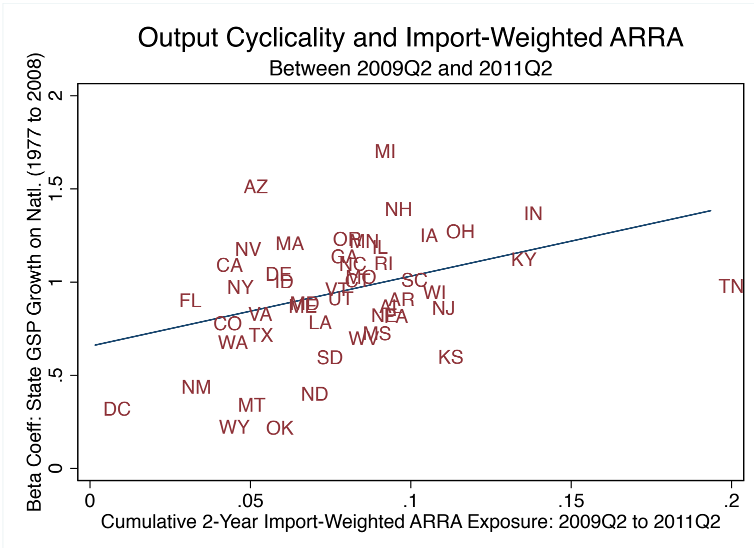
States with larger estimates of $\hat{\psi}_i$ tend to load more heavily on the aggregate business cycle.

Figure B.3 below reports the scatter plot of $\{\hat{\psi}_i\}$ against $\frac{ARRA^S_{i,2011Q2}}{GSP_{2009Q1}}$, which is the cumulative value of import-weighted ARRA obligations to which a state was exposed between 2009Q2 and 2011Q2, relative to GSP in 2009Q1. As can be seen from this figure, there is an upward sloping relationship between state-level output cyclicality and a state's import-weighted ARRA exposure in 2009Q2.

This exercise suggests that the relatively faster recovery among states differentially exposed to spending elsewhere in the country might simply be attributable to the national economic recovery that began in the latter half of 2009. If the national economy would have recovered during this time for reasons unrelated to the Recovery Act, then my estimates of the spillover effects are biased upwards, if not spurious altogether.

To directly address the concern that my results are driven solely by differential loadings on the business cycle, I interact the K-quarter ahead cumulative change in aggregate real GDP with the estimated coefficient $\hat{\psi}_i$:

Figure B.3: Scatter Plot of Output Growth Volatility and Cumulative ARRA Exposure Over 2 Years Following Passage of Recovery Act



$$C_{i,t}^{K} \equiv \left(\frac{\sum_{h=0}^{K} GDP_{t+h} - GDP_{t-1}}{GDP_{t-1}} \right) \hat{\psi}_i$$

I then estimate Equation 3.3, including $C_{i,t}^{K}$ as an additional regressor for the output, employment, and unemployment specifications. For the output specifications, the estimated coefficient on $C_{i,t}^{K}$ should be close to one. If the accumulated change in output over K quarters is entirely attributable to movements in aggregate output, the coefficient of interest $\phi_K^{S,Y}$, the spillover exposure effect, should be close to and statistically indistinguishable from zero.

The results of this exercise are reported in Table B.5. As before, in the first three columns I report estimated cumulative effects on output, employment, and unemployment over one year; in the final three columns I report the estimated cumulative effects over two years.

There are two takeaways from this exercise. First, at both the one year and two year horizon, the estimates of the spillover exposure effect on output, employment, and unemployment are quantitatively similar to my benchmark results. For example, after controlling for excess cyclicality, the cumulative two-year output effect of being exposed to one additional dollar elsewhere in the country is \$1.71 (SE: 0.32) additional dollars of output. This is similar to and statistically indistinguishable from the benchmark estimate of \$2.12. Second, in both output specifications, the coefficient on $C_{i,t}^{K}$ is close to and statistically indistinguishable from the null value of one.

In sum, this subsection shows my estimates of the spillover effects of the Recovery Act are robust to controlling for each state's excess sensitivity to the aggregate business cycle.

Table B.5: One and Two Year Cumulative Exposure Multiplier of Recovery Act Spending—Excess Cyclicality Interaction

	4-Quarter Effect			8-Quarter Effect		
	Output	Job-Years	Unemployed -Years	Output	Job-Years	Unemployed -Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead	0.88***	2.62***	-4.71***			
Spill. ARRA	(0.13)	(0.45)	(0.58)			
4-Qtr Ahead	0.24	3.43***	-2.05			
ARRA	(0.25)	(0.73)	(1.37)			
8-Qtr Ahead				1.71***	8.93***	-10.06***
Spill. ARRA				(0.32)	(1.20)	(1.38)
8-Qtr Ahead				1.44***	10.35***	-5.73***
ARRA				(0.43)	(2.20)	(2.05)
K-Qtr GDP	1.06***	2.79***	-4.34***	1.16***	5.31***	-5.18***
Interaction	(0.21)	(0.69)	(0.34)	(0.20)	(1.04)	(0.50)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.451	0.728	0.821	0.518	0.716	0.851
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.

It is not the case that states highly exposed to spending elsewhere exhibited relatively faster recoveries simply because they tend to co-move more strongly with the aggregate economy, which began recovering in the latter half of 2009.

Export Weight Matrix

In this section I investigate whether there is evidence that the tradable spillovers of fiscal policy estimated above also propagate through an export channel in addition to an import channel. In particular, I construct a different measure of exposure to spending elsewhere in the country by using the transpose of $\mathbf{W}$ as the weight matrix. Specifically, I calculate

$$ARRA_t^{\tilde{S}} = \mathbf{W}' \times ARRA_t^D$$

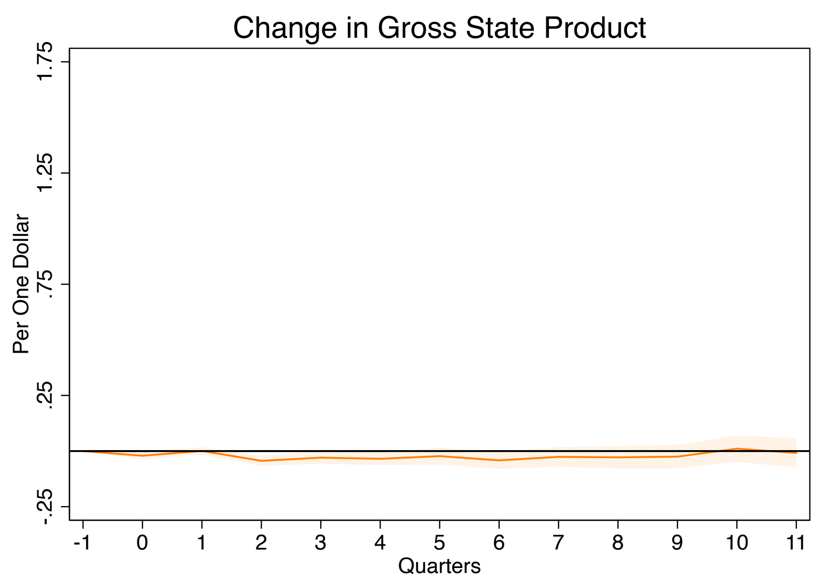
Again, each state's exposure is a weighted sum of spending in all other states, now given by:

$$ARRA^{\tilde{S}}_{i,t} = \sum_{j \neq i} w_{j,i} ARRA^{D}_{j,t}$$

Recall that $w_{j,i}$ has the equivalent interpretation as the share of goods exported by state j to state i as as share of all goods imported by state i. Values close to one would indicate that exports from state j represent a large share of goods imported by state i.

Figure B.4 reports the impulse response of output estimated according to Equation (3.2), where the only change is replacing $ARRA^{S}_{i,t}$ variables with $ARRA^{\tilde{S}}_{i,t}$. At all horizons, an innovation to export-weighted exposure has no impact on relative output growth. As discussed in the introduction, this is consistent with the predictions of the stylized production network model presented in Acemoglu, Akcigit, and Kerr (2016).

Figure B.4: Placebo Test: Estimated IRF for Change in Gross State Product for every $1 of Export-Weighted ARRA Spending



Weighting by Population

In this section, I estimate the benchmark cumulative specifications in equation (3.3), except that I weight by state population at the beginning of my sample to address concerns that my results are not nationally representative. Ignoring for a moment the common effects of the Recovery Act that effect all states symmetrically, if small states tend to have large local

[2] This concern is raised in Ramey (2019) when discussing the relation between local multiplier estimates and the aggregate multiplier that macroeconomists are interested in estimating.

multiplier effects (either direct or spillover), the unweighted regression will tend to overstate the aggregate multiplier[2]

Table B.6 reports the results of this exercise. Focusing first on the fourth column, the two year cumulative output effect from an additional \$1 of spillover exposure is \$1.29 (SE: 0.28). This estimate is lower than the unweighted result in which the spillover exposure effect was an additional \$2.12 over two years for each \$1 of exposure. Larger states are thus less effected by spending elsewhere in the country through the trade in intermediate goods.

Supposing that the spillover exposure effect is monotonically declining in the size of the state, as measured by population, a lower bound on how much each \$1 of local spending increased output elsewhere can be calculated using the scaling factor of 0.63. This lower bound is \$0.81 (SE: 0.17).

While the spillover exposure effect is smaller for larger states, the estimated direct effect increases. Over two years, each \$1 of local ARRA spending increased cumulative output by \$2.50 (SE: 0.38). This result likely stems from the fact that larger states tend to source a larger share of their intermediate goods from within their own state. For example, the share of goods reported as sourced by California in the CFS from other states is approximately 0.3 (see 3.1).

Moving to the final two columns, both the employment and the unemployment spillover exposure effects are smaller relative to the benchmark estimates in Table 3.2, in line with the results for output. As with output, the direct effect on output rises considerably such that the fall in unemployment over two years for each \$1 million of Recovery Act aid was 18 unemployed years.

The employment estimate falls relative to the benchmark, which parallels the findings in Ramey (2019), where the local employment multiplier falls when weighting by population; however, the standard errors on the direct employment effect rise considerably, such that one is unable to reject a direct employment effect of 10 job-years created or saved for each \$1 million of locally received ARRA aid.

Leontief Inverse Specification

In my benchmark specification I only incorporate the first order connection between states as implied by trade flows reported in the CFS between U.S. states. A natural question to ask is whether my results differ when explicitly incorporating higher order linkages between states that arise as the fiscal shock propagates upstream from states directly receiving fiscal stimulus to their upstream trading partners, to their upstream trading partners, and so on.

Let θ be the cost-share of intermediates in firm production with elements in $\mathbf{W}$ representing the share of intermediate goods sourced by state j from state i. Moroever, suppose that labor is the only other factor of production, with cost-share $(1-\theta)$. Under Cobb-Douglas production, with prices held fixed, and the wage as the numeraire, each additional nominal unit of output produced requires employing $(1-\theta)$ labor locally and purchasing θ nominal units of intermediate goods, split across regions according to the elements in $\mathbf{W}$.

If the effect on final, state-level output is proportional to local labor employed, both to satisfy direct government demand and to meet indirect government demand through the

Table B.6: One and Two Year Cumulative Exposure Multiplier of Recovery Act Spending—Weighted by Population at Beginning of Sample

	4-Quarter Effect			8-Quarter Effect		
	Output b/se	Job-Years b/se	Unemployed -Years b/se	Output b/se	Job-Years b/se	Unemployed -Years b/se
4-Qtr Ahead Spill. ARRA	0.40*** (0.14)	2.82*** (0.70)	-4.70*** (0.97)			
4-Qtr Ahead ARRA	0.96*** (0.16)	2.10 (1.61)	-6.27*** (2.18)			
8-Qtr Ahead Spill. ARRA				1.29*** (0.28)	9.91*** (1.76)	-11.26*** (2.10)
8-Qtr Ahead ARRA				2.50*** (0.38)	5.40 (5.35)	-18.13*** (4.16)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.545	0.773	0.845	0.604	0.760	0.872
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.

trade in intermediate goods, then this change in output, in partial equilibrium, may be written as

$$\begin{aligned} dy_t &= \tilde{\beta}_d(1-\theta)dg_t + \tilde{\beta}_N\left[(1-\theta)\theta W + (1-\theta)\theta^2 W^2 + \dots\right] dg_t \\ &= \tilde{\beta}_d(1-\theta)dg_t + \tilde{\beta}_N(1-\theta)\theta W[I-\theta W]^{-1}dg_t \\ &= \beta_d dg_t + \beta_N \theta W[I-\theta W]^{-1}dg_t \end{aligned} \tag{B.2}$$

where $\tilde{\beta}_d$ represents the direct effect of higher labor demand that is required to furnish the government with the goods and services it has purchased and $\tilde{\beta}_N$ represents the indirect effect of increased labor demand originating through the regional production network. In the final equation I absorb the $(1-\theta)$ terms into the coefficients β_d and β_N to simplify

[3] A Long and Plosser (1983) style production network would be one way to rationalize Equation B.2. See, for example, Proposition 1 in Acemoglu, Akcigit, and Kerr (2016) (and equation (A10)), where there is an additional "resource constraint" effect of government spending which leaves fewer resources for households to consume (today or in the future) through taxation needed to finance spending.

the interpretation. They represent, respectively, the direct change in output arising from increasing government demand for locally produced goods and the indirect change in output arising from increasing government demand for goods elsewhere in the country.[3]

I estimate the empirical analog to Equation (B.2) by first calculating the matrix $\mathbf{W}_L \equiv \theta\mathbf{W}[I-\theta\mathbf{W}]^{-1}$. I set $\theta = 0.44$ to be consistent with the share of intermediate inputs relative to gross production in the years prior to the Great Recession. Then, I construct a new spillover exposure measure

$$ARRA^{S,L}_{i,t} \equiv \mathbf{W}_L \times ARRA_t$$

and re-estimate Equation (3.3), replacing $ARRA^{S}_{i,t+h}$ with $ARRA^{S,L}_{i,t+h}$. The results of this exercise are reported in Table B.7.

At both the one and two year horizon, for both the direct and indirect effects of Recovery Act aid, the cumulative effect on output, employment, and unemployment is quantitatively similar to the benchmark results reported in Table 3.2. Focusing on the two year cumulative effect on output, each \$1 of directly received aid over a two year period is estimated to increase output by \$1.32 (SE: 0.42). For comparison, the comparable estimate in Table 3.2 is 1.46 (SE: 0.43).

Turning to the spillover effects, I find that each additional \$1 of exposure to spending elsewhere in the country, as implied by $\mathbf{W}_L$, increased output by \$2.15 (SE: 0.34). For comparison, the benchmark spillover estimate is \$2.12 (SE: 0.25). This suggests that the higher order linkages, and in turn spillover exposure, between states are well-approximated by using only the first order linkages as implied by $\mathbf{W}$.

Since this exercise uses a different weighting matrix than in the baseline specification, one needs to again rescale the point estimate on the spillover exposure variable. The column sums of $\mathbf{W}_L$ are all essentially equal to 0.785. Thus, by construction each one dollar of directly received Recovery Act aid is associated with 0.785 dollars of spillover exposure.

Multiplying the spillover output effect by 0.785, one would conclude that each \$1 of ARRA aid received over two years increased output elsewhere in the country over two years by \$1.68 (SE: 0.26), a point estimate somewhat elevated relative to my baseline findings but otherwise quantitatively similar. Performing a similar exercise with the labor market variables, using $\mathbf{W}_L$ to construct the spillover exposure variable I find that over two years each one million dollars of direct Recovery Act aid increased employment elsewhere by 7.63 (SE: 1.03) job-years and lowered unemployment by 10.69 (SE: 1.95) unemployed years.

Combining both the direct and the indirect effects, each \$1 of Recovery Act aid over two years increased cumulative output by \$3 (SE: 0.56) over two years. Absent other offsetting forces, the aggregate fiscal multiplier is again estimated as having a rough lower bound of approximately 3.

Event Study Specification

In this subsection I investigate the identifying assumption that the spatial distribution of spillover ARRA funding was orthogonal to potential growth in the quarters following the passage of the act. To do so, I restrict my use of the data in the following way. First, I

Table B.7: One and Two Year Cumulative Exposure Multiplier of Recovery Act Spending—Weighted by Population at Beginning of Sample

	4-Quarter Effect			8-Quarter Effect		
	Output b/se	Job-Years b/se	Unemployed -Years b/se	Output b/se	Job-Years b/se	Unemployed -Years b/se
4-Qtr Ahead	0.92***	2.50***	-5.25***			
Spill. ARRA	(0.17)	(0.40)	(1.13)			
4-Qtr Ahead	0.21	3.40***	-1.99			
ARRA	(0.28)	(0.67)	(1.65)			
8-Qtr Ahead				2.15***	9.73***	-13.62***
Spill. ARRA				(0.34)	(1.31)	(2.48)
8-Qtr Ahead				1.32***	9.82***	-5.32**
ARRA				(0.42)	(1.79)	(2.47)
No. Obs.	1764	1764	1764	1764	1764	1764
R-Squared	0.418	0.722	0.800	0.475	0.696	0.826
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Variables	Yes	Yes	Yes	Yes	Yes	Yes

- Tables report Driscoll and Kraay (1998) standard errors, which are robust to general forms of spatial and temporal dependence.
- The spillover and direct measure of ARRA spending (over the subsequent 4 and 8 quarters) is set to zero in quarters prior to 2009Q2.

assume that at the passage of the act (2009Q1) the eventual distribution of ARRA funding to the states was known by all agents in the economy—households, firms, etc. In this sense, the spillover exposure each state experienced, as a result of their trade with the rest of the country, occurred in a single period, the quarter of the passage of the act.

This restriction implies that the effects I estimate exploit *only* the cross-sectional variation in exposure. Indeed, it would be inappropriate to use the temporal variation in the spillover treatment if households and firms knew at the passage of the act how the future ARRA spending in the rest of the country would affect them and adjusted their behavior in response.[4] By collapsing the spillover exposure to a single date, I am able to investigate how economic conditions varied in the quarters prior to and following the passage of the act.

First, I estimate an analog to an event-study specification:

[4] Ramey (2011) presents evidence that incorrectly measuring the news shock of future government spending shocks matters for correctly estimating the consumption effects of fiscal policy and, in turn, the overall multiplier.

$$\frac{GSP_{i,t} - GSP_{i,t-1}}{GSP_{i,t-1}} = \sum_{s=-12}^{12} \chi_s \mathbf{1}(t = 2009Q2 + s) \frac{ARRA_i^S}{GSP_{i,t-1}} + \theta_i + \eta_t + \epsilon_{i,t}$$

This specification includes time fixed effects, η_t, as well as state fixed effects, θ_i. $ARRA_i^S$ does not have a time t subscript because it represents the cumulative value of ARRA spending to which a state was exposed according to the weight matrix $\mathbf{W}$ constructed from the CFS.

It is useful to point out two key differences between this specification and standard event-study designs: First, an event-study analysis is typically used in scenarios in which different observational units have different unit-specific event times. In this specification, I assume the event-time is the same for every state: 2009Q2. In this sense, the interaction coefficients, $\{\chi_s\}$, provide estimates of the correlation between output growth and spillover exposure in the quarters prior to and following 2009Q2.

The second obvious difference between the standard event-study specification and what I consider here is that the "treatment" variable, $\frac{ARRA_i^S}{GSP_{i,t-1}}$, is a continuous measure of treatment. Unlike standard event-studies, this specification imposes parametric restrictions on the interaction terms—namely, linearity.

The results of this exercise are provided in Figure B.5. Here I have accumulated the coefficients, χ_s, around 2009Q1 to convert the results to level differences. For example, the coefficient at $2010Q1$ is equal to 0.25, which indicates that each additional \$1 of spillover ARRA exposure was associated with \$0.25 additional output in the first quarter of 2010, relative to the level of its output in the first quarter in 2009. The shaded areas indicate 90% confidence intervals using Driscoll and Kraay (1998) standard errors. For comparison, I have also included 90% confidence intervals using cluster-robust standard errors.

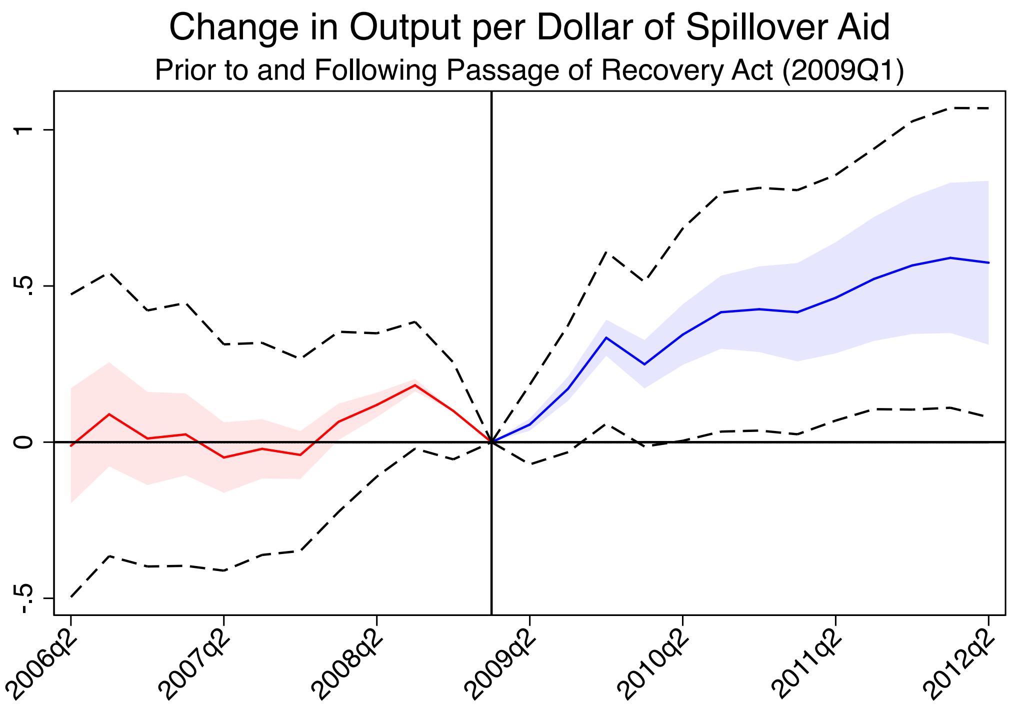
There are three observations to make about this plot: first, prior to 2007Q4, more and less exposed states appear to have been on similar growth trajectories, indicated by the near-zero and statistically insignificant values from 2006Q2 to 2007Q4.

Second, more highly exposed states appear to have been less affected *initially* by the onset of the Great Recession. The estimated growth rates between 2007Q4 and 2008Q3 are positive; however, these states also experienced a similarly sized relative economic decline in the two quarters prior to 2009Q1, as evinced by the negative growth rates implied by the figure.[5] Thus, at the time of the passage of the Recovery Act, more highly exposed states to ARRA spending elsewhere were contracting economic production at a faster rate.

Third, following the passage of the Recovery Act, states exposed to higher levels of ARRA spending elsewhere had a faster and sustained expansion of production from 2009Q2 onwards. One can calculate a two-year cumulative exposure multiplier from this figure by accumulating the coefficients from 2009Q2 to 2011Q2. The cumulative multiplier from this analysis is equal to \$2.65, indicating that, over two years, output in a state exposed to an additional \$1 increased by \$2.65. Multiplying by 0.63 again yields the implied 2-year

[5]This pattern of relatively faster growth in the early quarters of the recession alongside a more severe contraction just prior to the passage of the Recovery Act is also quite apparent in Figure 3.4.

Figure B.5: Pre-Post Specification: Change in Gross State Product 12 quarters before and after 2009Q1



- The solid line is constructed from the coefficients $\hat{\chi}_s$, accumulated so as to represent the level of output relative to the level as of 2009Q1.
- The shaded areas represent 90% confidence intervals, which are based on the Driscoll and Kraay (1998) methodology, which allows for general forms of spatial and temporal correlation of the error terms.
- The dashed lines represent 90% confidence intervals based on heteroskedasticity consistent standard errors, clustered by state.

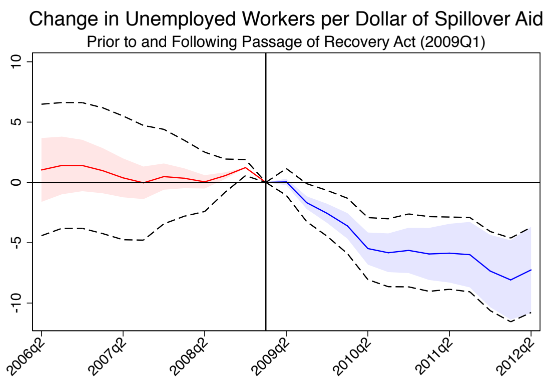
cumulative multiplier on output in other states for each $1 of Recovery Act allocated to a given state. This implied multiplier is 1.67, consistent with the baseline findings above.

In Figure B.6, I repeat the exercise for the labor market variables. Figure B.6a presents the results for unemployment. In the twelve quarters prior to the passage of the act, relative unemployment among states highly exposed to spending elsewhere through the Recovery Act was close to and, for the majority of quarters, statistically indistinguishable from zero. Following the passage of the Recovery Act, highly exposed states see a rapid and sustained decline in unemployment relative to less exposed states.

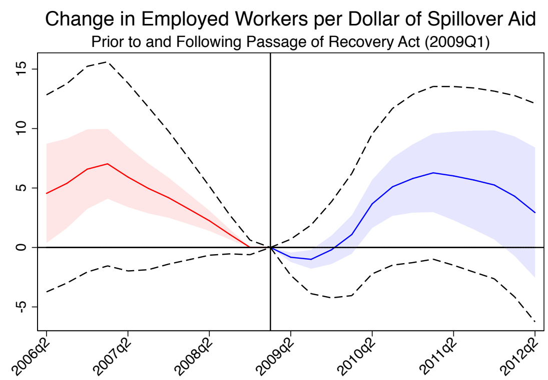
This pattern of a sharp relative response is replicated for employment, with the results presented in Figure B.6b. However, in this figure there is clear evidence of a downward pre-trend in employment among relatively highly exposed states. Nevertheless, there is a stark trend-break in employment growth at the passage of the Recovery Act. By the close of 2010, relative trend employment growth appears to have return to its pre-recession rate.

Figure B.6: Pre-Post Specification: Change in Unemployment and Employment 12 quarters before and after 2009Q1

(a)



(b)



- The solid line is constructed from the coefficients $\hat{\chi}_s$, accumulated so as to represent the level of unemployment/employment relative to the level as of 2009Q1.
- The shaded areas represent 90% confidence intervals, which are based on the Driscoll and Kraay (1998) methodology, which allows for general forms of spatial and temporal correlation of the error terms.
- The dashed lines represent 90% confidence intervals based on heteroskedasticity consistent standard errors, clustered by state.

B.3 Additional Tables

Correlating Spillover Exposure with Initial Downturn

In principle, the sum of the elements of the $\mathbf{w}_i$ vector, defined in Section 3.2, can range anywhere from zero to 48 if every state imported all commodities from a single state. In practice, the smallest sum is equal to 0.005 (Washington DC) and the largest sum is equal to 2.449 (California).

Table B.8 collects these values in the first column. One way of interpreting these values is to consider the following hypothetical. Suppose that every state in the country imported one dollar's worth of commodities from other states in exact proportion to the import weights constructed using the CFS data. For a given state, say Massachusetts, this statistic specifies the value of commodities imported from Massachusetts as a result of increasing imports in all other states by one dollar. The sum of elements in $\mathbf{w}_{Massachusetts}$ is approximately 1.

Thus, in this one-dollar counterfactual, imports from Massachusetts would increase by approximately \$1. Intuitively, this statistic is a measure of the centrality of each state to the

[6] This statistic is also known as the weighted out-degree of the directed, weighted graph of U.S. states as nodes and import shares as the weighted edges.

regional import/export network. Higher values imply that those states play a more central role in the regional production network.[6]

Now, one might be concerned that states that tend to ship more goods to other states (e.g. California, Texas, Illinois) were disproportionately exposed the economic downturn. The second column of Table B.8 reports the change in the unemployment rate for every state between the onset of the recession (2007Q4) and the quarter in which the Recovery Act was passed (2009Q1). This statistic measures, to some degree, the pre-Recovery Act severity of the economic downturn in each state. A strongly positive correlation between the one-dollar counterfactual statistic and the change in the unemployment rate would be troubling, suggesting that the distribution of spending intentionally or unintentionally targed worse-off states.

The raw correlation between state-level unemployment changes and this one-dollar statistic is 0.18, suggesting that the severity of the downturn was only weakly associated with the centrality of the state in the state import/export network.[7]

Of course, the geographic allocation of Recovery Act aid was not uniform, as in the one-dollar counterfactual scenario. Since the bulk of obligations were designated by the end of 2009Q2 (see Figure 3.2), we can compare the geographic distribution of obligations in this quarter to the change in the unemployment rate in the quarters preceding the passage of the ARRA. In the third column of Table B.8, I report $\frac{ARRA^S_{j,2009Q2}}{GSP_{j,2009Q1}}$, the value of import-weighted obligations to which each state was exposed relative to its own output in the prior quarter.

Although California tops the list as the most central state in terms of the CFS import/export network, it ranked 44 in terms of its import-weighted obligations exposure in 2009Q2, relative to output. With the possibility for such large rank-reversals, one might be concerned that the geographic allocation of Recovery Act aid, coupled with the weight matrix $\mathbf{W}$, induced exposure that was inadvertently correlated with the severity of the local downturn, either positively or negatively. I find that the correlation between the change in the unemployment rate and the value of import-weighted obligations relative to output in 2009Q1 was similar as before: 0.20.[8]

[7]Alternatively, one can instead calculate the eigenvector measure of centrality of the weighted, directed graph, $\mathbf{W}$. The correlation between the one-dollar hypothetical value and the eigenvector centrality is high; unsurprisingly, the correlation between the change in unemployment and the eignvector centrality is 0.18. See Jackson (2010) for additional information related to the eigenvector measure of centrality.

[8]If, instead, one looks at the entire value of import-weighted obligations to which a state was eventually exposed, this correlation drops further to approximately 0.06.

Table B.8: Dollar Counterfactual Exercise

	One-Dollar Conterfactual	Change UR: 2007Q4 - 2009Q1	Spillover ARRA: 2009Q2	Eigenvector Centrality
California	2.533	4.6	0.013	0.304
Texas	2.040	2.2	0.017	0.229
Illinois	1.783	4.1	0.031	0.218
Pennsylvania	1.630	3.1	0.031	0.227
New York	1.571	3.1	0.014	0.208
Ohio	1.510	4.3	0.038	0.197
Tennessee	1.420	5.0	0.069	0.201
New Jersey	1.245	3.9	0.036	0.198
Massachusetts	1.030	3.2	0.020	0.163
Indiana	0.962	5.4	0.047	0.154
North Carolina	0.920	5.4	0.027	0.136
Michigan	0.857	5.8	0.031	0.135
Minnesota	0.850	3.0	0.028	0.118
Georgia	0.845	4.5	0.026	0.128
Wisconsin	0.738	3.4	0.036	0.131
Maryland	0.714	3.4	0.023	0.189
Virginia	0.675	2.9	0.017	0.145
Missouri	0.650	3.5	0.028	0.124
Kentucky	0.618	4.7	0.046	0.134
Connecticut	0.571	2.6	0.027	0.129
Florida	0.544	5.1	0.009	0.103
Iowa	0.534	2.7	0.036	0.102
Washington	0.532	3.1	0.013	0.109
Kansas	0.482	2.2	0.039	0.100
Alabama	0.452	6.0	0.032	0.103
Utah	0.447	4.2	0.025	0.118
Colorado	0.432	2.9	0.013	0.110
Louisiana	0.420	2.5	0.024	0.089
Oregon	0.405	6.3	0.025	0.097
South Carolina	0.397	5.4	0.033	0.112
Arizona	0.372	5.0	0.016	0.121
Nebraska	0.329	1.5	0.031	0.096
Oklahoma	0.297	2.3	0.019	0.103
Arkansas	0.283	2.5	0.033	0.113
Mississippi	0.237	3.3	0.030	0.105
New Hampshire	0.200	2.6	0.031	0.127
Nevada	0.169	5.4	0.015	0.152
Maine	0.141	3.2	0.021	0.105
South Dakota	0.133	2.2	0.025	0.084
Idaho	0.130	4.5	0.019	0.091
Montana	0.129	2.7	0.016	0.106
West Virginia	0.124	2.4	0.027	0.121
Rhode Island	0.109	4.8	0.030	0.125
North Dakota	0.099	1.1	0.023	0.091
Wyoming	0.086	2.8	0.014	0.073
Vermont	0.063	2.6	0.025	0.111
Delaware	0.063	4.2	0.017	0.141
New Mexico	0.055	3.3	0.009	0.108
District of Columbia	0.005	2.9	0.001	0.147
N	49			

- The one-dollar counterfactual indicates the value of goods shipped from each state if each state were to import one dollar's worth of goods according to the import weights constructed in the baseline model. The second column provides the change in the unemployment rate for each state between 2007Q4 and 2009Q1. The correlation between these two statistics is 0.18. The correlation between trade-weighted spillover ARRA funds received in 2009Q2 and the change in the unemployment rate is 0.20.

Summary Statistics of Key Variables in Empirical Analysis

Table B.9: Summary Statistics of Variables as of 2009Q1 for States Included in the Benchmark Analysise

	Min	Mean	Median	Max	SD
GSP Change (4-Qtr Ahead)	-0.0955	0.0096	0.0125	0.0730	0.0274
Cumulative GSP Change (4-Qtr Ahead)	-0.3920	0.0138	0.0291	0.2406	0.0893
GSP Change (8-Qtr Ahead)	-0.1011	0.0293	0.0284	0.1698	0.0427
Cumulative GSP Change (8-Qtr Ahead)	-0.7599	0.1265	0.1326	0.6159	0.2153
Employment Change (4-Qtr Ahead)	-1.5429	-0.5418	-0.4660	0.8979	0.5132
Cumulative Employment Change (4-Qtr Ahead)	-1.3160	-0.5208	-0.4941	0.2599	0.3303
Employment Change (8-Qtr Ahead)	-1.6791	-0.0930	-0.2658	2.3119	0.8230
Cumulative Employment Change (8-Qtr Ahead)	-2.5473	-0.7433	-0.8286	2.0642	0.9340
Unemployment Change (4-Qtr Ahead)	-0.3397	0.3318	0.3239	1.2871	0.3283
Cumulative Unemployment Change (4-Qtr Ahead)	-0.2100	0.3234	0.3026	1.0098	0.2564
Unemployment Change (8-Qtr Ahead)	-1.6661	0.0000	-0.0263	1.2979	0.4934
Cumulative Unemployment Change (8-Qtr Ahead)	-0.8425	0.4417	0.4270	2.2306	0.6291
Cumulative Spill. ARRA (4-Qtr Ahead)	0.0013	0.0576	0.0570	0.1548	0.0259
Cumulative Spill. ARRA (8-Qtr Ahead)	0.0016	0.0719	0.0725	0.1934	0.0323
Cumulative ARRA (4-Qtr Ahead)	0.0597	0.1011	0.0995	0.1565	0.0218
Cumulative ARRA (8-Qtr Ahead)	0.0741	0.1269	0.1246	0.2140	0.0283
N	49				

- All variables are per million, relative to lagged Gross State Product
- Accumulated employment and unemployment statistics annualized by dividing through by 4.

Appendix C

Appendix to Chapter 4

C.1 Additional Empirical Results

Panel Specification

One concern with the cross-sectional specifications is that there may be some unobserved aggregate factor that induced large increases in UI claims at the same time that states and local municipalities implemented SAH orders. Alternatively, there may be time-invariant state-specific factors that drove both increases in unemployment claims and SAH orders. To address these concerns, we employ a panel specification, which allows us to control for week and state fixed effects.

We modify the specification so that the outcome variable is the flow value of initial claims on date t and the SAH order treatment is the share of the *current week* that a state was subject to SAH orders, where we take a weighted average of county-level exposure as before.[1]

$$\frac{UI_{s,t}}{Emp_s} = \alpha_s + \phi_t + \beta_P \times SAH_{s,t,t-7} + \mathbf{X}_{s,t}\Gamma + \epsilon_{s,t} \tag{C.1}$$

We consider a variety of state-time controls. We include two lags of $SAH_{s,t,t-7}$ to account for dynamics in the effect of SAH orders on unemployment claims. Additionally, we include the share of the population that works from home, the number of confirmed cases per one thousand people, and the Bartik-style employment control from before. Each of these three controls is interacted with a dummy equal to one for weeks ending March 21st, 2020 and onward.[2] We estimate the following fixed effects panel regression on weekly observations for the week ending January 4 through the week ending April 11.[3]

Table C.1 provides our estimate of $\hat{\beta}_P$ for the contemporaneous effect and two lags.

[1]Because in our sample no state or local municipality reopened, once $SAH_{s,t,t-7} = 1$ it remains equal to one for all remaining weeks.

[2]Note that because our measures of work-from-home and employment loss are constant across time, we are controlling for the relative effect of each from before the week ending March 21st.

[3]We drop the first two weeks in all specifications to ensure the sample size is constant throughout.

Column (1) presents the results with no lags. The point estimate of 0.90% (SE: 0.35%) suggests that a full week of SAH order exposure increased unemployment claims by .90% of total state-level employment. In column (2), we include two lags of SAH orders. The point estimate on the contemporaneous effect is little changed, though it rises slightly. Importantly, neither of the coefficients on the first nor the second lag is significant. This result suggests that, in our sample, that SAH orders have constant, contemporaneous effects on UI claims. At longer horizons, we would suspect non-linearities to eventually kick in, with the effect of SAH orders declining. Finally, our point estimates are little changed when including additional controls in Column (3).

Our estimates $\hat{\beta}_P$ in the first three columns tend to be somewhat lower than what we find in our benchmark, cross-sectional design. In particular, the panel design implies that each week of SAH exposure increased UI claims by 1% of state employment; in contrast, our estimates of $\hat{\beta}_C$ imply that each week of SAH exposure increased UI claims by approximately 1.9% of state employment. While, at first glance, β_C and β_P aim to estimate the same moment, the inclusion of state and time fixed effects imply that they are not directly comparable.[4] In column (4), we consider the panel specification in which we drop state fixed effects, to make the panel and cross-sectional regressions comparable: the point estimate rises to 1.2% and is statistically indistinguishable from what we find in the cross-section.

High Frequency Effects on Proxies for Local Economic Activity

In this subsection, we provide additional evidence that the SAH orders had immediate and highly localized effects on daily indicators of economic activity. This exercise is important because of concerns that the state-level effects we estimate above simply reflect differential labor market disruptions that would have occurred in the absence of SAH orders in precisely those places most likely to implement SAH orders earliest.

We estimate the local effect of SAH using high frequency proxies for economic activity from Google's Community Mobility Report, which measures changes in visits to establishments in various categories, such as retail and work.[5] Early on in the COVID-19 pandemic, Google began publishing data documenting how often its users were visiting different types of establishments. The data are reported as values relative to the median visitation rates by week-day between January 3, 2020 and February 6, 2020.[6],[7]

We use the retail and workplace mobility indices because these two indices are consistently recorded for the time sample we study. Failing to find an effect on these proxies for local economic activity would call into question the results we find in the aggregate, at the state-

[4]See Kropko and Kubinec 2020 for a discussion of the proper interpretation of two-way fixed effect estimators in relation to one-way fixed effect estimators.

[5]`https://www.google.com/covid19/mobility/`

[6]One possible limitation of this data is that the sample of accounts included in the surveys is derived from only those with Google Accounts who opt into location services. We believe sample selection bias is unlikely to be a major concern given Google's broad reach (there are over 1.5 billion Gmail accounts, for example).

[7]Note that for privacy reasons, data is missing for some days for some counties. When possible, we carry forward the last non-missing value. Excluding counties with missing values yields the same result; this figure is available from the authors upon request.

level. We interpret retail mobility as broadly representing "demand" responses to SAH orders and workplace mobility as broadly representing "supply," at least on-impact.[8] Over longer-horizons, workers laid off because of demand-side disruptions will, naturally, cease commuting to and from work.

Formally, we estimate event studies of the following form:

$$Mobility_{c,t} = \alpha_c + \phi_{CZ(c),t} + \sum_{k=\underline{K}}^{\overline{K}} \beta_k SAH_{c,t+k} + X_{c,t} + \underline{D}_{c,t} + \overline{D}_{c,t} + \varepsilon_{c,t} \tag{C.2}$$

where $Mobility_{c,t}$ represents either the retail or workplace mobility index published by Google for county c on day t, and $SAH_{c,t}$ is a dummy variable equal to 1 on the day a county imposes SAH orders. We set $\underline{K} = -17$ and $\overline{K} = 21$ so that the analysis examines three weeks prior and two and a half weeks following the imposition of SAH orders.[9] The event study is estimated over the period February 15th through April 24th, 2020. We non-parametrically control for county size by discretizing county employment into fifteen equally sized bins and interacting each bin with time fixed effects. α_c refers to the inclusion of county fixed effects. To isolate the local effect of SAH orders on economic activity, we also include commuting zone-by-time fixed effects.[10] This implies that our event-study estimates are identified only off of differential timing of SAH implementation among counties contained within the same commuting zone.

Results for retail mobility are presented in Figure C.1. The day SAH orders went into effect, there was an immediate decline of approximately 2% in retail mobility. This falls further to 7% the day after SAH order implementation, before slowly recovering to approximately 2% lower retail mobility two and a half weeks following the SAH order imposition.[11] The large transitory dip may reflect sentiment among consumers to shut-in before revisiting grocery stores and pharmacies. Alternatively, given our inclusion of commuting zone-by-time fixed effects, the transitory nature of the shock may reflect negative, within-labor market spillovers of SAH orders. Regardless, the lack of a pre-trend is noticeable and provides additional support for a causal interpretation.

SAH orders may have affected firms' ability to produce by preventing workers from accessing their places of employment. To investigate whether SAH orders may have affected firms' productive capacity through this channel, we re-estimate our event study using workplace

[8]Of course, both indicators are equilibrium outcomes of both supply and demand shocks. The on-impact effect on work-place mobility at the very least reflects disruptions to each firm's ability to produce. Similarly, the on-impact effect on retail mobility is indicative of a decline in retail demand by consumers since, presumably, the supply of retail goods is at least fixed in the very short-run.

[9]Because our sample is necessarily unbalanced in event-time, we also include "long-run" dummy variables, $\underline{D}_{c,t}$ and $\overline{D}_{c,t}$. $\underline{D}_{c,t}$ is equal to 1 if a county imposed SAH orders at least $\overline{K}$ days prior. $\overline{D}_{c,t}$ is equal to 1 if a county will impose SAH at least $\underline{\mathrm{K}}$ periods in the future.

[10]We use the United States Department of Agriculture (USDA) 2000 county to commuting zone crosswalk. This is available at `https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/`.

[11]Restricting the sample to exclude never-takers yields the same result. This design identifies the mobility effects off of counties that ultimately implemented SAH orders but at different times.

mobility as the outcome variable.[12]

Figure C.2 shows the result. As with the retail mobility event study, the workplace mobility index exhibits no differential pre-trend prior to the county-level imposition of SAH orders. In the first two days following the imposition of SAH orders, workplace mobility declined sharply relative to non-treated counties within its commuting zone. This relative decline in workplace mobility persists for nearly two and a half weeks following.

We draw three conclusions from these high-frequency event studies. First, the lack of pre-trends in the event studies suggest that the timing of SAH orders can be seen as plausibly randomly assigned with respect to local labor market conditions. This provides corroborating evidence for our cross-sectional identification strategy. In particular, it suggests that there were real effects of the SAH orders on local economies. Second, with the important caveat that both mobility indices are equilibrium objects, SAH orders appear to have had *both* local supply and local demand effects. Both retail mobility and workplace mobility fell substantially on impact and remained persistently low for at least two weeks following implementation of SAH orders. Third, given that overall workplace and retail mobility in the U.S. fell by 48 and 40 percent through April 24th relative to their baseline levels, our results bolster the claim that alternative mechanisms were responsible for the majority of job losses in the early weeks of the crisis; upon SAH implementation, relative workplace and retail mobility fell by, at most, 2 and 7 percent, respectively.

Alternative Cross-Sectional Specifications

The first type of robustness check we do is varying the horizon over which the cross-sectional regression is estimated, considering two natural alternative specifications: a two week horizon and a four week horizon. For the two week horizon specification, we consider cumulative initial claims between March 14 and March 28 regressed on SAH exposure over the same window; for the four week specification, the end date is April 11. We include the same set of controls as in our benchmark specification (Table 4.1, Column (5)).

Columns (1) and (2) of Table C.2 report the results from varying the horizon over which the model is estimated. Relative to our baseline result of 1.9%, estimating the model over just two weeks lowers the point estimate slightly to 1.83% (SE: 0.91%). Conversely, when the model is estimated over a four week horizon, the point estimate is 1.7% (SE: 0.59%).

In Column (3) of Table C.2 we estimate the effect of SAH exposure on UI claims, over the same three week horizon as in the benchmark case, weighting observations by state-level employment from the QCEW in 2018 (an approached advocated for by some papers in the

[12] An obvious concern with simply replacing the outcome variable is that changes in workplace mobility, unlike retail mobility, is highly dependent on the ability of individuals to work from home. The timing of SAH orders may be partially driven by the ability of workers in some regions to transition to working at home. In unreported regressions, we also non-parametrically control for this possibility by partitioning the WAH variable into 15 equally sized bins and interacting each bin with time fixed effects. The event study is essentially unchanged.

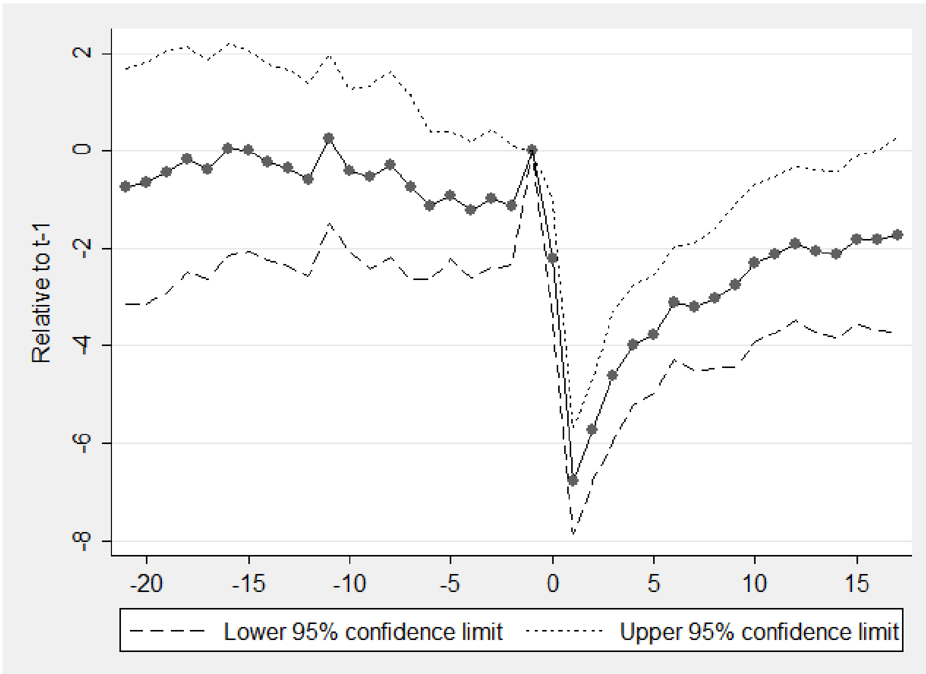
Table C.1: Panel Specification: Effect of Stay-at-Home Orders on Initial Weekly Claims Relative to State Employment

	(1)	(2)	(3)	(4)
SAH Exposure Current Week	0.00919**	0.0101***	0.00997***	0.0125***
	(0.00350)	(0.00321)	(0.00329)	(0.00353)
SAH Exposure First Lag		-0.00293	-0.00367	-0.00299
		(0.00359)	(0.00358)	(0.00372)
SAH Exposure Second Lag		0.00245	-0.00115	0.000809
		(0.00230)	(0.00302)	(0.00332)
State FE	Y	Y	Y	N
Week FE	Y	Y	Y	Y
Post-March 21 X Work at Home Index	N	N	Y	Y
Post-March 21 X Excess Deaths per 1K	N	N	Y	Y
Post-March 21 X COVID-19 Cases per 1K	N	N	Y	Y
Post-March 21 X Avg. UI Replacement Rate	N	N	Y	Y
Adj. R-Square	0.826	0.822	0.831	0.801
No. Obs.	765	663	663	663

Table C.2: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment: (i) 2-Week Horizon, (ii) 4-Week Horizon, (iii) Weighted Least Squares

	(1) Thru Mar. 28	(2) Thru Apr. 11	(3) WLS
SAH Exposure (varied horizons)	0.0183**	0.0166***	0.0209***
	(0.00908)	(0.00592)	(0.00541)
COVID-19 Cases per 1K	0.00197	0.000854	-0.00472
	(0.0109)	(0.00463)	(0.00306)
Excess Deaths per 1K	-0.0819	0.0691	0.214**
	(0.0959)	(0.0787)	(0.106)
Work at Home Index	-0.152	-0.587**	-0.486+
	(0.184)	(0.261)	(0.258)
Constant	0.111+	0.303***	0.242**
	(0.0649)	(0.0920)	(0.0921)
Adj. R-Square	0.0125	0.129	0.172
No. Obs.	51	51	51

Figure C.1: County Retail Mobility Event Study



local multiplier literature).[13] Again, we consider the same set of controls as in our benchmark specification. The point estimate from the WLS regression is elevated slightly: 2.10% (SE: 0.54%). Regardless, weighting delivers quantitatively similar estimates.

Influence of Specific States

One may also be concerned that individual states' responses, either in terms of rising unemployment claims or SAH orders, is driving our results. To understand whether this is the case, we replicate our benchmark specification (column (5) in Table 4.1) from above, dropping one state at a time. The resulting coefficient estimates for β_C are available in Figure C.3, along with 90 percent confidence intervals constructed from robust standard errors.

Pre-SAH Determinants of UI Claims

In this subsection, we broaden our analysis to adjust for determinants of state-level UI claims that may have been correlated with the timing of SAH implementation at the local level, as reported by the *New York Times*.

The first change that we make, relative to the results presented in Table 4.1, is to control for the March 7 to March 14 change in consumer spending. Because consumption is a

[13]For arguments in either direction, see Ramey (2019) and Chodorow-Reich (Forthcoming), respectively. See also Solon, Haider, and Wooldridge (2015).

Figure C.2: County Workplace Mobility Event Study

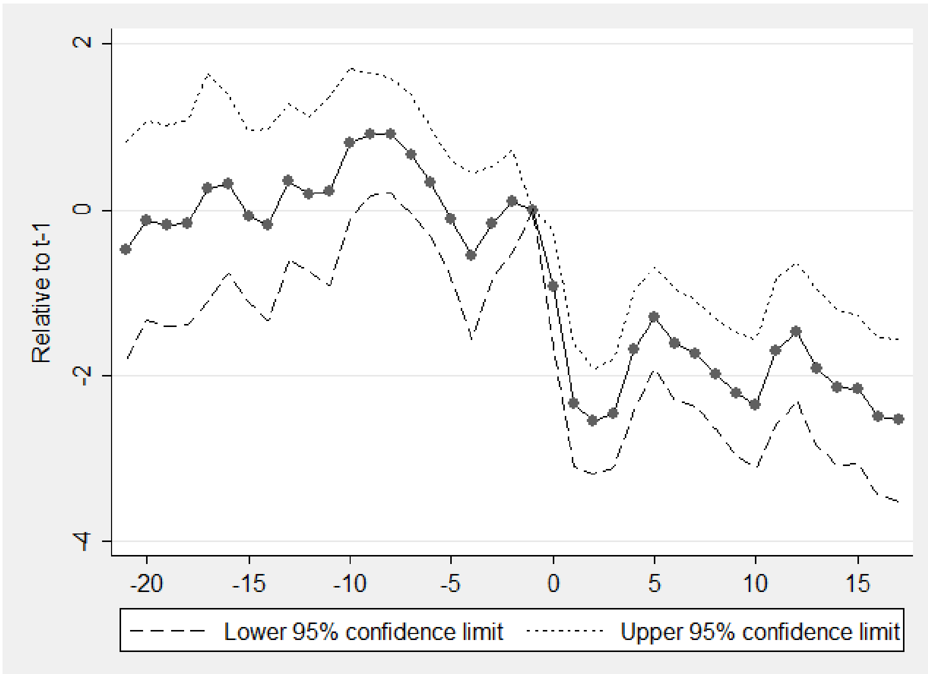
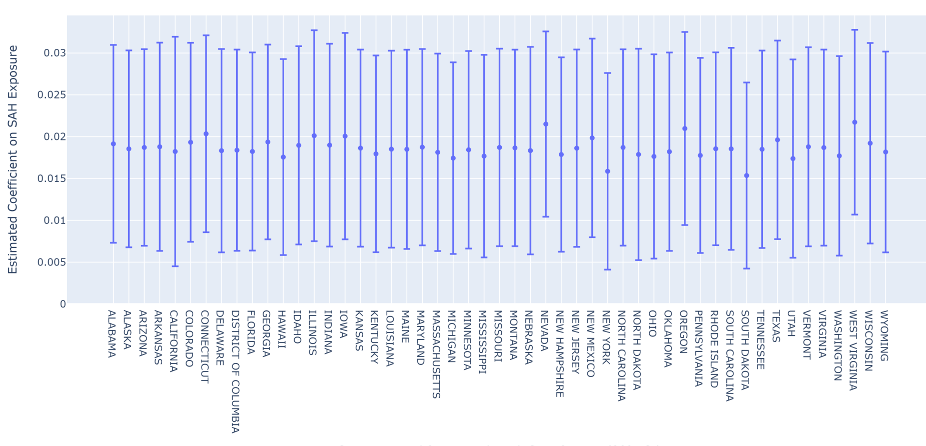


Figure C.3: Benchmark Specification Estimated Dropping One State at a Time



leading indicator, changes to consumer spending tend to precede changes to employment. Thus, this allows us to control for leading determinants—as manifested in changes to state-level consumer spending—of employment losses that may have also been correlated with the timing of the implementation of SAH orders.

To do so, we rely upon the newly available, daily consumer spending index constructed by Chetty et al. (2020). These high frequency indicators of state-level economic activity is constructed from proprietary private sector microdata and made publicly available at `https://tracktherecovery.org`.

The second adjustment made in this subsection relates to the timing of state-level SAH implementation. In a few notable instances, the closure of non-essential businesses by state and local officials did not coincide with the broader SAH orders requiring all individuals to remain at home except for essential activities.[14] For example, on March 19 the governor of Pennsylvania issued a statewide executive order that required non-essential, in-person business activity to cease. This preceded by nearly a week the full statewide SAH order that was put into effect on March 23. A similar discrepancy between SAH dates and non-essential business closure occurred in Nevada.

This is potentially important since both Pennsylvania and Nevada experienced larger cumulative increases in UI claims to employment than the rest of the country through April 4. If the discrepancy between non-essential business closure and SAH implementation (as reported by the *New York Times*) was systematically correlated with the severity of job losses, then our estimate of β_C may be biased. In particular, if the pattern for Pennsylvania and Nevada holds more generally—large UI claims increase and relatively early non-essential business closure—then our estimates of β_C in Table 4.1 will be biased downwards, leading us to understate both the relative employment effect of SAH orders and their implied aggregate effect.

We adjust for the discrepancy between SAH implementation as reported in the *New York Times* and non-essential business closures by constructing a combined SAH/business closure treatment variable:

$$SAHBIZ_{s,t} = \max\{SAH_{s,t}, BIZ_{s,t}\}, \tag{C.3}$$

where $BIZ_{s,t}$ is the number of weeks state s was subject to a non-essential business closure through date t.[15].

Table C.3 records the results after incorporating the March 7 to March 14 change in the consumer spending index and adjusting the treatment variable to handle discrepancies between reported SAH implementation dates and dates of non-essential business closures. This table is structured identically to Table 4.1 except for the aforementioned changes.

Both qualitatively and quantitatively the effect on unemployment of SAH orders is essentially unchanged relative to the benchmark specification. Consider Column (5): The point estimate of 1.9% (SE: 0.88%) implies that each additional week that a state was subject to a SAH order and/or non-essential business closures increased unemployment claims by 1.9% of the state's employment level.

While this point estimate is the same as our benchmark estimate, the relative-implied aggregate estimate of employment losses due to SAH orders through April 4, 2020 needs to be slightly adjusted. Incorporating non-essential business closure dates weakly increases each

[14]The closure of non-essential businesses is a prominent feature of most SAH orders.

[15]We use the state-level non-essential business closure dates compiled in Kong and Prinz 2020.

state's degree of SAH exposure. Recalculating equation (4.6) with the model estimated in Column (5) of Table C.3 yields an estimate of 4.6 million claims through April 4 attributable to SAH orders or approximately 27% of the overall increase in UI claims over the same period.[16]

County-Level Event Study Employment Specification

In Subsection 4.6 we use BLS-reported, month-to-month changes in county employment and unemployment to estimate the effect of SAH orders after controlling for state fixed effects. In what follows, we use county-level, high frequency employment indices to provide additional evidence that SAH orders had highly localized effects on county-level employment.[17]

Not only is the effect we estimate in this subsection consistent with our central finding, but by using high frequency, county-level data we are able to directly assess our assumption that the timing of local SAH implementation was uncorrelated with the relative severity of the local economic downturn. Consistent with the evidence presented in Subsection C.1, we find no evidence of differential pre-trends in employment around the implementation of SAH orders.

For the subset of counties for which the high-frequency employment indices are available, we estimate the following event study specification:

$$EmpIDX_{c,t} = \alpha_c + \phi_{state(c),t} + \sum_{k=\underline{K}}^{\overline{K}} \beta_k SAH_{c,t+k} + X_{c,t} + \underline{D}_{c,t} + \overline{D}_{c,t} + \varepsilon_{c,t} \tag{C.4}$$

where $EmpIDX_{c,t}$ represents the county-level, employment index available at `https://tracktherecovery.org`, $SAH_{c,t}$ is a dummy variable equal to 1 on the day a county imposes SAH orders, and $\phi_{state(c),t}$ is a state-by-time fixed effect. As in Subsection C.1, we set $\underline{K} = -17$ and $\overline{K} = 21$; the analysis thus examines three weeks prior and two and a half weeks following the imposition of SAH orders.[18] The event study is estimated over the period February 15th through April 24th, 2020. For this event study specification, we include no

[16]The two controls we consider in this section each slightly alter the estimated coefficient for the specification analogous to our benchmark specification. Controlling only for the change in the consumer spending index attenuates the point estimate to 1.4% (SE: 0.80%). Only adjusting for the discrepancies between non-essential business closure dates and reported SAH dates amplifies the point estimate somewhat to 2.4% (SE: 0.68); however, this latter effect appears to be driven almost entirely by Pennsylvania and Nevada. Dropping these states from the estimation yields a point estimate of 1.9% (SE: 0.68). These results are available upon request.

[17]The county-level employment indices we use were constructed by Chetty et al. 2020 and are available at `https://tracktherecovery.org` The county-level employment statistics we use are built out from anonymized microdata from private companies. See Chetty et al. 2020 for a fuller description of the data construction and for evidence that these series tend to track lower-frequency, publicly available series constructed from representative surveys.

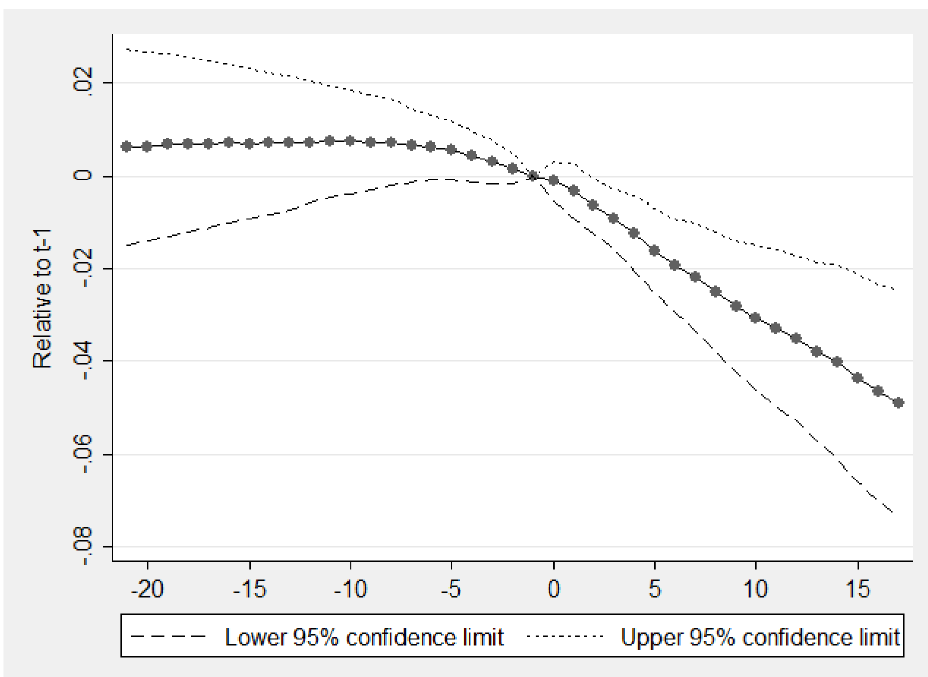
[18]Our sample is necessarily unbalanced in event time, so we include "long-run" dummy variables $\underline{D}_{c,t}$ and $\overline{D}_{c,t}$ which are equal to 1 if a county imposed a SAH order at least $\overline{K}$ days prior or will impose a SAH order at least $\underline{\mathrm{K}}$ days in the future, respectively.

additional controls beyond county fixed effects and state-by-time fixed effects.

The results of this exercise are reported in Figure C.4. In the three weeks prior to the implementation of SAH orders, there is no statistically discernible pre-trend in employment.[19] However, there is a clear decline in employment after SAH orders were put into place. By one week following the SAH implementation, the employment index was down by 1.9% (SE: 0.5%). Two weeks following SAH implementation, the county-level index was down by by nearly twice as much.

For this analysis, we rely upon a subset of counties for which we have a high frequency measure of employment changes and for which there exist within-state variation. Nevertheless, despite relying upon a different subset of the variation for identification, the weekly effect on employment we estimate here is remarkably consistent with our state-level analysis, in terms of both magnitude and linearity of the effect. We view this as strongly corroborating our baseline finding and allaying concerns that the timing of SAH implementation was differentially correlated with the severity of each labor markets economic downturn.

Figure C.4: County Employment Event Study



[19]While not statistically meaningful, there appears to be a slight inflection point approximately one week prior to SAH implementation. However, even this is likely a statistical artifact, since the county-level employment statistics we rely upon are primarily reliant upon weekly payroll data from the company Paychex. Chetty et al. 2020 write: We convert the weekly Paychex data to daily measures of employment by assuming that employment is constant within each week.

Table C.3: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment for Weeks Ending March 21 thru April 4, 2020 After Accounting for Additional Pre-SAH Determinants of UI Claims.

	(1) Bivariate	(2) Covid	(3) Pol. Econ.	(4) Sectoral	(5) All
SAH/Business Closure Exposure	0.0214**	0.0218**	0.0215**	0.0224**	0.0191**
	(0.00855)	(0.00916)	(0.00972)	(0.00882)	(0.00884)
Mar. 7 to Mar. 14 Spending Change	-0.158	-0.183	-0.183	-0.310	-0.351
	(0.293)	(0.289)	(0.289)	(0.272)	(0.279)
COVID-19 Cases per 1K		-0.00295			0.00249
		(0.00579)			(0.00592)
Excess Deaths per 1K		0.0537			0.0637
		(0.120)			(0.109)
60+ Ratio to Total Population		0.308			
		(0.266)			
Avg. UI Replacement Rate			0.0740		0.0751
			(0.0764)		(0.0754)
2016 Trump Vote Share			0.00881		
			(0.0589)		
Work at Home Index				-0.500***	-0.563***
				(0.184)	(0.187)
Bartik-Predicted Job Loss				1.219	
				(7.388)	
Constant	0.0743***	0.0144	0.0372	0.259***	0.239***
	(0.0152)	(0.0517)	(0.0536)	(0.0793)	(0.0764)
Adj. R-Square	0.131	0.107	0.106	0.186	0.179
No. Obs.	51	51	51	51	51

C.2 Local SAH Orders in a Currency Union Model

We develop a framework to help us interpret the "relative effect"—which we estimate in the data—as compared to the "aggregate effect" of stay-at-home orders. To that end, we use a simple version of Nakamura and Steinsson 2014 of a two-country monetary union model, albeit abstracting from government spending as that is not the focus of our paper.

Households

Consider a currency union comprised of two regions: a home region of size n, and a foreign region of size $1-n$. In each region, there are infinitely many households with *identical* preferences and initial wealth.

A household j in home region has the following preferences:

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\delta_t \frac{\left(C_t^j\right)^{1-\sigma}}{1-\sigma} - \chi \frac{\left(N_t^j\right)^{1+\psi}}{1+\psi} \right]$$

where

$$C_t^j = \left[\phi_H^{\frac{1}{\eta}} \left(C_{H,t}^j\right)^{\frac{\eta-1}{\eta}} + \phi_F^{\frac{1}{\eta}} \left(C_{F,t}^j\right)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}}, \text{ with } \phi_H + \phi_F = 1,$$

$$C_{H,t}^j = \left(\int_0^n \left(\frac{1}{n}\right)^{\frac{1}{\epsilon}} c_{h,t}^j(i)^{\frac{\epsilon-1}{\epsilon}} \mathrm{d}i \right)^{\frac{\epsilon}{\epsilon-1}}, \quad C_{F,t} = \left(\int_n^1 \left(\frac{1}{1-n}\right)^{\frac{1}{\epsilon}} c_{f,t}^j(i)^{\frac{\epsilon-1}{\epsilon}} \mathrm{d}i \right)^{\frac{\epsilon}{\epsilon-1}}.$$

Total consumption of a household j in a home region is a CES aggregator of a *bundle* of home goods, $C_{H,t}^j$ and a *bundle* of foreign goods, $C_{F,t}^j$. Here, ϕ_F denotes the steady state share of the foreign goods imported from by a household in the home region. When $\phi_H = 1-\phi_F > n$, there is home bias.[20] η is the elasticity of substitution between home goods and imported goods from a foreign region, and ϵ denotes the elasticity of substitution across differentiated goods. β is discount factor and δ_t denotes consumption-preference shock in a home region, which evolves according to the following law of motion:

$$\log \delta_t = \rho^\delta \log \delta_{t-1} + \epsilon_t^\delta.$$

Then optimal allocations of expenditures (per household) are given by

$$C_{H,t}^j = \phi_H \left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t^j, \quad C_{F,t} = \phi_F \left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t^j,$$

$$c_{h,t}^j(i) = \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon} C_{H,t}^j, \quad c_{f,t}^j(i) = \left(\frac{p_{f,t}(i)}{P_{F,t}}\right)^{-\epsilon} C_{F,t}^j,$$

[20]In the baseline calibration following Nakamura and Steinsson (2014), we calibrate $\phi_H = 0.69$ and $n = 0.1$, so that there is significant home bias.

with price indices defined as follows:

$$P_t = \left[\phi_H P_{H,t}^{1-\eta} + \phi_F P_{F,t}^{1-\eta}\right]^{\frac{1}{1-\eta}},$$
$$P_{H,t} = \left[\frac{1}{n}\int_0^n p_{h,t}(i)^{1-\epsilon}\mathrm{d}i\right]^{\frac{1}{1-\epsilon}},$$
$$P_{F,t} = \left[\frac{1}{1-n}\int_n^1 p_{f,t}(i)^{1-\epsilon}\mathrm{d}i\right]^{\frac{1}{1-\epsilon}}.$$

Here, P_t denotes consumper price index of a home region, and $P_{H,t}$ ($P_{F,t}$) is producer price index of home (foreign) goods.

In our baseline specification, we assume identical households in a given region with the same initial wealth and *complete* financial markets, which makes aggregation straightforward. Thus, we have

$$c_{h,t}(i) \equiv \int_0^n c_{h,t}^j(i)\mathrm{d}j = \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon} C_{H,t}, \quad c_{f,t}(i) \equiv \int_0^n c_{f,t}^j(i)\mathrm{d}j = \left(\frac{p_{f,t}(i)}{P_{F,t}}\right)^{-\epsilon} C_{F,t}$$
$$C_{H,t} = \int_0^n C_{H,t}^j\mathrm{d}j = \phi_H\left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t, \quad C_{F,t} = \int_n^1 C_{F,t}^j\mathrm{d}j = \phi_F\left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t,$$
$$C_t = \int_0^n C_t^j\mathrm{d}j = nC_t^j,$$

where variables without j superscript are aggregate variables in a home region.

With the optimal allocations, we can write household j's budget constraint (in real terms with the home region's CPI as a numeraire) as follows:

$$C_t^j + \mathbb{E}_t\left[M_{t,t+1}B_{t+1}^j\right] \leq B_t^j + \frac{W_t}{P_t}N_t^j + \int_0^1 \frac{\Xi_{h,t}^j(i)}{P_t}\mathrm{d}i - \frac{T_t^j}{P_t}.$$

Note that W_t is home region's nominal wage, and N_t^j is a household j's labor supply. Here, we assume perfect immobility across the regions, meaning wages will be determined at the regional level. B_{t+1}^j is a household j's state-contingent asset holdings and note again that we assume complete financial markets. Here P_t denotes price index that gives the minimum price of one unit of consumption good, C_t. *i.e.* P_t is the Consumer Price Index (CPI) in the home region.

Optimality conditions for $j \in (0, n]$ are

$$\chi\left(N_t^j\right)^{\psi} = \delta_t\left(C_t^j\right)^{-\sigma}\frac{W_t}{P_t},$$
$$\delta_t\left(C_t^j\right)^{-\sigma} = \beta\mathbb{E}_t\left[\delta_{t+1}\left(C_{t+1}^j\right)^{-\sigma}\frac{1+i_t}{1+\pi_{t+1}}\right],$$

where i_t is one-period nominal spot interest rate which satisfies $\mathbb{E}_t[M_{t,t+1}] = 1/(1+i_t)$.

Households in the foreign region are symmetric relative to those in the home region, and we use $*$ to denote foreign variables. So we have

$$C_t^{*j} = \left[(\phi_H^*)^{\frac{1}{\eta}} \left(C_{H,t}^{*j}\right)^{\frac{\eta-1}{\eta}} + (\phi_F^*)^{\frac{1}{\eta}} \left(C_{F,t}^{*j}\right)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}}, \quad \text{with} \quad \phi_H^* + \phi_F^* = 1.$$

For *aggregate* optimal allocations in the foreign region, we have

$$c_{h,t}^*(i) \equiv \int_n^1 c_{h,t}^{*j}(i)\mathrm{d}j = \left(\frac{p_{h,t}^*(i)}{P_{H,t}^*}\right)^{-\epsilon} C_{H,t}^*, \quad c_{f,t}^*(i) \equiv \int_n^1 c_{f,t}^{*j}(i)\mathrm{d}j = \left(\frac{p_{f,t}^*(i)}{P_{F,t}^*}\right)^{-\epsilon} C_{F,t}^*$$

$$C_{H,t}^* = \int_n^1 C_{H,t}^{*j}\mathrm{d}j = \phi_H^* \left(\frac{P_{H,t}^*}{P_t^*}\right)^{-\eta} C_t^*, \quad C_{F,t}^* = \int_n^1 C_{F,t}^{*j}\mathrm{d}j = \phi_F^* \left(\frac{P_{F,t}^*}{P_t^*}\right)^{-\eta} C_t^*,$$

$$C_t^* = \int_n^1 C_t^{*j}\mathrm{d}j = (1-n)C_t^{*j}.$$

Optimality conditions for foreign households for $j \in [n, 1)$ are

$$\chi \left(N_t^{s,j*}\right)^{\psi} = \delta_t^* \left(C_t^{j*}\right)^{-\sigma} \frac{W_t^*}{P_t^*},$$

$$\delta_t^* \left(C_t^{j*}\right)^{-\sigma} = \beta \mathbb{E}_t \left[\delta_{t+1}^* \left(C_{t+1}^{j*}\right)^{-\sigma} \frac{1+i_t}{1+\pi_{t+1}^*}\right].$$

Terms of Trade, and Real Exchange Rate

Before moving on to firms in each region, let us define terms showing the relationships between various price measures. First, we define terms of trade, S_t as

$$S_t \equiv \frac{P_{F,t}}{P_{H,t}}.$$

From this, we can write the relationship between CPI and Producer Price Index (PPI) in a home region as:

$$g(S_t) \equiv \frac{P_t}{P_{H,t}} = \left[\phi_H + \phi_F S_t^{1-\eta}\right]^{\frac{1}{1-\eta}}, \quad \frac{P_t}{P_{F,t}} = \frac{P_t}{P_{H,t}} \frac{P_{H,t}}{P_{F,t}} = \frac{g(S_t)}{S_t}.$$

For the case of the foreign region, we have

$$g^*(S_t) \equiv \frac{P_t^*}{P_{H,t}^*} = \left[\phi_H^* + \phi_F^* S_t^{1-\eta}\right]^{\frac{1}{1-\eta}}, \quad \frac{P_t^*}{P_{F,t}^*} = \frac{P_t^*}{P_{H,t}^*} \frac{P_{H,t}^*}{P_{F,t}^*} = \frac{g^*(S_t)}{S_t}.$$

Finally, we write the real exchange rate in terms of $g(S_t)$ and $g^*(S_t)$ as follows:

$$Q_t = \frac{P_t^*}{P_t} = \frac{g^*(S_t)}{g(S_t)}.$$

Firms

We assume that there is a continuum of intermediate-goods-producing firms in each region, producing differentiated intermediate goods by using labor as input. We assume a competitive labor market.

Production technologies of each intermediate-goods-producing firms are given by

$$\begin{aligned} y_{h,t}(i) &= A_t N_{h,t}(i)^{\alpha}, \quad \alpha < 1, \\ y_{f,t}(i) &= A_t^* N_{f,t}^*(i)^{\alpha}, \quad \alpha < 1, \end{aligned}$$

where $y_{h,t}(i)$ ($y_{f,t}(i)$) is the production output of a firm i in the home (foreign) region, $N_{h,t}(i)$ ($N_{f,t}^*(i)$) is the amount of labor input hired by a firm i in the home (foreign) region, and A_t (A_t^*) is region-wide technology in the home (foreign) region. Both technology processes evolve according to the following laws of motion:

$$\begin{aligned} \log A_t &= \rho^A \log A_{t-1} + \epsilon_t^A, \\ \log A_t^* &= \rho^{A*} \log A_{t-1}^* + \epsilon_t^{A*} \end{aligned}$$

This implies that region-wide labor demand can be written as

$$\begin{aligned} N_t &= \int_0^n N_{h,t}(i)\mathrm{d}i = \int_0^n \left(\frac{y_{h,t}(i)}{A_t}\right)^{\frac{1}{\alpha}} \mathrm{d}i = \left(\frac{1}{A_t}\right)^{\frac{1}{\alpha}} \int_0^n y_{h,t}(i)^{\frac{1}{\alpha}} \mathrm{d}i \\ &= \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1}{\alpha}} \int_0^n \frac{1}{n} \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\frac{\epsilon}{\alpha}} \mathrm{d}i = \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1}{\alpha}} \Delta_t^{\frac{1}{\alpha}}, \\ N_t^* &= \int_0^n N_{f,t}^*(i)\mathrm{d}i = \int_n^1 \left(\frac{y_{f,t}(i)}{A_t^*}\right)^{\frac{1}{\alpha}} \mathrm{d}i = \left(\frac{1}{A_t^*}\right)^{\frac{1}{\alpha}} \int_n^1 y_{f,t}(i)^{\frac{1}{\alpha}} \mathrm{d}i \\ &= \left(\frac{Y_{F,t}}{A_t^*}\right)^{\frac{1}{\alpha}} \int_n^1 \frac{1}{1-n} \left(\frac{p_{f,t}(i)}{P_{i,t}}\right)^{-\frac{\epsilon}{\alpha}} \mathrm{d}i = \left(\frac{Y_{F,t}}{A_t^*}\right)^{\frac{1}{\alpha}} (\Delta_t^*)^{\frac{1}{\alpha}}, \end{aligned}$$

by defining $\Delta_t \equiv \frac{1}{n} \int_0^n \left(\frac{p_{h,t}(i)}{P_t}\right)^{-\epsilon} \mathrm{d}i$, and $\Delta_t^* \equiv \frac{1}{1-n} \int_n^1 \left(\frac{p_{f,t}(i)}{P_t^*}\right)^{-\epsilon} \mathrm{d}i$ as price dispersion terms in each region.

Firms are subject to Calvo-type pricing frictions, so they solve the following problem:

$$\max_{p_{h,t}^{\#}(i)} \mathbb{E}_t \left[\sum_{k=0}^{\infty} Q_{t,t+k} \theta^k \left(p_{h,t}^{\#}(i) - MC_{h,t+k|t}(i)\right) y_{h,t+k|t}(i) \right]$$

subject to $y_{h,t+k|t}(i) = \left(\frac{p^{\#}_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon}\left(C_{H,t} + C^*_{H,t}\right)$, and with $Q_{t,t+k} = \beta^k \frac{\delta_{t+k}u'(C_{t+k})}{\delta_t u'(C_t)}$. Note that here, $C^*_{H,t}$ denotes a composite index of foreign consumption of home goods, and $MC_{h,t+k|t}(i)$ is nominal marginal cost.

Then optimality conditions for pricing are given by

$$p^{\#}_{h,t}(i) = \frac{\epsilon}{\epsilon - 1}\frac{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k \delta_{t+k}u'(C_{t+k})mc_{h,t+k|t}(i)P^{\epsilon}_{H,t+k}\left(C_{H,t} + C^*_{H,t}\right)}{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k \delta_{t+k}u'(C_{t+k})P^{\epsilon-1}_{H,t+k}\left(C_{H,t} + C^*_{H,t}\right)},$$

with $mc_{h,t+k|t}(i)$ is real marginal cost of a firm i in terms of PPI, $P_{H,t}$.

Aggregate real marginal cost with $\alpha < 1$ can be written as follows:

$$\begin{aligned} mc_{h,t}(i) &= \frac{W_t/P_{H,t}}{\alpha A_t N_{h,t}(i)^{\alpha-1}} = \frac{w_t}{\alpha A_t}N_{h,t}(i)^{1-\alpha} \\ &= \frac{w_t}{\alpha A_t}\left(\frac{y_{h,t}(i)}{A_t}\right)^{\frac{1-\alpha}{\alpha}} = \frac{w_t}{\alpha A_t}\left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1-\alpha}{\alpha}}\left(\frac{y_{h,t}(i)}{Y_{H,t}}\right)^{\frac{1-\alpha}{\alpha}} \\ &= mc_{H,t}\left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\frac{\epsilon(1-\alpha)}{\alpha}}, \\ mc_{H,t} &\equiv \frac{w_t}{\alpha A_t}\left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1-\alpha}{\alpha}}. \end{aligned}$$

with $w_t \equiv W_t/P_{H,t}$.
Combining this with the previous optimal pricing equation then generates

$$p^{\#}_{h,t}(i)^{1+\frac{\epsilon(1-\alpha)}{\alpha}} = \frac{\epsilon}{\epsilon - 1}\frac{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k u'(C_{t+k})mc_{H,t+k}P^{\epsilon/\alpha}_{H,t+k}Y_{H,t+k}}{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k u'(C_{t+k})P^{\epsilon-1}_{H,t+k}Y_{H,t+k}}.$$

We have similar conditions for intermediate-goods-producing firms in the foreign region.

International Risk Sharing Condition and Market Clearing Conditions

Combining each region's Euler equation gives

$$\delta_t\left(\frac{1}{n}C_t\right)^{-\sigma} = \kappa\delta^*_t\left(\frac{1}{1-n}C^*_t\right)^{-\sigma}\frac{1}{\mathcal{Q}_t},$$

with complete markets and symmety of initial conditions, $\kappa = 1$, generating

$$\delta_t^{-\frac{1}{\sigma}}C_t = \frac{n}{1-n}\delta_t^{*-\frac{1}{\sigma}}C^*_t\mathcal{Q}_t^{\frac{1}{\sigma}},$$

with $\mathcal{Q}_t \equiv P_t^*/P_t$ for the real exchange rate.

Goods market clearing conditions in each region are:

$$Y_{H,t} = C_{H,t} + C_{H,t}^* = \phi_H \left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t + \phi_H^* \left(\frac{P_{H,t}^*}{P_t^*}\right)^{-\eta} C_t^*,$$
$$Y_{F,t} = C_{F,t} + C_{F,t}^* = \phi_F \left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t + \phi_F^* \left(\frac{P_{F,t}^*}{P_t^*}\right)^{-\eta} C_t^*.$$

Finally, we close the model by imposing the following monetary policy rule:

$$i_t = \rho_i i_{t-1} + (1-\rho_i)(\phi_\pi \pi_t^{agg} + \phi_y \hat{y}_t^{agg}),$$

where π_t^{agg} is a union-wide inflation rate and $\hat{y}_t^{agg}$ is union-wide output gap.

Modelling Stay-at-Home Orders

We model the imposition of SAH orders in two ways: (i) as a local supply shock, and (ii) as a local demand shock. When we model the SAH as a local productivity shock, we introduce the negative productivity shock for intermediate-goods-producing firms by setting negative values for ϵ_t^A. Alternatively, we also model the imposition of SAH orders via a negative preference shock, since SAH orders may directly reduce consumption by limiting retail mobility, as discussed in Subsection C.1. In this case, we introduce negative shocks to ϵ_t^δ.

C.3 Data Appendix

Table C.1 reports all sources used in this paper.

Table C.1: Data Sources

Variable	Source
Initial Unemployment Claims (Accessed 6/17/2020)	FRED (Mnemonic *ICLAIMS, where * indicates state abbreviation)
County Employment Data	BLS `https://www.bls.gov/lau` (Accessed 6/4/2020)
Stay-at-Home Orders (Accessed with *Internet Archive*)	*New York Times* `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html`
Covid Confirmed Cases (Accessed 6/5/2020)	UsaFacts `https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/`
State Excess Deaths (Accessed 6/4/2020)	CDC `https://www.cdc.gov/nchs/nvss/vsrr/covid19/excess_deaths.htm`
Share Age 60 (Accessed 6/16/2020)	Census Bureau `https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-detail.html`
Average UI Replacement Rate (Accessed 6/16/2020)	Department of Labor's Employment and Training Administration `https://oui.doleta.gov/unemploy/ui_replacement_rates.asp`
2016 Trump Vote Share (Accessed 6/17/2020)	*New York Times* `https://www.nytimes.com/elections/2016/results/president`
Work at Home Index	Dingel and Neiman 2020
March Employment Losses for Bartik (Accessed 4/10/2020)	BLS `https://download.bls.gov/pub/time.series/ce/ce.industry`
Google Mobility Reports (Accessed 5/21/2020)	`https://www.google.com/covid19/mobility/`
Daily Consumer Spending and Employment	Track the Recovery `https://tracktherecovery.org`
State Non-Essential Business Closure Dates	Kong and Prinz 2020