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Tradable Spillovers of Fiscal Policy: Evidence from the 2009 Recovery Act

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Abstract

Local fiscal policy shocks propagate between labor markets through the trade in intermediate goods used in final production. Through this channel, each $1 of local aid from the 2009 Recovery Act increased output by $1.33 in the rest of the country over two years, in addition to its local state-level effect of $1.46. Combining both the local and spillover effects, absent other offsetting forces, the implied aggregate multiplier from the Recovery Act was approximately 2.8. A sectoral decomposition of the direct and spillover effects is consistent with the spillover effects being mediated through the trade in intermediate goods.

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When traditional monetary policy became constrained by the zero lower bound during the Great Recession, policymakers in the U.S. turned to fiscal policy in an effort to stabilize the economy: In February of 2009, Congress passed the American Recovery and Reinvestment Act (ARRA; Recovery Act) of 2009 for an estimated budgetary impact of $830 billion, according to a 2014 CBO estimate.[1] The expressed purpose of the act was "To preserve and create jobs and promote economic recovery" and "To assist those most impacted by the recession." Whether and to what extent this intervention achieved its goals has implications both for the design of fiscal policy and for our understanding of the aggregate economy.

To study the effects of the Recovery Act, researchers have typically exploited features of the Act that required spending to be apportioned across the country according to formulary rules or pre-recession observables. Equipped with this plausibly exogenous variation in government spending at the county, labor market, or state level, economists have estimated the number of jobs created or saved in regions receiving more aid relative to those regions receiving less aid. While useful for understanding the *local* effects of this policy, and fiscal policy more generally, most papers in this literature recognize that geographic variation in ARRA spending alone cannot pin down its *aggregate* effect. That is, while geographic variation may be useful for estimating the local multiplier of fiscal policy, it is less clear how it relates to the aggregate multiplier.

In this paper, I present empirical evidence of the economic significance of an underappreciated channel by which the local multiplier differs from the aggregate multiplier: trade in intermediate goods used in final production.[2] Using the 2007 Commodity Flow Survey (CFS), I first calculate the extent to which each U.S. state was differentially exposed to Recovery Act spending elsewhere in the country.[3] 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 four key findings.

My first finding is that each additional $1 of fiscal spending in a given state increased output in the rest of the country by $1.33 (SE: 0.16) over two years.[4] This finding is robust to the inclusion/exclusion of state fixed effects, time fixed effects, direct aid received by the state, and lags of variables included in estimation. As far as I know, this is the first paper

[1] See https://www.cbo.gov/publication/45122.

[2] In reviewing this 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 a lower bound on the closed economy, zero-lower bound, aggregate multiplier.

[3] 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.

[4] Standard errors are estimated following Driscoll and Kraay (1998) to account for spatial and temporal correlation of the error terms. Instead clustering by state and ignoring the spatial covariance structure yields larger standard errors; however, the baseline estimates continue to be statistically significant.

to document cross-state output spillovers of fiscal policy arising from the Recovery Act. My estimate of the direct effect is \$1.46 (SE: 0.43) over two years. Absent other offsetting forces, the implied aggregate multiplier from the Recovery Act was approximately 2.80.

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.

Chodorow-Reich (2019) reports that the mean cross-study estimate of the jobs-year multiplier from ARRA spending is approximately 18 job-years per \$1 million of aid. Thus, these spillover effects are approximately one third the size of the direct effect of aid previously estimated in the literature, providing further evidence that, at least in the case of the Recovery Act, local multiplier estimates understate the aggregate effect of government spending.[5]

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. This result is consistent with the construction of the spillover exposure from the Commodity Flow Survey and lends further support to the underlying mechanism of trade in intermediate goods as being an important propagation mechanism of shocks from one region to the rest of the country.

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 investigate the plausibility of my identifying assumption by estimating an an event-study style specification. In particular, for this robustness exercise, I assume that the distribution of and exposure to ARRA funding was determined in the quarter in which the act was passed.[6] This exercise isolates the cross-sectional variation in spillover exposure in identifying the effects. I find that those states most highly exposed to spending elsewhere in the country exhibit similar output and unemployment growth trajectories prior to the passage of the Recovery Act but divergent trends following. In the employment

[5] Dupor and McCrory (2018) find evidence of considerable spillover effects of the Recovery Act between counties of the same local labor market. However, that paper fails to address cross-labor market spillover effects of the Recovery Act arising from the trade in intermediate goods, an issue directly addressed in this paper.

[6] In part, this exercise is also intended to address timing concerns, an issue prominently discussed in Ramey (2011).

specification, I find suggestive evidence that my benchmark estimates may be downwardly biased.

Associated Literature

This paper is most closely connected to the local fiscal multiplier literature, which uses geographic variation to study the effects of government spending on the economy. Since the allocation of government spending is likely to be endogenous to local economic conditions, papers in this literature rely upon the historical, regulatory, and institutional determinants of spending to locate plausibly exogenous variation.[7]

In the case of the Recovery Act of 2009, the reliance upon pre-recession formulary rules to apportion stimulus funds has been used to study its effect. For example, Chodorow-Reich et al. (2012) study the effects of fiscal relief to state governments that was provided through an expansion to Medicaid reimbursements, determined in part by pre-recession levels of Medicaid spending. Studying the effects of this aid upon employment, the authors find that the cost of creating one job during the Great Recession was approximately $26 thousand. In contrast, Wilson (2012) studies the effect upon employment of the entire spending component of the Recovery Act. To overcome the endogeneity of spending to local economic conditions, Wilson (2012) uses a battery of instruments constructed according to the formulary allocation rules explicitly stated in the language of the Act. Wilson (2012) finds that the cost of creating one job was approximately $125 thousand. Harmonizing these results with other papers studying the economic effects of the Recovery Act, Chodorow-Reich (2019) finds a cross-study mean of approximately 1.8 job years per $100 thousand dollars.[8]

A subset of this local multiplier literature uses cross-sectional variation in government spending and economic outcomes to discipline fully-specified dynamic, stochastic, general equilibrium models. This is the approach used by Nakamura and Steinsson (2014), who estimate the relative output multiplier on government spending, identified from differential state-level exposure to military build-ups and drawdowns. Empirically, Nakamura and Steinsson estimate that the two-year local multiplier on output is approximately 1.5.[9] In a two-region, open economy, monetary union model, they find that that the frictionless

[7]A particularly novel example of this approach is Suárez Serrato and Wingender (2016), who cleverly use discrepancies in estimated and actual population at the county level to identify exogenous variation in spending. So long as these measurement errors are orthogonal to local economic conditions, the revision-induced changes in spending may be used to study the effects of government spending on the local economy. These authors find that 3.25 jobs are created for every $100 thousand. See Chodorow-Reich (2019) for additional papers in the local multiplier literature using non-ARRA spending to estimate the local fiscal multiplier.

[8]Other Recovery Act papers reviewed by Chodorow-Reich not already mentioned include Dube et al. (2018) and Feyrer and Sacerdote (2012).

[9]Their estimates are consistent with my own estimates of the direct, 2-year cumulative output multiplier.

neoclassical model predicts a counterfactually low local multiplier. A New Keynesian model with nominal rigidities and labor-consumption complementarities does better at matching this moment. Dupor 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.

As was discussed above, even 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.[10] 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 – estimates consistent with the view that government purchases of goods and services tends to crowd-out private production.[11]

This finding makes sense in the context of a neoclassical real business cycle model in which government spending increases output through its effect on labor supply. Government spending represents a reduction in the household's net present value of wealth since taxes must eventually be levied to finance the spending. Households optimally respond by simultaneously increasing their labor supply and reducing their consumption, leading to an increase in output that is less than one for one with government spending. More generally, frictionless dynamic general equilibrium tend to predict output multipliers lower than 1 (See, for example, Baxter and King (1993), Aiyagari (1994), Christiano et al. (2011), and Ramey (2011)).

Auerbach and Gorodnichenko (2012) were among the first to present empirical evidence that the fiscal multiplier is state-dependent, arguing that when there is slack in the economy that the fiscal multiplier between 1 and 1.5. Dube et al. (2018) argue that, in the context of the Recovery Act, counties with excess capacity saw larger employment responses to fiscal stimulus. My finding of considerable spillover effects of fiscal policy between U.S. states, mediated by the trade in intermediate goods, is additional evidence of this channel, particular since the labor market effects appear to be driven by a reduction in relative unemployment.

A fourth literature to which this paper is related is the literature on the effects of fiscal policy in an open economy. (e.g. Auerbach and Gorodnichenko (2013), Ilzetzki et al. (2013), Galí and Monacelli (2008), Nakamura and Steinsson (2014) and Farhi and Werning (2016)).

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

[11] 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 its financed, the responsiveness of monetary policy, and the differential impact the spending has on households of varying levels of financial constraints.

The general equilibrium responses in an open economy setting that are not well captured by a cross-sectional analysis can be quite important. Empirically, Ilzetzki et al. (2013) show that the estimated effects of fiscal policy shocks are larger among closed economies than among open economies.

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 direct, empirical evidence in support of this claim.

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.[12] 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.[13]

The following section outlines the data used in the analysis. Section II presents the empirical specification and benchmark results. In Online Appendix C, I assess the robustness of my empirical results. Section III concludes.

I Data

I.1 Commodity Flow Survey

To 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

[12] See, for example, Hulten (1978), Acemoglu et al. (2012), Baqaee (2018), and Baqaee and Farhi (2017). Stumpner (2017) uses the CFS to study the geographic spread of demand shocks during the Great Recession.

[13] For example, in a stylized production network model, Acemoglu et al. (2016) show that, with limited relative price changes, that 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.

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)."[14] 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, as pointed about above, Hillberry and Hummels (2003) present evidence that shipments between states are primarily between manufacturers and wholesalers, suggesting that these flows capture primarily the shipment of intermediate goods. Note that the CFS also includes shipments between establishments within the 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.[15] These import shares will be combined with data on government spending to construct a *spillover treatment* variable for each state.

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

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

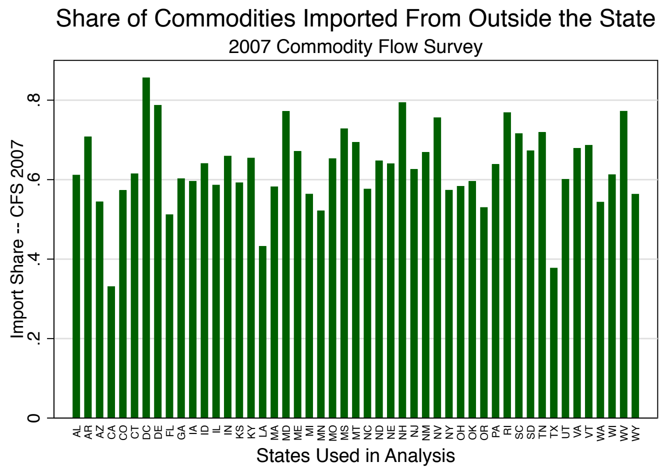
I.2 Recovery Act Data

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 pro-

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

[15] The commodity flow survey also reports commodities shipped between locations within the same state.

Figure 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 $w_{i,i} = 0$.

vide a weekly detailed report, entitled the Financial and Activity Report, in which the value of obligations and payments for 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.

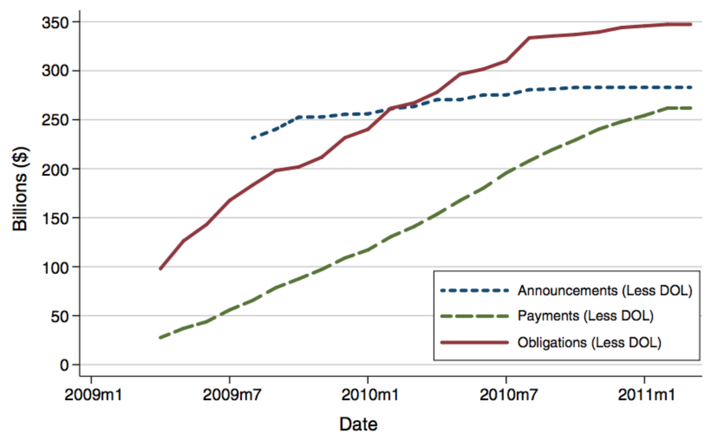
Payments, also reported in the weekly reports, correspond to when funds were actually transferred between the government and the recipient.

I use the state-level obligations series constructed by Wilson (2012). Reported in Figure 2 are three measures of ARRA spending over time, from April 2009 through March 2011.[16]

I include these shipments in the denominator of $w_{i,j}$. Thus, $\textit{Inbound-Shipments}_j \equiv \sum_k imports_{j \leftarrow k}$.

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

Figure 2: ARRA Spending Measures over Time From Wilson (2012)



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. Three agencies represent the majority of Recovery Act spending: Health and Human Services (27%), Department of Education (22%), Department of Labor (15%), and Department of Transportation (10%).[17]

I use $ARRA^D_{i,t}$ to indicate the cumulative dollar value of Recovery Act obligations directly made to 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.

I.3 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 $ARRA^D_t$:

$$ARRA^S_t = \mathbf{W} \times ARRA^D_t \tag{1}$$

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:

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

[17]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

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-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. That is, $\mathbf{w}_i$ collects the share of commodities imported by states $j \neq i$ from state i. This vector would be equal to zeros if no state imported commodities from state i.

In principle, the sum of the elements of the $\mathbf{w}_i$ vector 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 A.2 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 each other state 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 regional import/export network. Higher values imply that those states play a more central role in the regional production network.[18]

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 A.2 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 centrality of the state in the state import/export network.[19]

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exposed. Thus, for completeness, I include DOL obligations when I calculate how much each state was exposed to spending elsewhere in the country.

[18]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.

[19]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;

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 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 A.2, 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.[20]

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

$$\frac{\Delta ARRA^S_{i,t}}{GSP_{i,t-1}} = \frac{ARRA^S_{i,t} - ARRA^S_{i,t-1}}{GSP_{i,t-1}},$$

which is the value of *additional* import-weighted obligations to which state i was exposed in quarter t relative to output in the prior quarter.

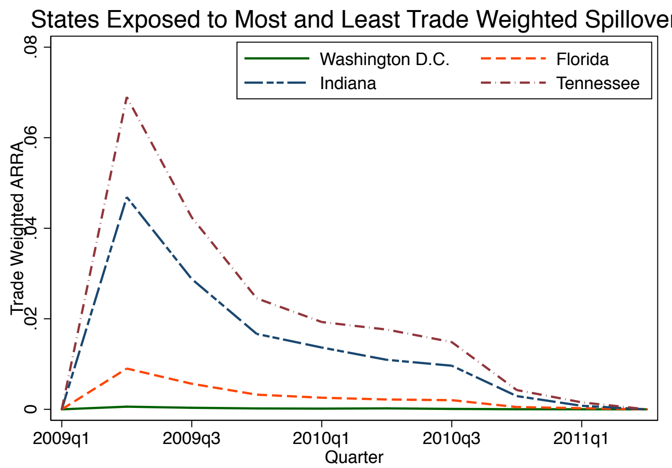
Figure 3 plots the time series of this variable for the two states most exposed to trade-weighted obligations in 2009Q2 relative to output (Tennessee and Indiana) and the two states least exposed (Florida and Washington D.C.). In all cases, this trade-weighted spillover variable attains its maximum in 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.

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

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.

[20]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.

Figure 3: Differences in the Path of $\frac{ARRA^S_{i,t}-ARRA^S_{i,t-1}}{GSP_{i,t-1}}$ for States with Highest and Lowest Values in 2009Q2



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.

I.4 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.[21] Seasonally adjusted employment and unemployment data for each state were acquired from the Bureau of Labor Statistics.

II Estimation and Results

II.1 Empirical Specification

To determine the spillover effect of fiscal policy upon an outcome variable Y, I estimate a series of local projections for horizons $h = 0, \dots, 11$.[22] The benchmark set of equations that I estimate on the panel data are of the following form:

$$\frac{Y_{i,t+h} - Y_{i,t-1}}{GSP_{i,t-1}} = \theta_{i,h} + \eta_{t,h} + \beta^Y_h \frac{\Delta ARRA^S_{i,t}}{GSP_{i,t-1}} + \alpha^Y_h \frac{\Delta ARRA^D_{i,t}}{GSP_{i,t-1}} + X_{i,t}\Gamma_h + \varepsilon_{i,h,t}, \tag{2}$$

[21]The GSP series are in chained 2009 dollars and are seasonally adjusted by the BEA.
[22]See Jordà (2005).

where $GSP_{i,t}$ is the gross state product in state i in quarter t, $ARRA^D_{i,t}$ is the cumulative value of Recovery Act obligations to state i through quarter t, and $ARRA^S_{i,t}$ is the spillover treatment to which state i was exposed to in quarter t (see construction above), with Δ indicating the time difference of the 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 $\frac{\Delta ARRA^S_{i,t}}{GSP_{i,t-1}}$, and four lags of $\frac{\Delta ARRA^D_{i,t}}{GSP_{i,t-1}}$.[23]

The coefficients of interest are $\{\beta^Y_h\}^{11}_{h=0}$, 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 shock. Specifically, the K-quarter cumulative exposure multiplier is given by:

$$\phi^{S,Y}_K \equiv \frac{\sum_{h=0}^{K-1} \beta^Y_h}{\sum_{h=0}^{K-1} \beta^{ARRA^S}_h}$$

where $\beta^{ARRA^S}_h$ is the cumulative impulse response of the spillover measure of K quarters, which I also estimate according to equation (2).

The interpretation of $\phi^{S,Y}_K$ 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 (2) with the accumulated change in the outcome variable of the relevant horizon and similarly replacing $\frac{\Delta ARRA^S_{i,t}}{GSP_{i,t-1}}$ with the cumulative increase in obligations over the same period.

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

$$\phi^{D,Y}_K \equiv \frac{\sum_{h=0}^{K-1} \alpha^Y_h}{\sum_{h=0}^{K-1} \alpha^{ARRA^D}_h}$$

When presenting my results below, I directly estimate $\phi^{S,Y}_K$ by running the following specification[24]:

[23] 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.

[24] Of course, this is just the long-difference between $t-1$ and $t+K$ of $ARRA_{i,t}$, which captures the cumulative value of new obligation spending between $t-1$ and $t+K$. Writing it this way makes clear that this variable is the accumulated value of new obligations.

$$\sum_{h=0}^{K-1} \frac{Y_{i,t+h} - Y_{i,t-1}}{GSP_{i,t-1}} = \phi_K^{S,Y} \sum_{h=0}^{K-1} \left(\frac{\Delta ARRA^S_{i,t+h}}{GSP_{i,t-1}} \right) \mathbf{1}(t \geq 2009Q2) + \phi_K^{D,Y} \sum_{h=0}^{K-1} \left(\frac{\Delta ARRA^D_{i,t+h}}{GSP_{i,t-1}} \right) \mathbf{1}(t \geq 2009Q2) + X_{i,t}\Gamma_K + \epsilon_{i,K,t} \tag{3}$$

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 for 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) in a single step.

I estimate the model using data from 2006Q2 to 2015Q1. The benchmark equations report Driscoll and Kraay (1998) standard errors, which allow for general forms of spatial and temporal dependence of the error terms $\varepsilon_{i,t,h}$.

Summary statistics as of 2009Q1 of the variables used to estimate equations (2) and (3) are reported in Table A.1. 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.

II.2 Assessing the Identifying Assumption

II.2.1 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 are not randomly assigning 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 (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 in the previous section, 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.[25] 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.[26]

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 of import-weighted obligations to which each state was exposed relative to the state's pre-recession level of output, observed in 2005: $Z_i = \frac{ARRA^S_{i,2011Q2}}{GSP_{2005}}$.[27] 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 4.

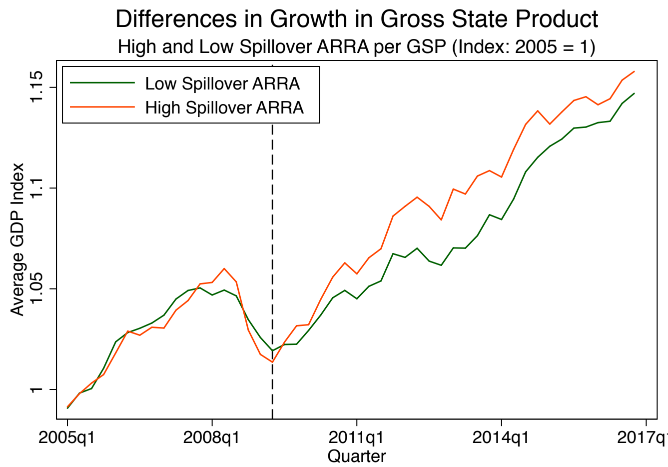
The reason for choosing 2005Q1 as the base quarter is to highlight two facts: First, in the two years prior to the passage of the Recovery Act the growth path of output in these two groups was very comparable prior to and during the early stages of the recession; Second, both groups reached the nadir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

[25]Boone et al. (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.

[26]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.

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

Figure 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.

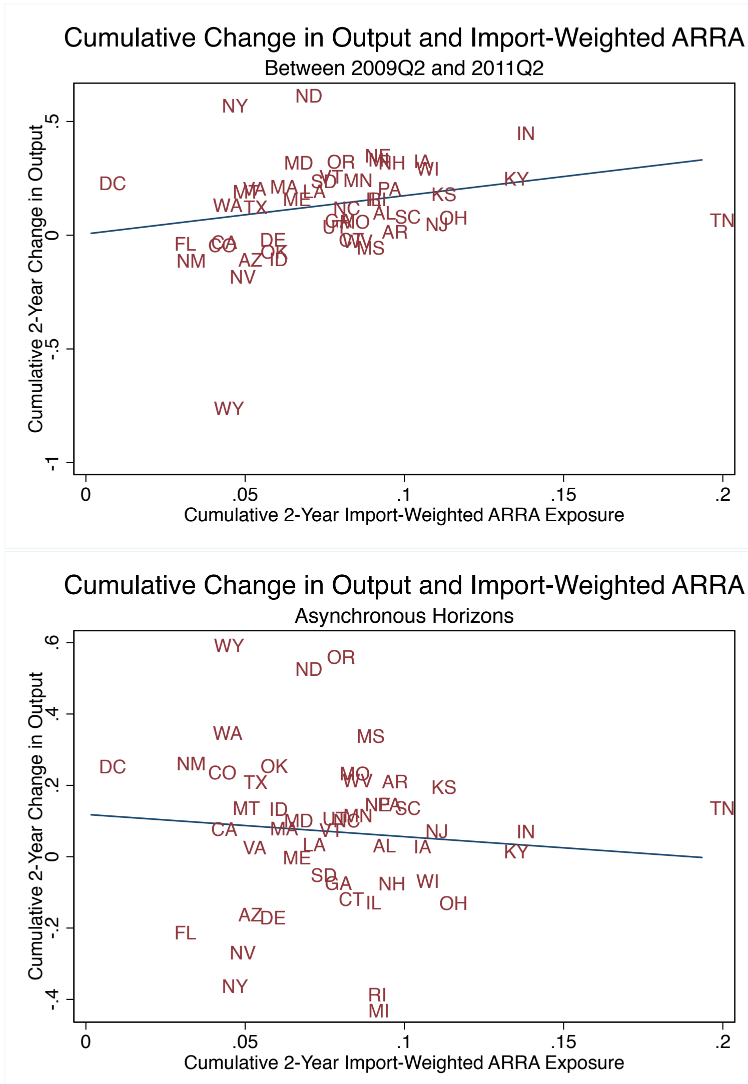
- Each line corresponds to the average within-group average of real GSP, after re-indexing each state's GSP to its level as of 2005Q1.

group was considerably faster than that in the control group. The common pre-trends in state-level output in the two years prior to the act and the subsequent divergence in outcomes following the passage of the Recovery Act is further evidence that the identifying assumption holds.[28]

The top panel of Figure 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 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.

[28]Consistent with evidence of local hysteresis in labor markets presented in Yagan (Forthcoming), this plot suggests that the spillover effects were extremely long-lived. Nevertheless, over longer horizons, one may suspect that factor reallocation of capital and labor may produce persistent relative differences in output growth. Investigating whether the long-run relative differences in outcomes arising from spillovers is due to local employment hysteresis or factor reallocation is beyond the scope of this paper.

Figure 5: Scatter Plots of Cumulative Output Change and Cumulative ARRA Exposure Over 2 Years Following Passage of Recovery Act



II.3 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 estimating equations are given by Equation (2), reprinted here for convenience:

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

Figure 6a plots the estimated effect on output (GSP) of a \$1 innovation to import-weighted obligations over 12 quarters, including the impact quarter: $\{\hat{\beta}_h^{GSP}\}_{h=0}^{11}$. 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 $ARRA_{i,t}^S$. 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 $ARRA_{i,t}^S$.

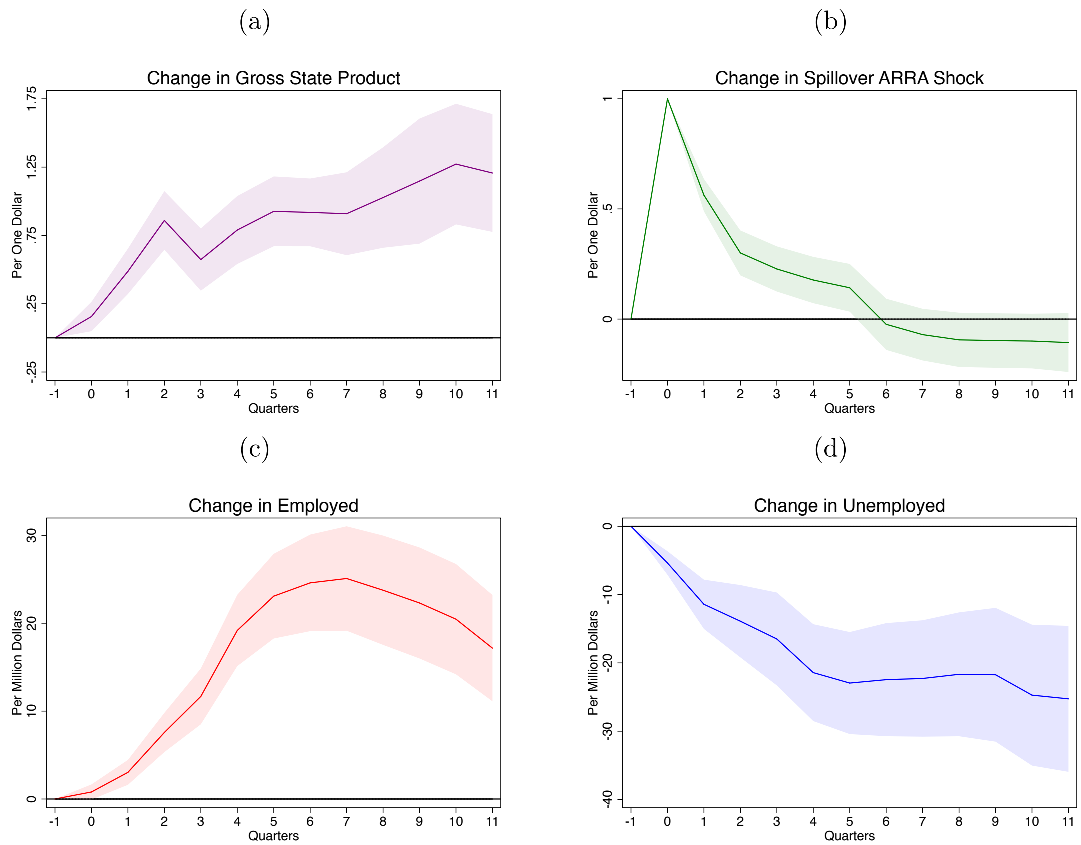
However, to properly scale this effect on output, we need to know the persistence of innovations to import-weighted obligations. Figure 3 suggests that import-weighted obligations, $ARRA_{i,t}^S$, have a strong auto-regressive component, even after controlling for other factors. Indeed, Figure 6b reports the impulse response of import-weighted obligations, $ARRA_{i,t}^S$ to a one dollar innovation to $ARRA_{i,t}^S$. Specifically, it plots the estimated coefficients $\{\hat{\beta}_h^{ARRA^S}\}_{h=0}^{11}$ from Equation (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 \$2.33.

In Table 1 I report the estimates of $\hat{\phi}_8^S$ when estimating the model in a single step according to Equation (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 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 add in 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.[29]

Figure 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 $ARRA^S$.

[29]In these specifications, and in the results presented in the rest of the paper, standard errors are constructed according to Driscoll and Kraay (1998), which allow for general forms of spatial and temporal dependence of the error terms. In my case, these standard errors tend to be smaller relative to those constructed with heteroskedasticity consistent standard errors clustered by state. Table C.1 and Table C.2 report the counterparts to Tables 1 and 2 with the heteroskedasticity consistent standard errors clustered by state. Running a Pesaran (2004) cross-sectional dependence test on the residuals from Equation (2) strongly rejects the null hypothesis that the residuals are cross-sectionally uncorrelated. I implement the estimation using the Stata package *xtscc* and the Pesaran (2004) test using the package *xtcsd*. See Hoechle (2007) for more details.

Table 1: Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Gross State Product: Varying Controls

	Bivariate b/se	+ State FEs b/se	+ Quarter FEs b/se	+ Direct ARRA b/se	All Controls 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: $\mathbb{E}_i\left[\bar{\omega}_i\right]$, which is 0.63. This means that, in the context of my empirical specification in Equation (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 $\sum_{j\neq i} ARRA^S_{j,t}$.

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 elsewhere in the country by an additional $1.33.

Table 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, in part because there is growing evidence that the geographic allocation of aid was largely uncorrelated with the economic severity of the downturn, especially after controlling for observables.[30] My estimate of the 2-year cumulative output multiplier of directly received ARRA obligations is $1.46 (SE: 0.43).[31] Thus, this trade-

[30] For more on this point, see Boone et al. (2014) and Dube et al. (2018).

[31] This is consistent with Chodorow-Reich (2019) who, using only cross-sectional variation and a battery of instruments, estimates a 2-year multiplier of 1.53 (SE: 1.19).

channel of fiscal policy is quantitatively significant, representing approximately 60% of the estimated local effect.

Absent other forces, this result suggests that the cross-sectional local multiplier estimated using the local expenditure component of the Recovery Act is indeed a lower bound relative to the aggregate multiplier. Over two years, the implied aggregate multiplier of the Recovery Act was approximately 2.80.

II.4 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 upon 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.[32]

Complementing the results for employment, I also estimate the spillover effect upon 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.[33]

Consider first the effects of import-weighted ARRA obligations in all other states upon a particular state's employment. Figure 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 attaining a maximum value of 28 jobs in quarter 7, and then declining slightly.

As with the output estimates, we can calculate the integral of this figure to calculate the cumulative employment effect for every $1 innovation to import-weighted ARRA obligations. Over two years, the cumulative effect is 28.75 job years created or saved. Dividing 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 created is thus approximately $133K.

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

[33] 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.

The unemployment estimates, $\{\hat{\beta}_h^{UR}\}_{h=0}^{11}$, are reported in Figure 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.

II.5 Taking Stock: Cumulative One & Two Year Trade Exposure Multipliers

In this section I summarize my 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) 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.[34] 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 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 economy was $95K and $150K elsewhere in the country. The combined cost of creating a

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

Table 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		
	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	(0.14)	(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.

job anywhere in the country was $58K.

II.6 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) with K equal to 4 and 8 and change the left hand side variable to represent various broad sectors of the economy:

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

As reminder, $\phi_K^{S,Y}$ has the interpretation of the cumulative K-quarter effect on the outcome variable $Y_{i,t}$ 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.[35] The analog coefficient for directly received ARRA obligations, which I'll refer to as ϕ_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 reports the estimated coefficients $\hat{\phi}_K^{S,Y}$ and $\hat{\phi}_K^{D,Y}$ 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 in the rest of the country (column 1) and the one year effect of directly receiving one additionl dollar of aid (column 2). Driscoll and Kraay (1998) standard errors are reported below each point estimate.

For example, 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, are consistent with the spillover effects being mediated by the trade in goods, particularly intermediate goods. For example, as mentioned above, Hillberry and Hummels (2003) reports that the bulk of cross-state trade between states is between manufacturers and wholesalers; trade between wholesalers and retailers tends to be within the state. Over two years, little more than a tenth of the total direct effect (1.46) is through an increase in wholesale trade activity: each \$1 of directly received aid over two years leads to increased wholesale trade production of \$0.19 (SE: 0.07). As would be expected, there is no discernible spillover effect on wholesale trade.

Adding further support to this channel, the spillover exposure effect on manufacturing and transportation/warehousing activity is larger than the corresponding direct effects. Over two years, each additional \$1 of aid to which a state was exposed led to an increase in manufacturing output of \$1.47 (0.16). In contrast, each \$1 of directly received aid increased

[35] Recall that $\hat{\phi}_K^S$ 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: One and Two Year Cumulative Exposure Multipliers of Recovery Act Spending on Sectoral Output

	Cumulative Spillover ARRA	Cumulative Direct ARRA
	b/se	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.

manufacturing output within the recipient state by only $0.11 (SE: 0.19), though this is statistically insignificant.

II.7 Robustness Exercises

In Section C of the Online 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 restriction that $w_{ii} = 0$ by including own-spending multiplied by self-import shares in the constructrion 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 disproportationely 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 style 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 captures the bulk of indirect exposure.

Finally, to allay any 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. I find no evidence of a pre-trend in output growth among states more indirectly exposed to spending elsewhere in the country. However, there is a sharp effect on output growth following the passage of the Recovery Act, again consistent with the benchmark results.

III Conclusion

This paper presents evidence of 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 allowed me to identify the spillover effects.

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, this implies that, absent other offsetting forces, the aggregate fiscal multiplier from the Recovery Act was approximately 2.80. This result further implies researchers should exercise caution when using cross-sectional variation to draw conclusions about the aggregate effects of policies—in this case fiscal policy.

In terms of employment, I find that the implied spillover cost of creating one job lasting one year was $150,000; together with the local effects of Recovery Act aid, the implied cost per job was $58,000. These results are consistent with models that predict large output effects of fiscal policy when monetary policy is constrained at the zero lower bound and when there is slack in the economy—both prominent features of the economy when the Recovery Act was passed.

In summary, this paper presents novel evidence of the importance of trade linkages in propagating local demand shocks between regions. This finding is valuable not only for our understanding of fiscal policy but also for our understanding of the mechanisms by which shocks propagate through the economy.

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A Additional Tables

Table A.1: 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.

Table A.2: 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.

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B State-Clustered Standard Errors

Table C.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 b/se	+ State FEs b/se	+ Quarter FEs b/se	+ Direct ARRA b/se	All Controls 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 1 because, without state fixed effects, the random effects GLS estimator is invoked.

Table C.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 Spill. ARRA	0.97	2.79**	-5.40***			
	(0.59)	(1.29)	(0.95)			
4-Qtr Ahead ARRA	0.27	3.53**	-2.35*			
	(0.53)	(1.61)	(1.34)			
8-Qtr Ahead Spill. ARRA				2.12*	10.54***	-12.61***
				(1.21)	(3.62)	(2.24)
8-Qtr Ahead ARRA				1.46	10.56**	-6.14*
				(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.

C Robustness Exercises

C.1 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) 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 D.1: Outlier Analysis: Estimated 2-Year Cumulative Spillover Output Multiplier from Dropping Each State

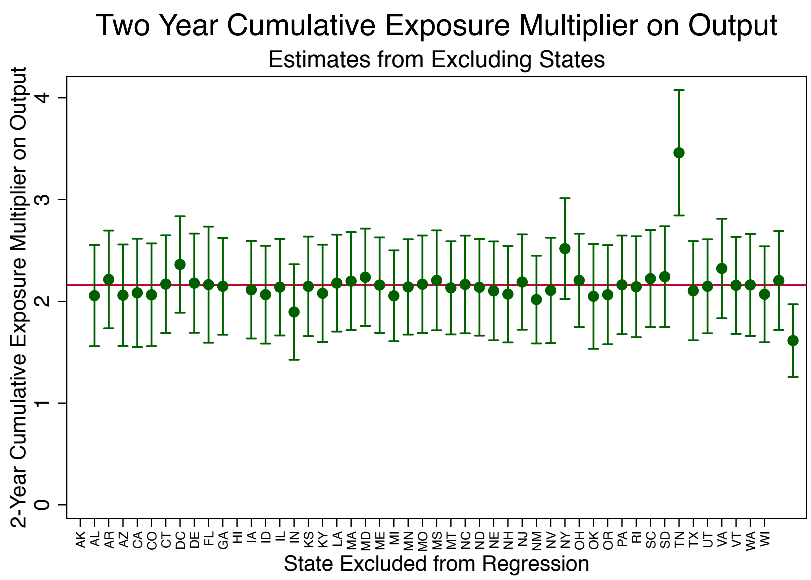


Figure D.1 reports the results from this exercise. The height of each bullet indicates the point estimate when excluding the given state from the sample. 90% confidence intervals are drawn around each bullet point.[36] The solid, red horizontal line indicates the benchmark point estimate of 2.12.

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

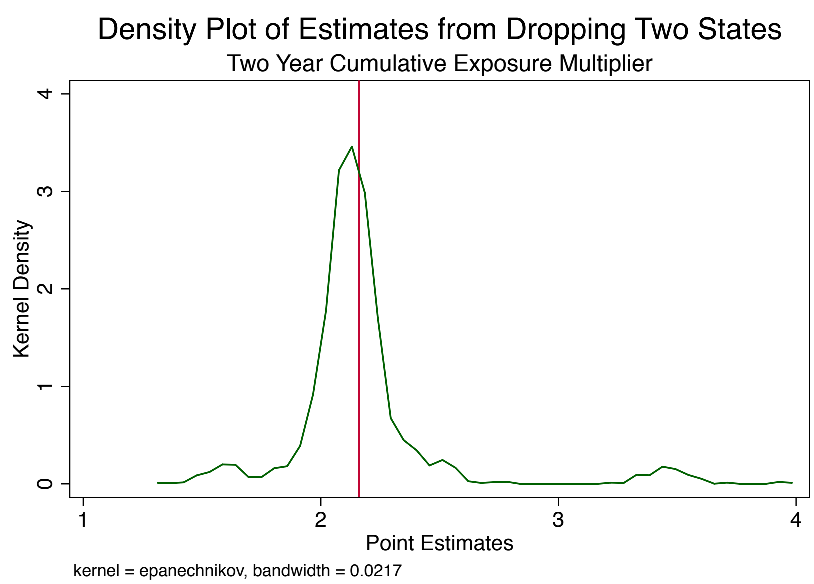
[36]Since Alaska and Hawaii are dropped from the analysis to begin with, there is no bullet point for these

Tennessee, 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 II 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 D.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 D.2: Outlier Analysis: Estimated 2-Year Cumulative Spillover Output Multiplier from Dropping Combinations of Two States



C.2 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

states.

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 (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 D.1, 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 2 in part captures the indirect, local effect mediated by trade within the state.

C.3 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 II. 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), 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

Table D.1: 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.

the nation between 1977 and 2008. For each state, I then separately estimate the following regression:

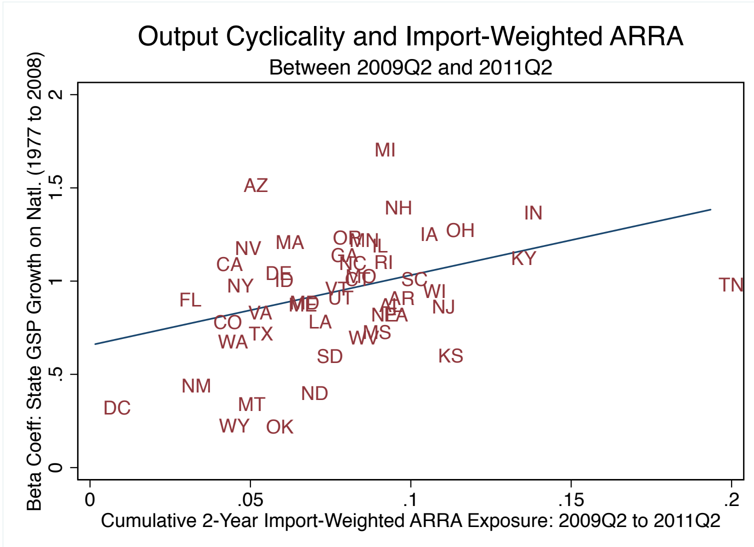
$$\Delta ln(GSP_{i,t}) = \alpha_i + \psi_i \Delta ln(GDP_t) + \epsilon_{i,t} \tag{4}$$

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

Figure D.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.

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



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 $\hat{\psi}_i$:

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

I then estimate Equation 3, 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 D.2. 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

Table D.2: 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.

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. 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.

C.4 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

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

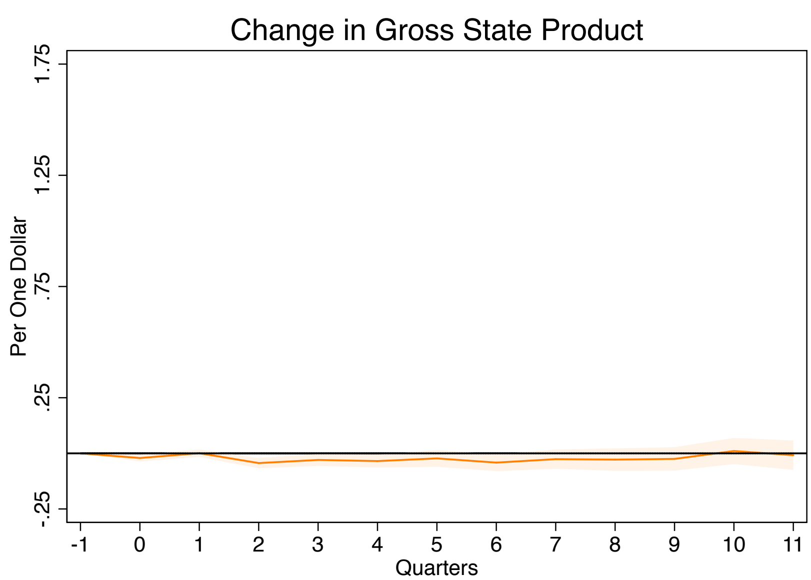
Again, each state's exposure is a weighted sum of spending in all other states, now given by:

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

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 D.4 reports the impulse response of output estimated according to Equation (2), where the only change is replacing $ARRA_{i,t}^S$ variables with $ARRA_{i,t}^{\tilde{S}}$. 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 et al. (2016).

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



C.5 Weighting by Population

In this section, I estimate the benchmark cumulative specifications in equation (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 multiplier effects (either direct or spillover), the unweighted regression will tend to overstate the aggregate multiplier[37]

Table D.3 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 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 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.

[37] 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.

Table D.3: 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	Job-Years	Unemployed -Years	Output	Job-Years	Unemployed -Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead	0.40***	2.82***	-4.70***			
Spill. ARRA	(0.14)	(0.70)	(0.97)			
4-Qtr Ahead	0.96***	2.10	-6.27***			
ARRA	(0.16)	(1.61)	(2.18)			
8-Qtr Ahead				1.29***	9.91***	-11.26***
Spill. ARRA				(0.28)	(1.76)	(2.10)
8-Qtr Ahead				2.50***	5.40	-18.13***
ARRA				(0.38)	(5.35)	(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.

C.6 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, each unit of output produced requires employing $(1-\theta)$ labor locally and purchasing θ 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 trade in intermediate goods, then this change in output may be written as

$$
\begin{aligned}
dy_t &= \tilde{\beta}_d(1-\theta)dg_t + \tilde{\beta}_N\left[(1-\theta)\theta W + (1-\theta)\theta^2 W^2 + \dots\right] dg_t \\
&= \tilde{\beta}_d(1-\theta)dg_t + \tilde{\beta}_N(1-\theta)\theta W[I-\theta W]^{-1} dg_t \\
&= \beta_d dg_t + \beta_N \theta W[I-\theta W]^{-1} dg_t
\end{aligned} \tag{5}
$$

where $\tilde{\beta}_d$ represents the direct effect of higher labor demand that is required satisfy 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 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.[38]

I estimate the empirical analog to Equation (5) 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

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

and re-estimate Equation (3), replacing $ARRA_{i,t+h}^{S}$ with $ARRA_{i,t+h}^{S,L}$. The results of this exercise are reported in Table D.4.

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 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 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

[38] A Long and Plosser (1983) style production network would be one way to rationalize Equation 5. See, for example, Proposition 1 in Acemoglu et al. (2016).

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 being roughly 3.[39]

Table D.4: 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	Job-Years	Unemployed -Years	Output	Job-Years	Unemployed -Years
	b/se	b/se	b/se	b/se	b/se	b/se
4-Qtr Ahead	0.92***	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.

[39] If one takes the stance that the underlying regional production network is such that the Leontief Inverse matrix $\mathbf{W}_L$ accurately captures the relevant increased local labor demand through the trade in intermediate goods, then it would be inappropriate to rescale the spillover exposure coefficient. In this case, absent other offsetting forces, the implied aggregate multiplier from combining the direct and indirect effects is 3.47 (SE:

C.7 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 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.[40] 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:

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

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

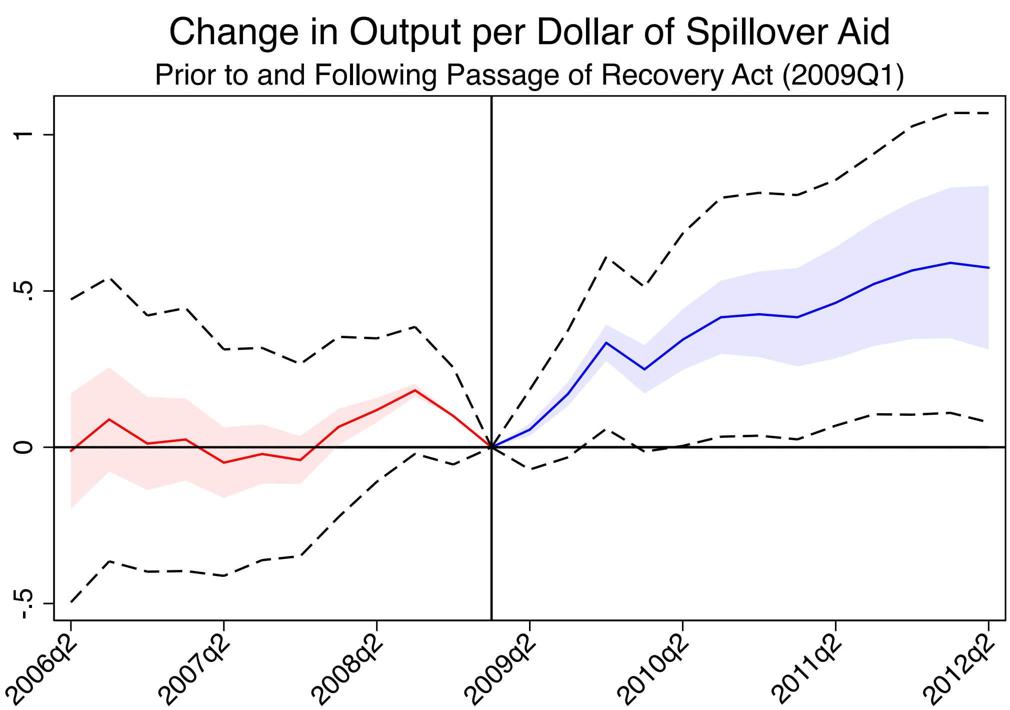
0.61).

[40]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.

the interaction terms—namely, linearity.

The results of this exercise are provided in Figure D.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.

Figure D.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.

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.[41] 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 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 D.6, I repeat the exercise for the labor market variables. Figure D.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.

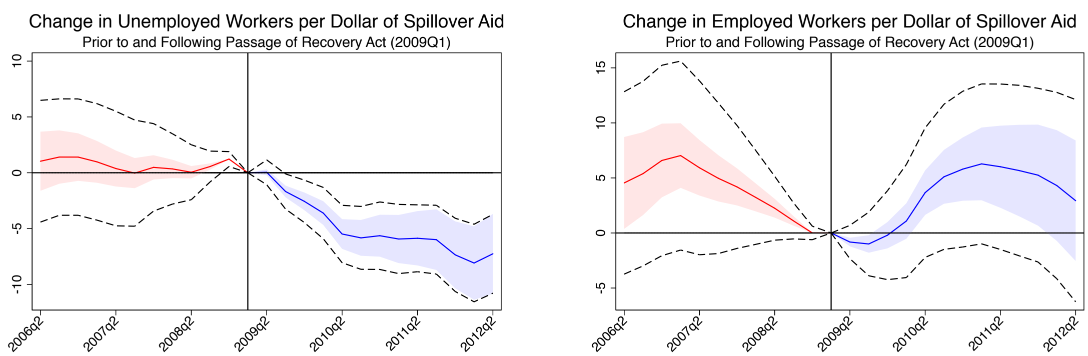
This pattern of a sharp relative response is replicated for employment, with the results presented in Figure D.6b. However, in this figure there is clear evidence of a downward pre-trend in employment among relatively highly exposed states. Nevertheless, there is a stark trend-break in employment growth 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.

[41] 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 4.

Figure D.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.