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Fiscal Policy Spillovers: Points of Employment to Places of Residence*

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Abstract

In this paper, we study the effects of interregional spillovers from the government spending component of the American Recovery and Reinvestment Act of 2009 (the Recovery Act). Using cross-county Census Journey to Work commuting data, we cluster U.S. counties into local labor markets, each of which we further partition into two subregions. We then compare differential labor market outcomes and Recovery Act spending at the regional and subregional levels using instrumental variables. Among pairs of subregions, we find evidence of fiscal policy spillovers. For example, \$1 of Recovery Act spending in a large subregion increases its own wage bill by \$0.79 and increases the wage bill in its neighboring subregion by \$0.59. We find similar spillover effects when we replace the wage bill with employment as our measure of economic activity. Next, we build a dynamic equilibrium trade model with interregional commuting capable of propagating these spillovers across regions.

Keywords: fiscal policy, spillovers, the American Recovery and Reinvestment Act.

JEL Codes: E52, E62.

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1 Introduction

In response to the 2007-09 recession, the U.S. government enacted the American Recovery and Reinvestment Act of 2009 (hereafter, Recovery Act). The Recovery Act was the largest countercyclical fiscal intervention in the U.S. since FDR's New Deal. The law's total budget impact was $840 billion. Drautzburg and Uhlig (2013) report that $350 billion of this amount constituted government purchases of goods and services.[1]

The act was a massive commitment from the federal government to many sectors of the economy, including highway infrastructure, energy and education. For example, the U.S. Department of Education distributed $94 billion in Recovery Act spending, which equals nearly $2,000 per elementary/secondary public school student.

In this paper, we estimate the extent to which the Recovery Act increased local economic activity as well as how this impact propagated itself geographically. Our starting point is the observation that roughly 34% of workers in a typical county are employed outside their county of residence.[2]

As an example, of the 1 million workers residing in Brooklyn (Kings County, NY), 50% are employed in a different county. Seventy-four percent of these commuters from Brooklyn work in nearby Manhattan (New York County, NY), representing approximately one of every five workers in Manhattan. Not only do a sizable number of Brooklyn residents work in Manhattan, these commuters comprise a substantial portion of all Manhattan workers. The degree of economic interdependence between these New York City boroughs regions is high.

Suppose the federal government increased its purchases in Manhattan. Since many Brooklyn residents earn their income in Manhattan, presumably these commuters would spend a significant part of their income in their home county. The government purchases in Manhattan could in turn increase consumer purchases, and other measures of economic activity, in Brooklyn. This potential for cross-county economic interdependence motivates our analysis and our methodological approach.

Using county-level job commuting data, we organize the U.S. into 601 distinct local labor markets, or regions. We then partition each of these regions into two subregions: a large county subregion and a satellite subregion; the latter is the agglomeration of all of the remaining counties within the region. We then ask: how government spending in one subregion affects its own economic activity as well as in its partner subregion?

We measure economic activity in each subregion by its employment level and wage bill.[3] We measure counter-cyclical government spending using quarterly reports filed by over 570,000 recipients (businesses, nonfederal government agencies and nonprofit organizations) of Recovery Act funds. These reports provide zip-code-level detail on spending, allowing us to execute a highly disaggregated analysis.

[1] The remainder reflected direct entitlement payments to individuals and tax cuts.
[2] Authors' calculation using county-to-county commuting flows reported in the American Community Survey.
[3] Unfortunately, data on gross domestic product and its components are not available at the county level.

We use instrumental variables to address potential endogenity in the allocation of Recovery Act spending. Specifically, we use the highway infrastructure component of the Recovery Act as our instrument. In distributing federal dollars to states, most funds were assigned to states according to factors established before the recession, such as each state's pre-recession highway usage and its number of highway lane miles.[4] For each state, spending was distributed among its counties largely according to its infrastructure needs, rather than the business cycle conditions in particular counties.[5]

Comparing differences in labor market outcomes with differences in Recovery Act spending across observations, we construct average treatment effects for both a subregion's own spending and spending in a neighboring, economically interdependent subregion.

We have four main findings. First, there are substantial spillovers from spending in large counties to nearby counties. In particular, \$1 of spending in a large county subregion increases its own wage bill by \$0.79 (SE = 0.23) and increases the wage bill in the rest of the region (i.e., the satellite subregion) by \$0.59 (SE = 0.19). Second, there are large spillovers from a satellite subregion to its nearby large county neighbor but little direct effect on the wage bill in the satellite subregion itself. Our benchmark result is that \$1 of spending in a satellite subregion has no statistically significant effect on its own wage bill, but increases the wage bill in the corresponding large county subregion by \$1.19 (SE = 0.37). Third, we find similar effects when we replace the wage bill with the level of employment as our measure of economic activity. Combining the direct and spillover effects of this spending, we estimate that the Recovery Act increased employment at a cost of \$63,700 per job-year. Fourth, we present a dynamic equilibrium trade model with interregional commuting capable of qualitatively capturing some of these spillover effects.

Our paper relates to two lines of research. First, our general methodology follows other studies that use cross-sectional instrumental variable techniques to estimate the economic impact of spending at the subnational level. These include Shoag (2012), who studies the effect of unanticipated capital gains to government pension funds, and Clemens and Miran (2012), who use differences in state balanced budget requirements to identify the effects of fiscal stabilization policy. A few papers also apply this general methodology to the Recovery Act episode. These include Chodorow-Reich et al. (2012), Conley and Dupor (2013), Dupor (2013) and Wilson (2012). Each of these papers, as well as our paper, uses the observation that a portion of Recovery Act dollars was allocated according to formulary rules, which serves to overcome endogenity concerns.

Second, our paper is related to research on fiscal policy spillovers. Carlino and Inman (2013) study a panel of U.S. states and show that an exogenous increase in one state's deficit can generate increased employment in neighboring states. Beetsma and Giuliodori (2011) apply short-run restrictions to identify the effects of government spending shocks in the European Union using a structural vector autoregression. They find that an exogenous spending shock in a large European

[4] See Conley and Dupor (2013).
[5] See Dupor and Mehkari (2014).

Union country increases output in other union member countries.

The outline of the paper follows. Section 2 presents the data and our econometric model. Section 4 presents a dynamic economic model to motivate and focus our empirical work. Section 3 provides our benchmark results as well as robustness checks. The model features region-specific government spending shocks and interregion worker commuting. The final section concludes.

2 Empirical Analysis

2.1 The Data

The local labor market

We begin by defining a local labor market as a set of counties with an interdependent economic structure. We use cross-county commuting patterns to identify economic spatial dependence among counties and spatial economic independence across local labor markets. This approach incorporates potential cross-region commuting, analogous to that in our theoretical model in Section 4. We identify local labor markets with two conditions in mind: within-market dependence and across-market independence. Hereafter, we refer to local labor markets as regions or regional markets, interchangeably.

Following a methodology similar to that of Tolbert and Sizer (1996), we use an agglomerative hierarchical clustering technique to identify such independent regional markets.[6] This approach makes the implicit assumption that commuting patterns are good proxies for economic interdependence. The procedure for identifying the regions follows.

First, we construct a single nationwide pairwise flow matrix from county-to-county commuting data (the 2000 Journey to Work survey) that measures relative distance between any two counties. We define economic distance between county i and county j as

$$D_{i,j} = D_{j,i} = 1 - \frac{(C_{i,j}) + (C_{j,i})}{min(LF_i, LF_j)} \tag{2.1}$$

where $C_{i,j}$ indicates the number of commuters from counties i to j. LF_i refers to the employed resident labor force in county i.[7] With this measure of economic distance, we use the average linkage algorithm to map the pairwise flow matrix to x clusters of counties (regional markets)—with x dependent on a threshold parameter, γ, that indicates the average distance between clusters. By lowering the average distance between regions, we increase the number of regions identified. Likewise, by increasing the average distance between regions, we can group together increasingly

[6] These are referred to as commuting zones in their paper. For an example of an application of the commuting zone approach, see Autor, Dorn and Hanson (2013) and Chetty et al. (2014).

[7] Our measure of distance differs slightly from that of Tolbert and Sizer (1996) in that we use the *employed* resident labor force, whereas they use the entire resident labor force. We also use a nationwide flow matrix rather than overlapping regional matrices to identify clusters.

disparate, less-economically interdependent counties.

The choice of γ balances two possible costs: (i) setting γ too high, in which case one might fail to group together counties that are, in fact, economically interdependent or (ii) setting γ too low, in which case one might incorrectly conclude that some set of relatively isolated counties together form a single regional market.[8]

Partitioning the local labor market

First, we are interested in the overall regional effect of an injection of exogenous federal spending and the geographic spillover of that effect. As such, a more systematic discussion of the regional markets is in order. Note that when referring to a particular set of identified regions, we use LM_J, where J indicates the total number of regions in the set. Our baseline identification is the set denoted by LM_{601}.[9] Unless otherwise noted, results are from our baseline identification. For each value of γ (identifying a specific set, LM_J), we construct regional market variables in the three following ways.

First, we construct variables at the regional market level. That is, we take the level values from the constituent counties (or county) of an identified region and combine them as befitting the form needed. For instance, for N counties in region j, the natural log of the population for market j is given by $lnpop_j = \log\left(\sum_{n \in N} Population_n\right)$. *Mutatis mutandis* are other requisite variables constructed. Variables observed at the regional level are identified by the superscript A. Unless otherwise noted, each variable is constructed from the aggregate of constituent county values.

Second, *we further subdivide each region into a pair of subregions.*[10] Each pair consists of the largest county and the sum of the remaining counties in the region. We denote variables pertaining to the largest county in a particular region with the superscript L and refer to them collectively as the large county subregions.

Finally, the satellite counties in the region—that is, excluding the largest county—constitute the last level of observation. Just as for the regional level, A, variables are observed for each of the satellite counties and combined to form satellite subregion variables. We use the superscript O to refer to this set within a region.

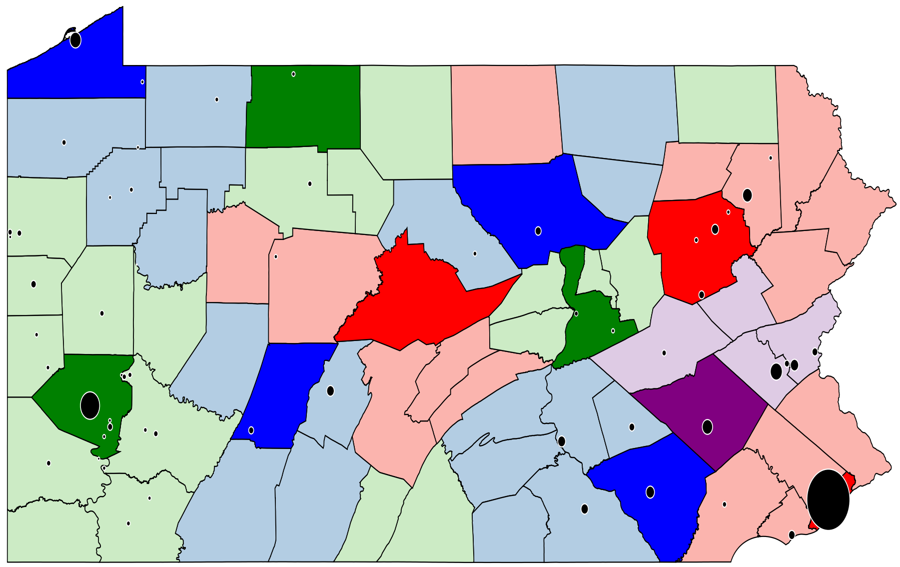
As an illustration, Figure 1 presents the regional market partition of Pennsylvania. We delineate regions by color so that no region shares a color with an adjacent region. That is, any contiguous mapping of counties of the same color corresponds to a *single* regional market. To further indicate

[8]Tolbert and Sizer (1996) delineate regions with γ set to 0.98, which is the value we choose. Our results using this parameter with the 2000 data result in a slightly more agglomerated mapping than the official U.S. Department of Agriculture case. This arises primarily because we use a nationwide flow matrices rather than overlapping regional matrices.

[9]In our estimates, J does not always equal the sample size because we restrict our aggregate analysis to markets with more than 25,000 residents. This, and the additional condition that more than one county be present in a market, further restricts the sample size for the spillover analysis.

[10]This subdivision is not possible for 24 regions consisting of a single county. We drop these from our subregional analysis.

Figure 1: Local labor markets (regions) in Pennsylvania



Notes: Any contiguous mapping of a single color represents a specific regional market from LM_{601}. The large county subregion is represented by the darker tone and the satellite subregion by the lighter tone. Black circles indicate the 57 cities in Pennsylvania. Circle sizes are proportional to city population in 2010.

subregions, we color the large county subregion with a darker color tone. For example, in the bottom-right quadrant of the map we identify Philadelphia County, which contains the eponymous city of Philadelphia, as the largest county in its regional market. As the large county subregion in the five-county regional market, Philadelphia County is colored dark red. The corresponding satellite subregion consists of the remaining four counties is colored light red. This partitioning implies that Pennsylvania has 14 regional markets. To show how these regional markets reflect the distribution of population across the state, we indicate the 57 cities in Pennsylvania with black circles that are proportional to the city population in 2010.

We assess how LM_{601} partitions the nation into regional markets in Table 1. First, we group regions by the number of counties contained within them. These groups are listed in column (1). Next, for every region identified, we construct the following ratio: $ratio_{LM_{601}} = \frac{Pop_j^L}{Pop_j^O}$, which is the population of the large county subregion relative to the population of the satellite subregion.

For each grouping by number of counties, we report the average of ratios in column (2). Column (3) tabulates the number of regions used to construct the ratio averages. For example, there are 106 regions that are comprised of three counties; for this group, the average ratio between the population of the largest county and the population of the two remaining counties is 2.39. Column (4) provides the total population for each regional market grouping. Note that we exclude Alaskan regions where commuting patterns and local market conditions are likely to differ substantially

Table 1: Population distributions of regional markets by number of constituent counties from LM_{601}

Number of Counties (1)	Ratio (2)	Number of LMs (3)	Population (millions) (4)
1	.	24	4
2	2.96	57	4
3	2.39	106	22
4	1.23	93	19
5	1.14	80	23
6	1.05	77	43
7	0.97	43	31
8	0.93	33	19
9	0.78	30	29
10	1.01	15	18
11	0.88	7	12
12	0.79	9	21
13	0.57	8	33
14	0.47	3	18
15	0.75	3	6
16	1.63	1	1
17	0.25	1	5
All LMs	1.49	590	308

Notes: Population distributions from the agglomeration identifying 601 regions (labor markets [LMs] in table). The statistics above exclude Alaska, where commuting patterns and the economics of regional markets are likely to differ substantially from those across the rest of the nation.

from those across the rest of the nation.

For the majority of regions in LM_{601} and for a sizable portion of the population, the largest county population, on average, is larger than the rest of the counties combined. This implies that many regional markets have a distinctively unimodal structure when disaggregated by county.[11]

Outcome variables

We explore two alternative outcome variables: the wage bill and employment. The wage bill data are from the Quarterly Census of Employment and Wages (QCEW), which covers approximately 98% of U.S. jobs. We focus first on the wage bill response to government spending. Total wages received by employees in a given quarter are in the form of "bonuses, stock options, severance pay, profit distributions, cash value of meals and lodging, tips and other gratuities, and, in some States,

[11] The ratio of the large county subregion to the satellite subregion is inversely correlated with the size of the regional market, implying that the unimodal structure does not hold among the most populous markets.

Table 2: Summary statistics for regional market variables used in estimating spatial spillovers, LM_{601}

	Mean	SD	10th Percentile	90th Percentile
ARRA obligations in large county SR p.c.	479.688	727.171	131.183	847.107
ARRA obligations in satellite SR p.c.	361.642	304.780	118.481	630.097
FHWA obligations in large County SR p.c.	67.501	168.495	0.000	150.088
FHWA obligations in satellite SR p.c.	46.527	86.170	0.337	110.265
Regional market income (3-yr moving average)	699.536	672.391	42.803	1506.593
Log of regional market population	12.329	1.265	10.794	14.073
Regional market manufacturing share	0.134	0.071	0.045	0.232
Wage bill level (2008Q4)	3715.929	1112.646	2474.803	5243.021
Δ Wage Bill p.c., lag 1	83.323	170.894	-73.467	250.254
Δ Wage Bill p.c., lag 2	185.230	145.900	43.364	357.448
Regional market job level p.c.(2008Q4)	0.394	0.064	0.310	0.478
Δ Jobs (p.c.), lag 1*	-0.691	1.024	-1.724	0.554
Δ Jobs (p.c.), lag 2*	0.283	0.718	-0.527	1.190
Total obligations, large county SR = $126 billion				
Total obligations, satellite SR = $120 billion				
FHWA obligations, large county SR = $13 billion				
FHWA obligations, satellite SR = $13 billion				
Number of observations = 506				

Notes: The statistics above exclude Alaska and regional markets with fewer than 25,000 residents. SR indicates subregion; ARRA, American Recovery and Reinvestment Act of 2009; FHWA, Federal Highway Administration; p.c., per capita; SD, standard deviation.
* Variables are rescaled by 100 to ease interpretation.

employer contributions to certain deferred compensation plans such as 401(k) plans."[12] These data are observed at the county level and mapped to the regions identified above. Table 2 contains summary statistics for the variables used in our analysis.

More formally, the outcome variable is the accumulated change in the per capita wage bill in the two years following the passage of the Recovery Act, relative to a base-period of 2008:Q4. That is,

$$\Delta\text{Wage-Bill}_j^R = \frac{1}{Pop_j^R}\sum_{k\in K}(P_{j,k}^R - P_{j,2008:Q4}^R) \tag{2.2}$$

where j indicates a particular region, k indicates the quarter, $R \in \{A, L, O\}$ dictates the level of aggregation, $K = \{2009 : Q1, ..., 2010 : Q4\}$, and $P_{j,k}^R$ indicates the total wages received by

[12] See: http://www.bls.gov/cew/cewfaq.htm#Q15 for a description of the QCEW data. These wages do not include other forms of worker compensation, such as medical insurance.

employees.

Treatment variables (V_j^R)

Define $\bar{V}_j^R$ as the cumulative value of Recovery Act dollars through 2010:Q4 obligated to organizations in region j for some subset, $R \in A, L, O$. These amounts are constructed from quarterly reports filed by all recipients of contracts, grants, and loans. The data were downloaded from Recovery.Gov.

In these reports, recipients provide the place of performance zip code, which allows us to map the amount received by prime recipients, subrecipients, vendors, and subvendors to a particular county, net of any portion of the funding reported as spent by a different entity.[13]

For each regional market and for $R \in A, L, O$, we scale the cumulative Recovery Act obligations by the overall regional market population and report the variable in terms of millions of dollars:

$$V_j^R = \frac{\bar{V}_j^R}{(1e+6) \times Pop_j} \tag{2.3}$$

Instrument variables (Z_j^R)

Because policymakers intended to distribute funds, in part, to regions most affected by the recession, our treatment variable may suffer from endogenity. To ameliorate this potential endogeneity, we use the cumulative Recovery Act highway obligations as an instrument for overall spending. Allocated to states by the federal government according to formulary rules, which are based on non-business cycle conditions (e.g., total eligible highway lane-miles and share of total vehicle-miles traveled), highway obligations are unlikely to be correlated with the idiosyncratic economic conditions within the state. Furthermore, highway obligations represented a substantial portion of the overall funding made available to states. As of 2010:Q4, the Federal Highway Administration (FHWA) had reported that obligations had reached a total of \$26 billion.[14]

Dupor and Mehkari (2014), who also use county-level highway Recovery Act funding as an instrument, discuss the potential for endogeneity to arise, not at the state level, but at the within-state allocation of funds. They point out that the bias could arise in two ways: if officials allocate funding to economically depressed regions within the state (downward bias) or to growing labor markets in need of infrastructure to maintain their growth (upward bias). Dupor and Mehkari

[13] For instance, a prime recipient might spend a portion of the award in the county in which it operates while redistributing funding to a subrecipient operating in an adjacent county. Recipients were required to report only payments made to subrecipients, vendors, and subvendors in excess of \$25,000. A casual review of the data shows that many payments less than \$25,000 were reported. Though we cannot observe recipient level data for these unreported awards, we do know the total value of unreported awards by recipient. In total, these small awards represent less than 3% of all funding reported in the recipient reports. Finally, vendor expenditures are mapped to a particular market through the zip code location of its headquarters.

[14] Total obligations as of 2010:Q4 across all categories included in our analysis equaled \$246 billion.

(2014) find little evidence of endogenous allocation of FHWA funding in either direction.[15]

Define Z_j^R as the cumulative FHWA funding to some subset partition $R \in A, L, O$ in region j, constructed in the same fashion as overall Recovery Act obligations, V_j^R.

Conditioning Variables

We estimate our models with three sets of control variables along with a constant. Each of the variables listed below is observed at the regional market level.

- The first set consists of eight Census region dummies.
- The second set includes three wage bill trend controls: (i) the level per capita wage bill in 2008Q4, (ii) the change in the wage bill between 2007Q4 and 2008Q4, and (iii) the change in the wage bill between 2006Q4 and 2007Q4. These three variables, respectively, control for immediate pre-recession wage conditions, the idiosyncratic exposure of the regional market to the rapidly unfolding recession, and the movement of wages during the final stages of the speculative housing buildup.
- The third set of controls pertains to the idiosyncratic economic conditions and structure in the region. We include in this set the share of employment in manufacturing, the natural log of population, and a 3-year moving average of annual personal income per capita (from 2006 to 2008).

3 Estimation and Results

Let $X = [1, \text{Region Dummies}, \text{Wage Trend Controls}, \text{Labor Market Conditions}]$. This is the full set of conditioning controls used in estimating the following simultaneous equations. We report estimated results with varying subsets of X. For convenience, we drop the subscript j in what follows and observe variables at the regional market level, A, unless stated otherwise.

The regional estimation specification is given by the following two equations:

$$\begin{aligned} \Delta\text{Wage-Bill} &= \psi V + X\beta + \epsilon \\ V &= \eta Z + X\Gamma + \nu \end{aligned} \tag{3.1}$$

[15] They find weak and statistically imprecise evidence that high productivity counties (measured by pre-recession per capita income) received more highway funding than less wealthy counties. Higher-productivity markets may have, in turn, been more immune to the effects of the recession. In this case, our estimates should be interpreted as an upper bound on the jobs effect of Recovery Act spending.

The subregional specification is as follows, for $R, K \in L, O$ and where $K \neq R$:

$$\begin{aligned} \Delta\text{Wage-Bill}^R &= \psi_1^X V^R + \psi_2^K V^K + X\beta + \epsilon^R \\ V^R &= \eta_1^R Z^R + \eta_2^K Z^K + X\Gamma + \nu^R \end{aligned} \tag{3.2}$$

The subregional specification in (3.2) parses out both the direct effect and the spillover effect of spending in a particular subregion.

The disturbance process for the large county subregion is likely to be correlated with that of the satellite subregion (i.e., $corr(\epsilon^L, \epsilon^O) \neq 0$). For this reason, we estimate the pair of spillover equations by three-stage least squares (3SLS). This allows us to take into account the cross-equation correlation of the disturbance process between the two subregions. Also, we are then able to test cross-equation restrictions on parameter estimates, which will then allow us to directly compare the regional estimates with the subregional estimates.

A brief note on interpreting the coefficients is in order. In the first instance, ψ corresponds to the marginal increase in the wage bill over eight quarters for every dollar of Recovery Act funding. A coefficient of 1 would indicate that the wage bill increased dollar for dollar. ψ_1^L and ψ_1^O correspond to the wage bill increase in the large county subregion and satellite subregion for every additional dollar obligated in the large county subregion and satellite subregion, respectively—*holding the obligations in the adjacent subregion constant.* These are, in a sense, the direct effects on the subregion economy for every dollar expended. Likewise, ψ_2^L and ψ_2^O quantify the degree of spillover, respectively, in the form of an increased wage bill in the satellite subregion as a result of spending in the large county subregion and vice versa.

3.1 Wage Bill Estimates

Regional specification estimates

The estimates for equation (3.1) are listed in Table 3. Column (1) shows the bivariate two-stage least squares (2SLS) estimates of the effect of Recovery Act spending on the change in the wage bill. These estimates are observed at the regional market level. In this column, we do not control for regional conditions, labor market characteristics, or wage trend patterns. The coefficient on this estimate is 1.13 (SE = 0.37). This should be interpreted as indicating that an additional \$1 in Recovery Act funding is associated with an increase in the wage bill of \$1.13. The negative R^2 results from using actual Recovery Act funding rather than predicted amounts in constructing the residual sum of squares.

Column (2) of Table 3 controls for labor market conditions, such as per capita income, the log of population, and the share of employment in manufacturing. In this column, we estimate that a \$1 injection of Recovery Act spending increased the wage bill by \$0.95 (SE = 0.44).[16] In column

[16]Two observations are dropped because manufacturing employment data were unavailable due to confidentiality

(3) we include wage trend controls and our estimate ticks up to 1.40 (SE = 0.53).

The final column of Table 3 includes the full set of controls—all conditioning variables plus census region dummies. We consider this fully specified model our baseline regional market estimate. The coefficient equals 1.25 (SE = 0.54). This implies that an additional \$1 of spending allocated to a regional market increased the wage bill by \$1.25. For this specification, as in those previously explicated, the partial F-statistic is sufficiently high to conclude that FHWA funding is strongly correlated with overall spending.

It should be noted that the wage bill increase resulting from Recovery Act funding does not fully reflect the full value of dollars paid out in the form of contracts, grants, and loans. There are two reasons this should be the case. First, we cannot rule out the possibility that some dollars went to other regions through cross-regional trade; however, our method for identifying regional markets mitigates this confounding form of "leakage."

Second, some funds likely went to firm profits or to other nonlabor factors of production. Under the assumption of constant returns to labor, our estimate of a \$1.25 wage bill increase can be used to construct an output multiplier. With the share of output flowing to labor set at 0.6, this result implies an output multiplier equal to $2.1 \approx 1.25/0.6$.

The least squares estimates analogous to the estimates in Table 3 are reported in Appendix Table A.1. Those wage bill responses are smaller than the corresponding 2SLS values. This suggests that the endogeniety bias works in the expected direction.

Subregional estimates

As explained in Section 2.1, we also partition each region into a large county subregion, L, and a satellite subregion, O. Next, we estimate (3.2) by 3SLS to separately identify direct and spillover effects. These results are given in Table 4.[17] To save space, we present estimates solely from the model with the full set of conditioning variables. Column (1) provides estimates for ψ_1^L and ψ_2^O, which are the direct spending effects. Respectively, these are the effects of Recovery Act funding within and upon the large county subregion and the effect of funding within and upon the satellite subregion. Column (2) provides coefficient estimates for ψ_1^O and ψ_2^L, which are the subregion spillover effects. These are the conditional outcomes in one subregion as a result of spending in the adjacent subregion.

The direct effect coefficients are, for the large county subregion, 0.79 (SE = 0.23) and, for the satellite subregion, -0.38 (SE = 0.31). Considering first the large county subregion, L, this implies that \$1 of Recovery Act spending *within the large county subregion* increased the wage bill *in that subregion* by \$0.79. The p-value of this coefficient is less then 1%. The point estimate for direct satellite subregion funding implies that the wage bill decreased by \$0.38; however, we cannot reject

concerns. See online QCEW documentation for an explanation of reporting procedures.

[17] The least squares estimates analogous to the estimates in Table 4 are reported in Appendix Table A.5

Table 3: Two-stage least squares estimates of the effect on the wage bill of Recovery Act spending, aggregate results for LM_{601}

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Recovery Act obligations	1.13***	0.95**	1.40***	1.25**
($1 million p.c.)	(0.37)	(0.44)	(0.53)	(0.54)
Income	-	-67.68***	-24.67	-14.32
(3-yr moving average)†		(19.44)	(18.76)	(17.81)
Log of population†	-	-0.02**	0.02**	0.01
		(0.01)	(0.01)	(0.01)
Manufacturing share†	-	0.07	0.01	-0.07
		(0.15)	(0.15)	(0.15)
Wage bill level (2008Q4)	-	-	-0.92***	-0.88***
			(0.13)	(0.13)
Δ Wage bill	-	-	-0.91	-1.17
($1 million p.c.), lag 1			(0.91)	(0.96)
Δ Wage bill	-	-	0.04	-0.28
($1 million p.c.), lag 2			(0.65)	(0.74)
Census Region Dummies	No	No	No	Yes
N	511	509	509	509
R^2	-0.109	0.091	0.214	0.330
First-stage results				
t-Statistic FHWA instrument	11.052	9.792	8.943	10.039
Partial F-statistic	122.148	95.880	79.976	100.775
R^2, first stage	0.071	0.118	0.127	0.144

Notes: The regressions above exclude Alaska, where commuting patterns and the economics of regional markets likely substantially differ from those across the rest of the nation. Equations estimated with Huber-White robust standard errors (SEs).
†Coefficients/SEs are rescaled by 100 to ease interpretation.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Three stage least squares estimates of the effect on the wage bill of Recovery Act spending, subregion spatial spillovers LM_{601}

	Large County Subregion Wage Bill	Satellite Subregion Wage Bill
	(1)	(2)
	Coef./SE	Coef./SE
ARRA obligations in large	0.79***	0.59***
county Subregion ($1 million p.c.)	(0.23)	(0.19)
ARRA obligations in satellite	1.19***	-0.38
subregion ($1 million p.c.)	(0.37)	(0.31)
Full set of controls	Yes	Yes
Implied Total Obligation Effect	1.105	
p-value	0.001	
P(x = 1.25)	0.666	
N	506	
R^2	0.326	0.140
Partial F-Statistic	17.400	17.400

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated using the full set of controls. Since only the dependent variable changes across specifications, the partial F-statistic is identical across specifications. The implied total obligation effect is taken as the appropriately weighted sum of direct and spillover estimates. Large counties received roughly 51% of ARRA funding. Thus, the implied effect $= 0.51(\psi_1^L + \psi_2^L) + 0.49(\psi_1^O + \psi_2^O)$. Partial regression plots of these estimates are provided in the Appendix Figure A.1.
* $p < .1$, ** $p < .05$, *** $p < .01$

the null hypothesis of no effect for direct satellite subregion spending and so this result should be interpreted cautiously.

The coefficient for the effect of spending *in the large county subregion* on the wage bill *in the satellite subregion* is 0.59 (SE = 0.19), which implies that $1 of spending in the large county subregion increased the wage bill by $0.59 in the satellite subregion. The estimated coefficient for spillovers *from the satellite subregion* to the *large county subregion* is 1.19 (SE = 0.37), which similarly implies that $1 of funding in the satellite subregion produced a wage bill increase of $1.19 in the large county subregion. At least from the satellite subregion to the large county subregion, Recovery Act funding appears to have had an amplifying effect: every dollar spent in the former increased the wage bill in the latter by more than a dollar. In both sets of equations, the spillover estimates are statistically significant. It is not within the scope of this paper to discuss the possible role of size and population density in contributing to the differential spillover effects observed; rather, it is sufficient to note that these spillovers are present and that they are quantitatively and economically important. In the appendix, we show that these patterns of direct spending effects and spillover effects are robust to all control specifications.

Next, we consider whether our decomposed direct/spillover subregional estimates are consistent with our regional estimates provided in Table 3. When we estimate the regional market equation, we find that an additional $1 of Recovery Act funding increased the wage bill by $1.25. To compare this result with the subregional estimates, we compute a weighted sum of the coefficients according to the proportion of overall Recovery Act funding that each subregion type received. By 2010:Q4, large counties had received $126 billion and outlying counties had received $120 billion. Using these values, we weight our large county coefficients by 0.51 and our outlying counties by 0.49. We designate the resultant value, 1.10, as the implied total obligation effect. This statistic is in the first row in the second section of Table 4. Furthermore, we show that this value is statistically different from zero at the 1% level and is indistinguishable from our regional market result of 1.25. Thus, the regional results are internally consistent with the subregional estimates.

3.2 Employment response estimates

Regional estimates

Increasing employment was, perhaps, the primary objective of the Recovery. Next, we re-estimate each of the above specifications, but we replace the wage bill with employment as our outcome variable.

Our specific outcome variable is the accumulated number of job-years, relative to 2008:Q4 over the first two years following the Act's passage. The formula is:

$$\Delta\text{Job-years}_j = \frac{1}{4 \times Pop_j} \sum_{k \in K} (Y_{j,k} - Y_{j,2008:Q4}), \tag{3.3}$$

where $K = 2009:Q1, ..., 2010:Q4$. We then divide this variable by 1 million, which allows us to interpret the coefficient of interest as the number of jobs created for an entire year for every million dollars of Recovery Act money obligated.

The sets of conditioning variables used in the job-years regressions are similar, though not identical to those previously used. Instead of the wage bill trend variables, we use (i) the total number of jobs in region j in 2008:Q4 per capita, (ii) the change in the number of jobs per capita between 2007:Q4 and 2008:Q4, and (iii) the change in the number of jobs per capita between 2006:Q4 and 2007:Q4.

Table 5 shows the regional estimates of the job-years effect. As with our results for the wage bill regressions, we provide estimates with varying subsets of conditioning variables ranging from the naïve bivariate analysis to the specification with the full set of controls included. The first notable finding in Table 5 is the relative stability of the estimates. Our estimates range from 15.54 to 18.65, all of which are statistically significant. In column four we observe a point estimate of 15.69 (SE = 6.46), which implies that employment increased by roughly 16 jobs for every million dollars obligated by the federal government.

The implied cost per job estimate is $63,700. To put this amount in context, we note that the typical U.S. full-time worker earns roughly $37,100 per year.[18] Taken together, we observe that the cost to the government to add a single job for a year was approximately 72% higher than the typical cost of adding a single job in the overall economy.

The least squares estimates analogous to the estimates in Table 5 are reported in Appendix Table A.2. Those employment responses are smaller than the corresponding 2SLS values. This suggests that the endogeniety bias works in the expected direction.

Subregional estimates

Table 6 shows the subregional estimates when we change the outcome variable to job-years and include the full set of controls. We report results for all varying sets of controls in the appendix.[19]

The patterns of direct and spillover effects in Table 6 mirror those reported for the wage bill regressions. The direct job-years effect of $1 million in Recovery Act obligations is given by the point estimate 9.66 (SE = 3.49) for the large county subregion and -0.38 (SE = 4.71) for the satellite subregion. As before, we caution that the direct effect of spending in the satellite subregion is indistinguishable from the null hypothesis of no effect. We also see that for every $1 million obligated in the large county subregion employment increased by 6.62 jobs (SE = 3.04) in the satellite subregion. Similarly, for every $1 million obligated in the satellite subregion, the large

[18]We compute this number based on the following evidence. According to the 2009 Occupational Employment Statistics, the median hourly wage was $15.95 in 2009. According to the Current Employment Statistics, the average hours worked per week in 2009 was 33.9. The Employer Cost for Employee Compensation reported that wages accounted for 70% of total compensation. Assuming a 48-week work year, this implies a typical annual employment compensation of roughly $37,100.

[19]The least squares estimates analogous to the estimates in Table 6 are reported in Appendix Table A.6

Table 5: Two-stage least squares estimates of the effect on employment of Recovery Act spending, aggregate results for LM_{601}

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Recovery Act obligations	18.65***	15.54**	17.96***	15.69**
($1 million p.c.)	(5.62)	(6.48)	(6.31)	(6.46)
Income	-	-774.17***	-609.09**	-339.13
(3-yr moving average)†		(287.91)	(278.36)	(265.27)
Log of population†	-	-0.30***	-0.06	-0.12
		(0.10)	(0.11)	(0.12)
Manufacturing share†	-	-2.78	-0.38	-1.14
		(2.24)	(2.18)	(2.16)
Job level (2008Q4)	-	-	-0.12***	-0.14***
			(0.03)	(0.03)
Δ Jobs	-	-	0.36*	0.27
(1 million p.c.), lag 1			(0.21)	(0.23)
Δ Jobs	-	-	0.24	0.34*
(1 million p.c.), lag 2			(0.19)	(0.20)
Census Region Dummies	No	No	No	Yes
N	511	509	509	509
R^2	-0.127	0.039	0.067	0.190
First-stage results				
t-Statistic FHWA instrument	11.052	9.792	9.006	9.944
Partial F-statistic	122.148	95.880	81.113	98.874
R^2, first stage	0.071	0.118	0.123	0.144

Notes: The regressions above exclude Alaska, where commuting patterns and the economics of regional markets likely substantially differ from those across the rest of the nation. Equations estimated with Huber-White robust standard errors (SEs).

†Coefficients/SEs are rescaled by 100 to ease interpretation.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Three-stage least squares estimates of the effect on employment of Recovery Act spending, subregion spatial spillovers LM_{601}

	Large County Subregion Job-Years	Satellite Subregion Job-Years
	(1)	(2)
	Coef./SE	Coef./SE
ARRA obligations in large	9.66***	6.62**
county Subregion ($1 million p.c.)	(3.49)	(3.03)
ARRA obligations in satellite	13.01**	-0.38
subregion ($1 million p.c.)	(5.44)	(4.71)
Full set of controls	Yes	Yes
Implied total obligation effect	14.510	
p-Value	0.007	
P(X = 15.69)	0.826	
N	506	
R^2	0.173	0.127
Partial F-statistic	16.762	16.762

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated with the full set of controls. Since only the dependent variable changes across equations, the partial F-statistic is identical in each equation. The implied total obligation effect is taken as the appropriately weighted sum of direct and spillover estimates. Large counties received roughly 60% of ARRA funding. Thus, the implied effect $= 0.51(\psi_1^L + \psi_2^L) + 0.49(\psi_1^O + \psi_2^O)$.
* $p < .1$, ** $p < .05$, *** $p < .01$

county subregion experienced an increase in employment of 13.01 jobs (SE 5.44). Using the same spending weights as before, we observe an implied total obligation effect of 14.46 a result that is significant at the 1% level and statistically indistinguishable from our regional estimate of 15.69. Thus, our overall impact estimates from the regional and subregional analyses are consistent with each other.

Intensive and Extensive Margins

There are two key margins along which firms can increase output in response to a demand shock. First, along the extensive margin, the firm can create new positions to meet demand. This is costly both in terms of the time it takes to find and hire new employees as well as in terms of the resources reallocated to train new employees. Second, along the intensive margin, the firm can increase wages for those *already* employed in the form of increased hours and overtime pay. With regard to the Recovery Act, Dupor and Mehkari (2014) provide evidence that the intensive margin effect was nearly the same size as the extensive margin effect of a newly created job as measured by total dollar cost. They estimate that 46% of the increase in the wage bill flowed to those already employed. Because the intensive margin effect of the Recovery Act was large in their study, we ask whether the intensive margin remains as important in this analysis. This latter point serves as yet another diagnostic concerning the robustness of our regional approach.

With the employment and wage bill estimates in hand (Sections 3.1 and 3.2), we now ask what portion of the wage bill increase can be attributed to the increase in employment as a result of the Recovery Act. To make the two statistics comparable, we need an estimate of what the typical annual compensation for a single job in the U.S. We follow Dupor and Mehkari (2014) and specify that the typical full-time worker is compensated \$37,100 per year in 2009. Using our baseline regional estimates of a wage bill effect of \$1.25 million and job-years effect of 15.96 jobs for every \$1 million obligated, we estimate that 53% of the wage bill increase comes from increased employment. This result is very similar to that of Dupor and Mehkari (2014).[20] In both analyses, the intensive and extensive margin effects are roughly the same size.

3.3 Comparison across choices of LM_G

How does our choice of LM_{601} relate to estimates derived from other possible levels of agglomeration? To answer this question, we estimate the benchmark specification for several alternative values of γ, which govern the degree of agglomeration. Specifically, we are interested in how the coefficient on Recovery Act obligations changes along varying degrees of agglomeration. The first column of Table 7 indicates the number of regional markets identified for a particular threshold value of γ. These range from the highest level of agglomeration LM_{300} to the lowest level $LM_{3,143}$—that is, a county-level cross section.

The next five columns of Table 7 delineate for a particular LM_G, in order, the point estimate of the effect of Recovery Act funding on the change in the wage bill, the standard error of the estimate, and the partial F-statistic from the first stage. In all of our specifications, the partial F-statistic indicates that FHWA funding continues to be a strong instrument for overall spending.

One motivation for estimating the wage bill effect at the regional market level was to estimate differential outcomes across quasi-closed regions as a result of varying levels of Recovery Act funding. This approach, explained above, incorporates potential cross-county spillover effects into the analysis. A cross-sectional analysis that ignores such spillovers may yield biased estimates. Suppose for a moment that LM_{601} is the most appropriate partitioning of the country into regional markets, we can construct a measure of the implied bias of other under-agglomerated and over-agglomerated sets. We calculate this bias as $\frac{LM_J - LM_{601}}{LM_{601}}$ = Implied Bias of Estimate. This value for the county-level analysis is -0.83. This implies that the county-level cross-section understates the actual effect of the Recovery Act by more than 80%. The implied bias is thus quite substantial.

The county-level analysis yields a point estimate of 0.21 (SE = 0.19). If taken at face value, this implies that Recovery Act funding was ineffectual in increasing the wage bill. Given that our regional

[20] $\frac{(1.25-0.371\times15.69)}{1.25} = 53\%$

Table 7: Two-stage least squares estimates of the wage bill response along different degrees of agglomeration

Number of LMs (1)	Wage Bill Estimate (2)	SE (3)	F-Statistic (4)	Implied Bias of Estimate (5)
150	2.03	0.77	8.25	0.62
200	1.80	0.60	7.97	0.44
250	2.06	0.54	13.50	0.64
300	1.89	0.41	49.02	0.51
372	1.75	0.34	133.20	0.39
601*	1.25	0.54	100.78	0.00
709†	1.08	0.56	64.40	-0.14
750	0.90	0.52	139.20	-0.29
904	0.78	0.49	168.17	-0.38
1054	0.77	0.45	159.61	-0.38
1293	0.92	0.46	153.50	-0.27
2500	0.16	0.17	98.66	-0.87
3000	0.06	0.16	119.12	-0.95
All counties	0.21	0.19	96.00	-0.83

Notes: Estimates use full set of controls, exclude Alaska, and are estimated with Huber-White robust errors.
* Indicates baseline agglomeration, LM_{601}.
†LM_{709} corresponds to the official USDA commuting zone delineation for 2000.

market estimate is both statistically significant and of an economically important magnitude, the difference is stark. A comparable calculation of the implied aggregate output multiplier with the county-level point estimate of 0.21 yields an implied multiplier of just 0.35: six times smaller than the value derived in our regional market estimation.

Next, a concern may arise that we have identified a spurious bias from an imprudently chosen identification of regions, that is, the agglomeration effect we identify is no more than a random outcome among many possible LM_J sets used to analyze the wage bill effect. The evidence, however, points in the opposite direction. Reading Table 7 from bottom (least agglomerated) to top (most agglomerated), the estimated coefficient increases in value. This near-monotonic increase in the coefficient as a function of the degree of agglomeration indicates that we are, in fact, estimating a true effect and in turn an actual bias in the county estimation.

Figure 2a shows how our estimates vary with agglomeration. The x-axis shows varying regional market specifications, corresponding to different degrees of county agglomeration. The y-axis is the estimated ψ for each LM_J, along with 90% confidence intervals. Although not all LM_J specifications are statistically distinguishable from zero, there is a clear positive correlation between the degree of agglomeration and the wage bill response.

Figure 3a shows the corresponding results of the job-years regressions. Again, the coefficient increases with the degree of agglomeration. The point estimate from the county-level analysis is 11.91 (SE = 3.52), which implies that for every \$1 million expended employment increased by 11.91 jobs, implying a cost per job-year equal to \$84,000. However, as in the wage bill analysis, there appears to be a confounding cross-county trade bias in the county-level analysis. We remind readers of our estimate of the jobs-year effect for the baseline regional market identification, LM_{601}—a point estimate of 15.69, which implies a cost per job of \$63,700. The largest job-years effect we observe is for the LM_{250} model, in which case the implied cost to the government to add a single job for a year was \$42,600. All of these estimates include the full set of conditioning variables. Tabulations corresponding to Figure 3a are provided in the appendix.

Given sample size restrictions, we do not provide estimates for agglomerations with fewer than 150 regional markets. Nevertheless, we observe that our wage bill estimates, as seen in Figure 2a, eventually stabilize around 2 for agglomerations beyond LM_{372}. This implies that, at a certain point, the spillovers we measure are fully contained within the identified regional markets and that there is little to no commuting-related spillovers between them. That is, no new information is gained by agglomerating regions further. For each of these most agglomerated sets we can reject at the 90 percent confidence level a wage bill effect of 0.76; if the share of income flowing to labor is less than 76 percent, then these results provide statistically significant results of an output multiplier greater than unity.[21]

3.4 Random Assignment: A Diagnostic

Regional exercise

As an initial check on whether commuting linkages are driving our results, we perform the following diagnostic. First, we group counties into quantiles according to their employed resident labor force. Then, within each quantile, we randomly reassign the geographic location of each county to that of a different county from the same quantile. Apart from the change in location, each county keeps all of its own variables used in the analysis (e.g. employment, wage bill, Recovery Act spending). Then, for each level of agglomeration, we calculate the regional variables. Relative to the "true" non-shuffled geographies, our procedure maintains the total number of regional markets, the number of counties comprising each region and, roughly, the labor force distribution within each region. However, because we have randomized locations, we have broken the commuting linkages in the original data.

Next, we estimate the regional 2SLS model and plot the estimate for the shuffled data at various levels of agglomeration. If the increase in the wage response estimate with the degree of agglomeration (as seen in Figure 2a) is due to commuting spillovers, then that relationship should

[21] Given the imprecision with which we estimate agglomeration effects for the job-years regressions, we cannot make the same observation; yet, we do observe that the job-years effect appears also to have an upper bound, even if it is imprecisely estimated.

disappear in the shuffled data. As seen in Figure 2b, this is precisely what happens. Using the shuffled data, the wage bill response estimate is flat with respect to the degree of agglomeration.

Figures 3a and 3b present the analogous estimates for the employment responses. The conclusion for this variable is the same.

Subregional exercise

To determine whether our results could be driven by a type of spurious mechanical relationship between large county subregions and satellite subregions, we proceed as follows. First, we randomly match each large county subregion with a *different* satellite subregion. We then re-estimate our baseline, subregional regression with the wage bill as the outcome variable. The full set of controls is included in these regressions and each control is observed relative to the regional market from which the dependent variable is derived. For example, the large county subregional equation in the 3SLS model includes the set of controls observed for the regional market j in which it is located; however, the satellite subregional equation will include the set of controls observed for the regional market $i \neq j$ in which it is located.

Table 8 delineates the results from these random assignment regressions. The spillover estimates (bottom-left and upper-right quadrants) are statistically indistinguishable from the null hypothesis of no effect. There is no reason to expect that large county subregion spending should affect the wage bill in a randomly selected satellite subregion, and vice versa. Thus, our finding of no effect lends further support for the estimates reported in Tables 4 and 6.

Importantly, we detect a direct large county effect in the wage bill regression that is of a similar magnitude to the effect observed in the baseline spillover estimates. The point estimate from Table 8 for the direct large county wage bill response is \$0.71 (SE = 0.23) for every \$1 obligated. Rather than reporting the implied total obligation effect, we report the test of equality between our direct effect response and our baseline large county direct effect estimate. We cannot reject that the direct large county effect is equal to our previous estimate. We perform a similar exercise with the employment response and find similar results. This table is reported in the appendix.

3.5 Trade in Goods Rather Than Commuting?

Whereas we have emphasized inter-county commuting as a source of spillovers thus far, inter-county trade might also cause spillovers. Ideally, we could consider this latter possibility by clustering counties according to the intensity of trade between them. Unfortunately, this dataset does not exist.

Next, we construct a simple method to estimate the role for inter-county trade. We begin by computing an intensity of non-traded economic activity in each region. First, we treat the service sector as non-traded and all other sectors as traded. Next, we use the Mian and Sufi (2012) classifications to place each of the 4-digit NAICS industries into services (non-traded goods) and

Table 8: Three-stage least squares estimates of the effect on the wage bill of Recovery Act spending, randomly assigned subregion spatial spillovers LM_{601}

	Large County Subregion Wage Bill	Satellite Subregion Wage Bill
	(1)	(2)
	Coef./SE	Coef./SE
ARRA obligations in large	0.71***	0.09
county Subregion ($1 million p.c.)	(0.23)	(0.79)
ARRA obligations in random	-0.24	-0.18
satellite subregion ($1 million p.c.)	(0.36)	(1.35)
Full set of controls	Yes	No
P(X = Benchmark)	0.723	
N	506	
R^2	0.300	-6.555
Partial F-statistic	10.347	11.094

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated by 3SLS with the full set of controls. We test whether the direct effect for the large county subregion is statistically different from the estimate derived in our benchmark spillover specification.
* $p < .1$, ** $p < .05$, *** $p < .01$

non-services (traded goods). With this partition, we measure the intensity of non-traded production as the percentage of each regions total employment in the non-traded sector.

Intuitively, a region with mostly non-traded goods is more “closed than a region with many traded goods. For the former, economic activity should be more self-contained and, as such, spillovers to other regions should be relatively smaller. Its regional multiplier effect is likely to be self-sustaining.

Figure 4 is a histogram of the regions non-traded goods. It shows substantial heterogeneity. Next, we estimate the regional model one two sub-samples of the data. We then estimate the wage bill model for the 50th percentile and lower in terms of non-traded intensity and the 75th percentile and higher in terms of the same measure. Table 9 shows that, for tradable-intense regions, there is no statistically significant effect of spending on the wage bill. What might be going on? If government spending in a region leads to substantial leakagepotentially because many goods purchased by that regions earners are importedthen the estimated direct multiplier may be small.

On the other hand, for non-tradable intense regions, a one dollar increase in spending increases the regions wage bill by $2.25 (SE = 0.37). As explained above, a within region multiplier effect may be stronger when dollars remain in a region with a large non-traded sector.

Table 9: Two-stage least squares estimates of the wage bill response for tradable and non-tradable intense regional markets

	Tradable Coef./SE	Non-Tradable Coef./SE
Recovery Act	0.29	2.25***
Obligations ($ million p.c.)	(0.37)	(0.37)
Controls†	Yes	Yes
N	254	126
R^2	0.418	0.292
Partial F-statistic	16.193	74.006

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated by 2SLS.
†We include only labor market condition and wage trend variables in these estimations because of the loss of degrees of freedoms when partitioning the sample by trade intensity.
* $p < .1$, ** $p < .05$, *** $p < .01$

4 An Economy with Trade, Commuting and Spillovers

4.1 The model

We present a model of a national economy that contains two geographic areas, A and B, in addition to the rest of the nation (RON).[22] Areas A and B are identical with respect to population size, but each is small relative to RON.

Residents of area A

There exists a representative family domiciled in area A, with variables subscripted with 1. Similarly, there exists a representative family domiciled in area B, with variables subscripted with 2. The preferences of the type 1 family are given by

$$\sum_{t=0}^{\infty} \beta^t E_0 \left[\log \left(Q\left(C_{1,t}^H, C_{1,t}^F\right) - \frac{\theta}{2}\left(L_{1,t}^A\right)^2 - \frac{\theta_c}{2}\left(L_{1,t}^B\right)^2 \right) \right]$$

where Q is a Cobb-Douglas aggregator. Next, $C_{1,t}^H$ and $C_{1,t}^F$ are the consumption of the home-produced traded good and the RON-produced traded good, respectively. Each type 1 family has two productive, but distasteful, uses for its non-leisure time: (i) supplying $L_{1,t}^A$ hours in area A, and (ii) supplying $L_{1,t}^B$ in area B. A time period should be interpreted as one year.

The family uses labor income to pay taxes to the government of area A, buy consumption from within area A and RON, as well as save in a national bond market. The law of motion for its debt,

[22]Our set-up builds upon the models in Galí and Montacelli (2005) and Santacreu (2005), although we use flexible prices.

$D_{1,t}$, is

$$D_{1,t} = (1 + r_{1,t}) D_{1,t-1} + P^A_{H,t} C^H_{1,t} + P_t C^F_{1,t} - W^A_t L^A_{1,t} - W^B_t L^B_{1,t} + P^A_{H,t} Z_{1,t}$$

Since area A is small with respect to RON and RON is not modeled, we treat P_t as exogenous. Throughout the paper, let the RON-produced tradables produced act as the numeraire (i.e., $P_t = 1$).

We assume that the family faces a standard debt limit in order to rule out Ponzi schemes. The family is also required to pay a lump-sum tax, $Z_{1,t}$, of the area A good to the area A government.

The net interest rate paid by the type 1 family is $r_{1,t}$. Following Schmitt-Grohé and Uribe (2014), we assume that

$$r_{1,t} = r^* + \omega \left[\exp\left(\tilde{D}_{1,t} - \tilde{D} \right) - 1 \right]$$

where $\omega > 0$ and $\tilde{D}_{1,t}$ is the per capita average debt level of type 1 families. This can be interpreted as the national risk-free rate plus a premium required of borrowers from an area with a high level of indebtedness. Moreover, let $r^* = (1 - \beta)/\beta$.

The Euler equations of the type 1 family are

$$\theta L^A_{1t} = W^A_t$$

$$\beta (1 + r_{1,t}) = \frac{C_{1,t+1} - \theta \left(L^A_{1,t+1} \right)^2 /2 - \theta_c \left(L^B_{1,t+1} \right)^2 /2}{C_{1,t} - \theta \left(L^A_{1,t} \right)^2 /2 - \theta_c \left(L^B_{1,t} \right)^2 /2}$$

$$\frac{\theta}{\theta_c} \left(\frac{L^A_{1,t}}{L^B_{1,t}} \right) = \frac{W^A_t}{W^B_t}$$

$$\frac{Q_1 \left(C^H_{1,t}, C^F_{1,t} \right)}{Q_2 \left(C^H_{1,t}, C^F_{1,t} \right)} = P^A_{H,t}$$

where

$$C_{1,t} \equiv Q \left(C^H_{1,t}, C^F_{1,t} \right)$$

as well as a transversality condition.

Government of area A

This government borrows B^A_t at net interest rate r^* and collects taxes from area A residents. The government also faces an exogenous, iid process for its expenditure G^A_t, which has expectation $\bar{G}$. The law of motion for government debt is

$$B^A_t = (1 + r^*) \left[B^A_{t-1} + P^A_t \left(G^A_t - Z_{1,t} \right) \right]$$

where $r^* = (1-\beta)/\beta$. The government debt rule is

$$B_t^A = \rho B_{t-1}^A + \psi P_t^A \left(G_t^A - \bar{G}\right)$$

This debt rule, in turn, defines the tax process. We consider the case where ψ and ρ are close to but less than 1. In this case, a government spending shock is financed primarily by deficit spending in the short run and the debt generated by the spending, which takes many years to erode. For any choice of (ρ, ψ), the spending shock will be debt neutral in the long run.

The area A resource constraint is

$$X^A + C_{1,t}^H + G_t^A = L_{1t}^A + L_{2t}^A$$

Here, L_{2t}^A are the hours worked in area A by commuters from type 2 families, who are domiciled in area B. X_t^A is an exogenous demand for exports.

Home-produced tradable goods firm from area A

This firm hires labor from both area A and area B workers at wage rate W_t^A. The firm transforms labor into goods in a one-for-one fashion. The first-order condition for optimization implies $W_t^A / P_t^A = 1$.

Firms, families and government of area B

There are analogous conditions for the firms and the government operating in area B, as well as the type 2 families. The equilibrium conditions generated from this area of the nation are

$$\theta L_{2,t}^B = W_t^B$$

$$\beta\left(1+r_{2,t}\right) = \frac{C_{2,t+1} - \theta_c \left(L_{2,t+1}^A\right)^2/2 - \theta\left(L_{2,t+1}^B\right)^2/2}{C_{2,t} - \theta_c \left(L_{2,t}^A\right)^2/2 - \theta\left(L_{2,t}^B\right)^2/2}$$

$$r_{2,t} = r^* + \omega\left[\exp\left(\tilde{D}_{2,t} - \tilde{D}\right) - 1\right]$$

$$W_t^B / P_{H,t}^B = 1$$

$$X^B + C_{2,t}^H + G_t^B = L_{1t}^B + L_{2t}^B$$

$$\frac{Q_1\left(C_{2,t}^H, C_{2,t}^F\right)}{Q_2\left(C_{2,t}^H, C_{2,t}^F\right)} = P_{H,t}^B$$

where

$$C_{2,t} \equiv Q\left(C_{2,t}^H, C_{2,t}^F\right)$$

Equilibrium

A regional equilibrium is a set of stochastic processes for prices, $\left\{P^A_{H,t}, P^B_{H,t}, W^A_t, W^B_t, r_{1,t}, r_{2,t}\right\}$, and quantities, $\left\{C^H_{i,t}, C^F_{i,t}, L^A_{i,t}, L^B_{i,t}, D_{i,t}, B^A_t, B^B_t, Z_{i,t}, G_{i,t}\right\}_{i=1,2}$, such that

(i) type 1 and type 2 families solve their optimization problem taking as given the price and tax processes.

(ii) area A and area B firms solve their problems taking as given the price processes.

(iii) for both area A and area B, the laws of motion for government debt and the government debt policy rule are satisfied, and

(iv) the labor and goods markets clear.

4.2 Solution and Analysis

The model has only a few parameters that need to be chosen. Given its simplicity, we choose parameters to demonstrate the qualitative potential for fiscal policy spillovers. We leave building a richer model capable of quantitatively matching our econometric estimates, presented in Section 3, to future research.

First, we choose $\beta = 0.96$ to match a steady-state interest rate of 4%. Next, we choose $\left(\theta, \theta_c, \bar{G}\right) = (2, 3, 0.2)$. This allows us to hit steady-state targets of government spending to output in each region equal to 0.25 and to have one-third of workers in a region be commuters from the other region. Next, we choose ρ and ψ equal to 0.95 for reasons explained above. We choose $\tilde{D} = -1$.[23] We let $X^A = X^B = 0.03$. Finally, we let Q be a Cobb-Douglas function with the elasticity with respect to the home-produced good equal to 0.75.

Figure 5(a) shows the impulse responses to a 5% increase in government spending (solid line) in area A and the corresponding path for taxes in that area (dashed line). While the temporary spending increase occurs only at $t = 1$, the tax increase used to finance the spending is spread over many years.

Figure 5(b) shows the response of final consumption in both areas. Consumption in each area increases. The increase is driven primarily by our use of Greenwood, Hercowitz and Huffman (1988), preferences (GHH). For these preferences, an increase in consumption decreases the marginal disutility of work.

As demand for the home-produced good in area A increases, resulting from the spending shock, labor input from both commuters from region B as well as domestic workers increases. This raises

[23] In experimenting with parameter choices, we observed that the parameter $\tilde{D}$ largely influences whether the model exhibits a unique, local steady-state equilibrium. We choose this parameter to ensure uniqueness and generate a moderate value for the spillover elasticity.

the marginal utility of consumption in both regions, leading to an increase in consumption in both regions.

Figure 6 plots the responses of hours for the four types of workers. This is the first indicator of a spillover effect. Hours worked in area B increases even though the government spending occurred only in area A. The area A government demand draws in labor both from domestic residents and commuters from area B. The increase in hours from these commuters drives up the marginal utility of consumption of area B residents. This in turn increases the private demand for goods, and therefore labor, in area B.

Hours worked increases more for those employed in area A than in area B because the former has two sources of increased demand: higher area A government demand plus higher demand for consumption because of the GHH marginal utility effect. Hours worked in area B increases only as a result of the second effect. Examining either panel, note that (in percentage deviation terms) hours worked by those in an area does not depend on which area in which the worker resides. This is because both types of workers employed in a particular region are paid the same wage; furthermore, the use of GHH preferences means hours is a function of only that wage.

Figure 7(a) plots the wage bill responses in each area. The wage bill increases in both areas, which is further evidence of fiscal policy spillovers. Note that the wage bill increases more in area A than area B. This is because the goods price in area A increases by more than in area B, as a result of higher demand from government in area A driving up the goods price more in that area.

Finally, we are interested in the wage bill multipliers. We can calculate these using the simulations conducted above. All of these are on-impact multipliers, which are computed using the traded good price as the numeraire. The impact wage bill multipliers with respect to a spending shock are: (a) 2.32 in area A, where the spending occurs (i.e., the "own" wage bill multiplier); and (b) 0.689 in the neighboring area B, (i.e., the "spillover" wage bill multiplier).[24]

5 Conclusion

This paper explores the importance of crossregional spillovers in assessing the impact of countercyclical government spending on labor market outcomes. We study a single spillover type: the movement of income from points of employment to places of residence.

In our modeling context, a spillover exists whenever an effect of the treatment variable in one area influences the outcome variable in another area. There are other potential sources of spillovers besides the one studied here. First, workers consume goods delivered from far outside their areas of residence. These linkages are not captured solely by examining commuting data. Second, the location where government spending occurs may not be the same location at which the taxes to cover the spending will be paid—an issue that is particularly relevant for the U.S. Point of fact,

[24]The own wage bill multiplier is given by $\Delta\left(W_t^A L_{1,t}^A + W_t^A L_{2,t}^A\right) / \left[\Delta\left(P_t^A G_t^A\right)\right]$; the spillover wage bill multiplier is given by $\Delta\left(W_t^B L_{1,t}^B + W_t^B L_{2,t}^B\right) / \left[\Delta\left(P_t^A G_t^A\right)\right]$.

a 2007 Tax Foundation analysis shows that there is a great deal of heterogeneity across states in the federal spending received per dollar of federal tax dollars paid. As a stylized example, suppose \$1 million is spent in Mississippi in one year but the offsetting taxes will eventually be paid by residents in New Jersey. The spending in Mississippi may have a negative spillover on New Jersey if agents in the latter state reduce capital accumulation in anticipation of future distortionary taxes. Third, another type of spillover is possible because U.S. states share a common monetary policy.[25]

It is worth noting that our cost-per-job estimate is substantially lower than that found in three previous studies. Whereas our baseline estimate is \$63,700, other studies find costs that are 2 to 3 times this amount.[26] Each of the three is conducted at the state level. Although none of the studies account for crossregional interaction, it is unlikely that a state-level study would suffer from the type of bias described in Section 3. Presumably, a state-level analysis would not run the risk of using data constructed at an overly granular level.

We present here a brief suggestion for how these disparate findings might be reconciled. As shown, cross-county patterns of trade matter for correctly identifying the effects of government spending. Many regional markets are contained entirely within a particular state; yet, the normal course of economic activity is not confined to state borders. As such, there will still be cross-state trade. With these ideas in mind, we can consider each state as representing an imprecisely observed regional market. As such, the outcome economic effect (wage bill, job-years, and so on) is measured with error as is the treatment effect (in this case, Recovery Act funding). Whereas the first error produces no bias, the latter introduces an attenuation bias toward zero. Whether and to what extent this potential attenuation bias can reconcile our results with the existing literature remains to be seen. A more thorough explanation of the differences between our results in this current paper and those from other studies merits further attention.

Next, our subregional results also present a puzzle. We found that spending in a satellite subregion has no direct effect, but yet has a strong positive spillover effect on the wage bill and employment in its partner subregion. Explaining this finding is a valuable topic for future research.

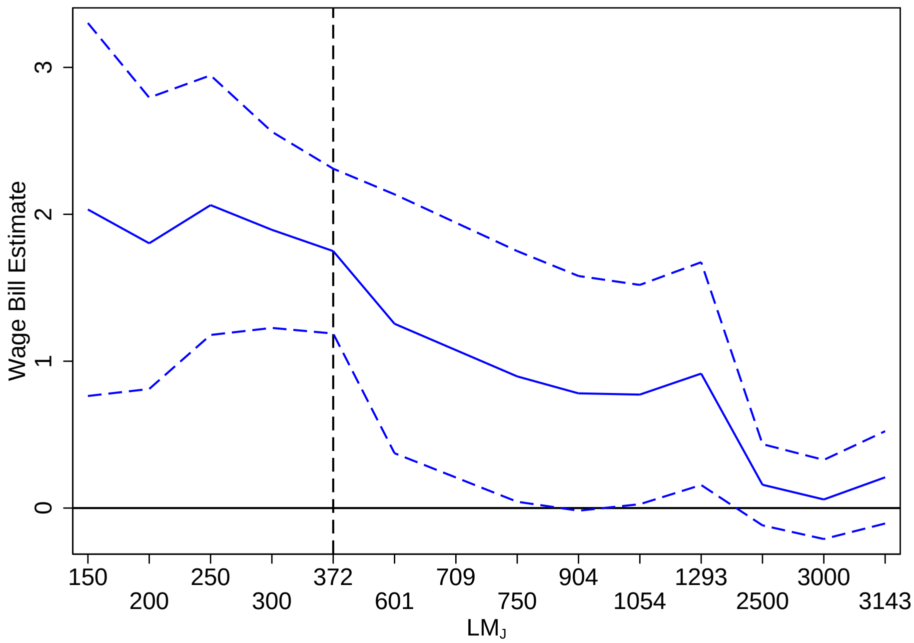
Finally, our comparison of estimates across different levels of agglomeration leads us to a cautionary conclusion. Our results suggest and provide an example for which cross-sectional analyses that ignore crossregional interaction have the potential to lead researchers to biased conclusions.

[25]See Nakamura and Steinsson (2013).

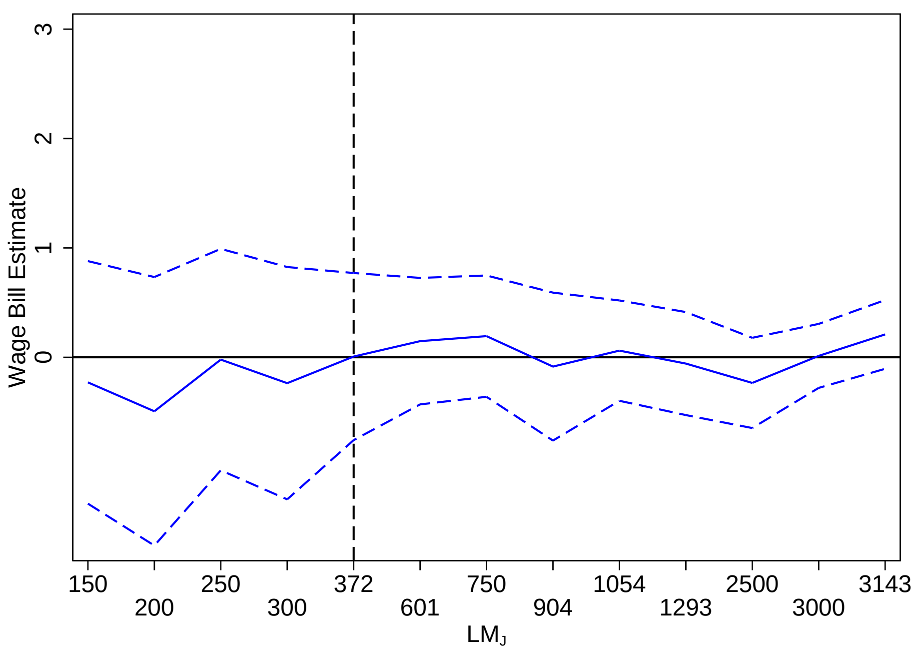
[26]See Conley and Dupor (2013), Dupor (2013) and Wilson (2012).

Figure 2: Estimates of the wage bill effect of government spending for different degrees of agglomeration: actual and randomly assigned regional model specification

(a) Wage bill agglomeration effects by LM_J



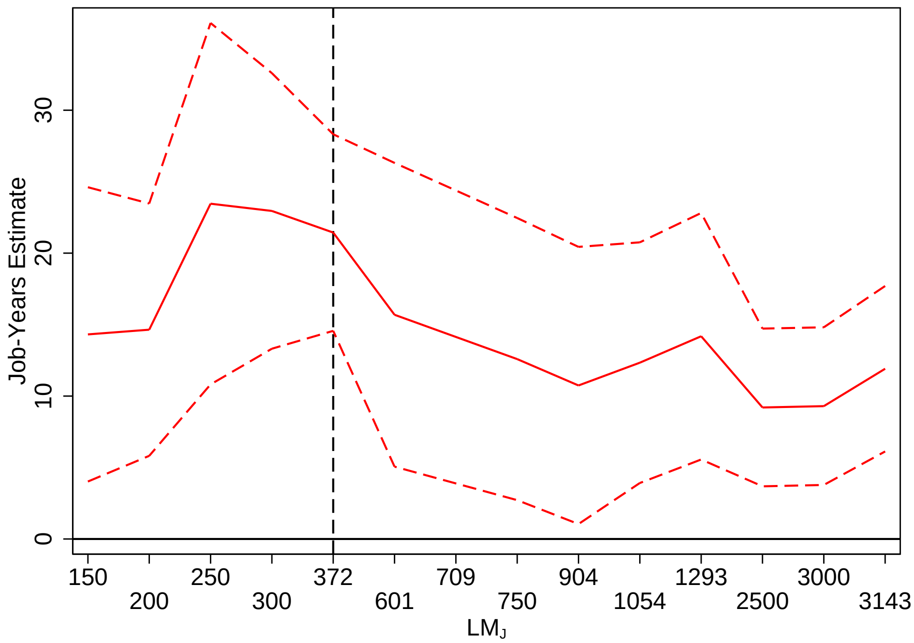
(b) Random assignment wage bill agglomeration effects by LM_J



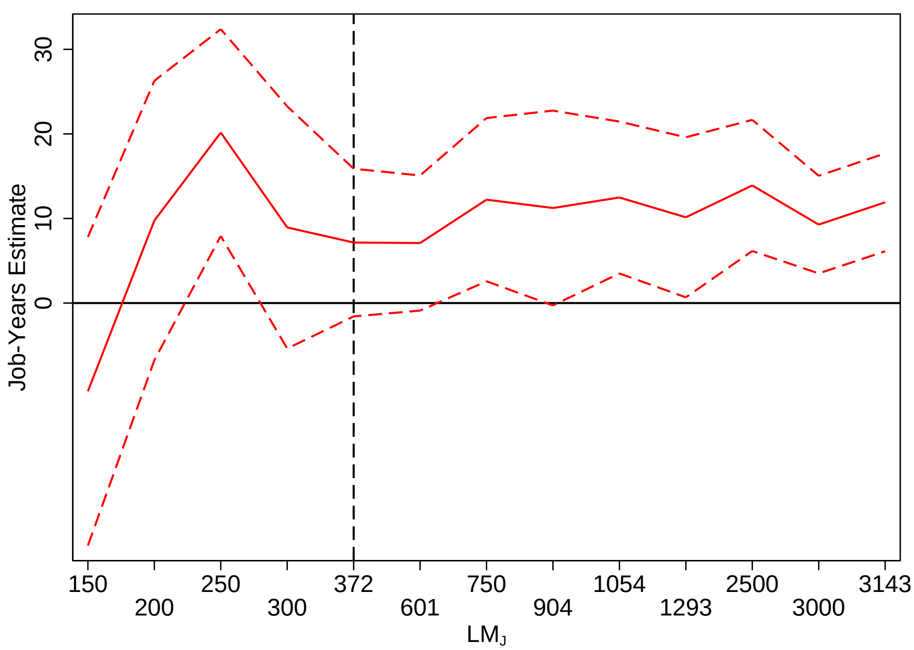
Notes: Regional markets with fewer than 25,000 residents are excluded from the analysis. Estimated with robust SEs. The dotted line indicates LM_{601}.

Figure 3: Estimates of employment effect of government spending for different degrees of agglomeration: actual and randomly assigned regional model specification

(a) Job-years agglomeration effects by LM_J



(b) Random assignment job-years agglomeration effects by LM_J



Notes: Regional markets with fewer than 25,000 residents are excluded from the analysis. Estimated with robust SEs. The dotted line indicates LM_{601}.

Figure 4: Histogram of proportion of regions labor in the tradable sector

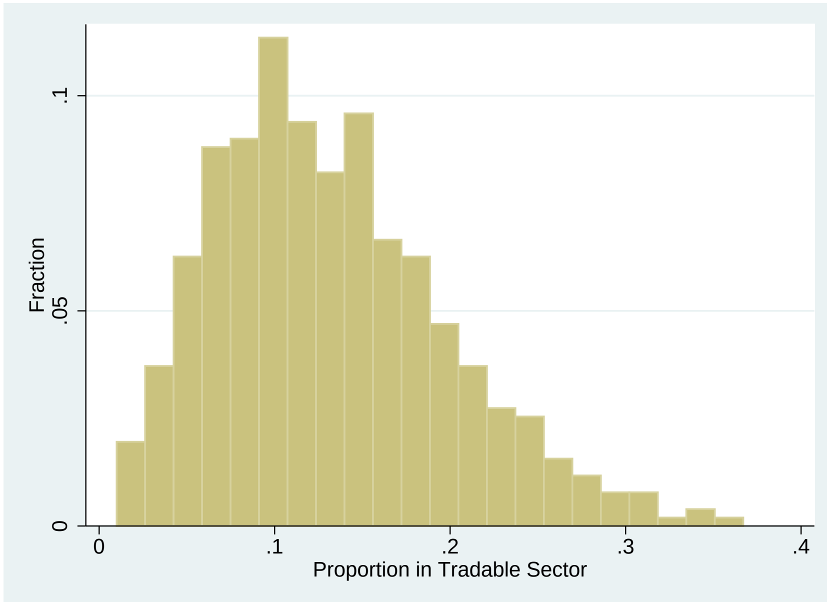


Figure 5: Model impulse responses of taxes, area A final consumption, area B final consumption to an area A government spending shock, in percentage deviation terms

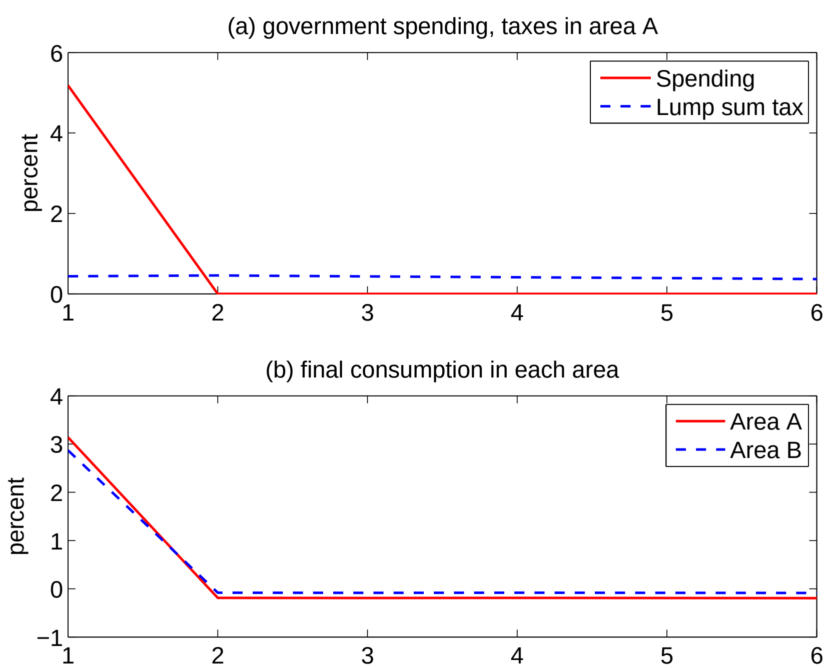


Figure 6: Model impulse responses of hours worked to an area A government spending shock, in percentage deviation terms

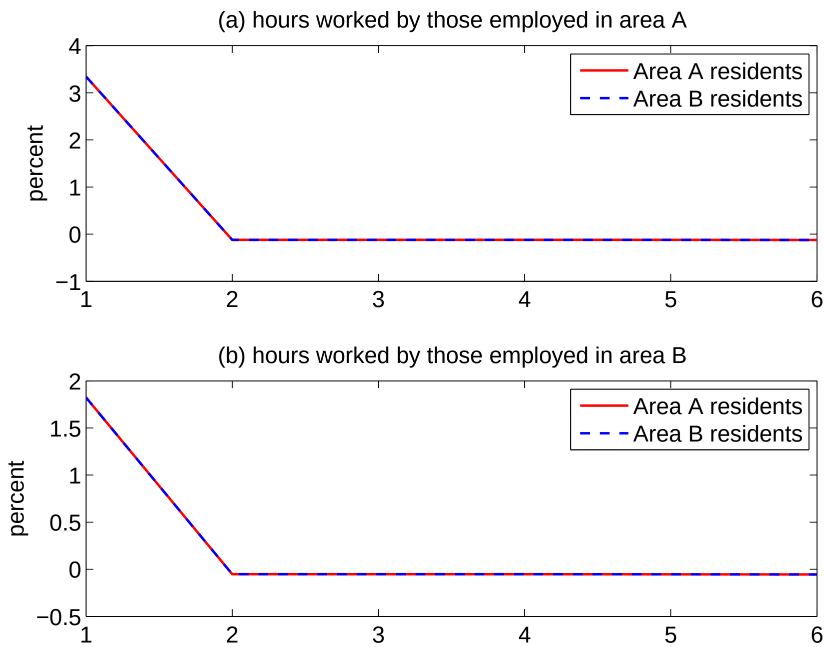
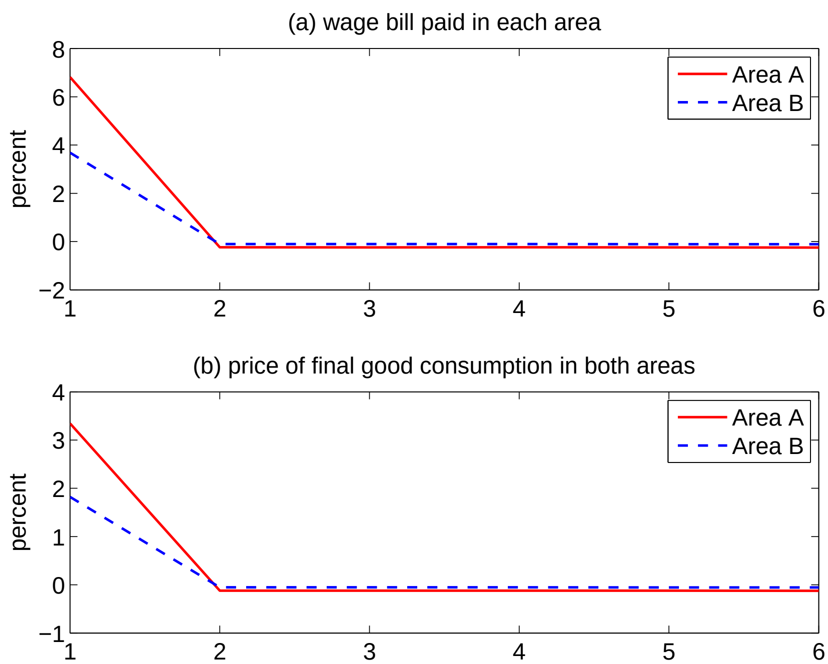


Figure 7: Model impulse responses of the area wage bills and final goods prices to an area A government spending shock, in percentage deviation terms



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A Appendix

Table A.1: OLS estimates of the effect on the wage bill of Recovery Act spending, aggregate results for LM_{601}

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Recovery Act obligations	0.36**	0.29*	0.41**	0.43**
($1 million p.c.)	(0.14)	(0.15)	(0.20)	(0.19)
Income	-	-79.86***	-47.41***	-31.09**
(3-yr moving average)†		(18.89)	(13.30)	(13.67)
Log of population†	-	-0.02**	0.01	0.01
		(0.01)	(0.01)	(0.01)
Manufacturing share†	-	-0.10	-0.23**	-0.27***
		(0.10)	(0.10)	(0.10)
Wage bill level (2008Q4)	-	-	-0.82***	-0.83***
			(0.12)	(0.13)
Δ Wage bill	-	-	-0.97	-1.09
($1 million p.c.), lag 1			(1.00)	(1.02)
Δ Wage bill	-	-	0.13	-0.03
($1 million p.c.), lag 2			(0.71)	(0.80)
Census Region Dummies	No	No	No	Yes
N	511	509	509	509
R^2	0.031	0.192	0.433	0.479

Notes: The regressions above exclude Alaska, where commuting patterns and the economics of regional markets likely substantially differ from those across the rest of the nation. Equations estimated with Huber-White robust standard errors (SEs).
†Coefficients/SEs are rescaled by 100 to ease interpretation.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.2: OLS estimates of the effect on employment of Recovery Act spending, aggregate results for LM_{601}

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Recovery Act obligations	6.36**	4.75*	5.25*	5.18*
($1 million p.c.)	(2.95)	(2.78)	(3.02)	(2.81)
Income	-	-971.03***	-862.71***	-526.34*
(3-yr moving average)†		(290.51)	(282.86)	(274.16)
Log of population†	-	-0.31***	-0.09	-0.15
		(0.10)	(0.12)	(0.13)
Manufacturing share†	-	-5.50***	-3.39*	-3.58**
		(1.60)	(1.78)	(1.79)
Job level (2008Q4)	-	-	-0.10***	-0.12***
			(0.03)	(0.03)
Δ Jobs	-	-	0.39*	0.34
(1 million p.c.), lag 1			(0.22)	(0.23)
Δ Jobs	-	-	0.24	0.36*
(1 million p.c.), lag 2			(0.21)	(0.22)
Census Region Dummies	No	No	No	Yes
N	511	509	509	509
R^2	0.046	0.165	0.241	0.306

Notes: The regressions above exclude Alaska, where commuting patterns and the economics of regional markets likely substantially differ from those across the rest of the nation. Equations estimated with Huber-White robust standard errors (SEs).
†Coefficients/SEs are rescaled by 100 to ease interpretation.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.3: 3SLS estimates of the effect on the wage bill of Recovery Act spending, subregion spatial spillovers LM_{601} for all control specifications

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Large County Subregion				
ARRA obligations in large	0.57**	0.59**	0.92***	0.79***
county Subregion ($1 million p.c.)	(0.26)	(0.26)	(0.25)	(0.23)
ARRA obligations in satellite	1.20***	1.04**	1.21***	1.19***
subregion ($1 million p.c.)	(0.44)	(0.41)	(0.39)	(0.37)
Satellite Subregion				
ARRA obligations in large	0.65***	0.49**	0.66***	0.59***
county Subregion ($1 million p.c.)	(0.20)	(0.20)	(0.20)	(0.19)
ARRA obligations in satellite	-0.33	-0.45	-0.36	-0.38
subregion ($1 million p.c.)	(0.34)	(0.31)	(0.31)	(0.31)
Labor Market Conditions	-	Yes	Yes	Yes
Wage Trend Controls	-	-	Yes	Yes
Census Region Dummies	-	-	-	Yes
Implied Total Obligation Effect	1.046	0.839	1.225	1.105
p-value	0.011	0.026	0.001	0.001
P(x = Agg. Est.)	0.844	0.760	0.633	0.666
N	506	506	506	506
R^2 (Large County)	-0.013	0.117	0.198	0.326
R^2 (Satellite)	-0.123	0.047	0.066	0.140
First Stage Results				
Partial F-Statistic	19.661	18.723	17.294	17.400

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated by 3SLS with varying sets of controls. Since only the dependent variable changes across equations, the partial F-statistic is identical in each equation. The implied total obligation effect is taken as the appropriately weighted sum of direct and spillover estimates. Large counties received roughly 60% of ARRA funding. Thus, the implied effect $= 0.51(\psi_1^L + \psi_2^L) + 0.49(\psi_1^O + \psi_2^O)$.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.4: 3SLS estimates of the effect on employment of Recovery Act spending, subregion spatial spillovers LM_{601} for all control specifications

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Large County Subregion				
ARRA obligations in large	10.23***	9.28**	11.56***	9.66***
county Subregion ($1 million p.c.)	(3.61)	(3.62)	(3.78)	(3.49)
ARRA obligations in satellite	14.33**	12.56**	13.49**	13.01**
subregion ($1 million p.c.)	(6.07)	(5.74)	(5.74)	(5.44)
Satellite Subregion				
ARRA obligations in large	9.32***	7.05**	7.77**	6.62**
county Subregion ($1 million p.c.)	(3.04)	(2.97)	(3.11)	(3.03)
ARRA obligations in satellite	0.65	-0.31	0.06	-0.38
subregion ($1 million p.c.)	(5.11)	(4.70)	(4.72)	(4.71)
Labor Market Conditions	-	Yes	Yes	Yes
Job Trend Controls	-	-	Yes	Yes
Census Region Dummies	-	-	-	Yes
Implied Total Obligation Effect	17.327	14.346	16.519	14.510
p-value	0.004	0.010	0.004	0.007
P(x = Agg. Est.)	0.825	0.829	0.800	0.826
N	506	506	506	506
R^2 (Large County)	-0.084	0.010	0.021	0.173
R^2 (Satellite)	-0.081	0.064	0.071	0.127
First Stage Results				
Partial F-Statistic	19.661	18.723	16.916	16.762

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated by 3SLS with varying sets of controls. Since only the dependent variable changes across equations, the partial F-statistic is identical in each equation. The implied total obligation effect is taken as the appropriately weighted sum of direct and spillover estimates. Large counties received roughly 60% of ARRA funding. Thus, the implied effect $= 0.51(\psi_1^L + \psi_2^L) + 0.49(\psi_1^O + \psi_2^O)$.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.5: SUR estimates of the effect on the wage bill of Recovery Act spending, subregion spatial spillovers LM_{601} for all control specifications

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Large County Subregion				
ARRA obligations in large	0.15**	0.14**	0.20***	0.21***
county Subregion ($1 million p.c.)	(0.07)	(0.07)	(0.06)	(0.06)
ARRA obligations in satellite	0.87***	0.75***	0.93***	0.94***
subregion ($1 million p.c.)	(0.17)	(0.16)	(0.14)	(0.14)
Satellite Subregion				
ARRA obligations in large	0.18***	0.13***	0.17***	0.18***
county Subregion ($1 million p.c.)	(0.05)	(0.05)	(0.05)	(0.05)
ARRA obligations in satellite	-0.31**	-0.35***	-0.25**	-0.25**
subregion ($1 million p.c.)	(0.13)	(0.12)	(0.11)	(0.12)
Labor Market Conditions	-	Yes	Yes	Yes
Wage Trend Controls	-	-	Yes	Yes
Census Region Dummies	-	-	-	Yes
Implied Total Obligation Effect	0.441	0.332	0.524	0.538
p-value	0.000	0.004	0.000	0.000
P(x = Agg. Est.)	0.000	0.000	0.000	0.000
N	506	506	506	506
R^2 (Large County)	0.057	0.195	0.383	0.446
R^2 (Satellite)	0.034	0.131	0.230	0.256

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations are estimated by SUR with varying sets of controls. The implied total obligation effect is taken as the appropriately weighted sum of direct and spillover estimates. Large counties received roughly 60% of ARRA funding. Thus, the implied effect $= 0.51(\psi_1^L + \psi_2^L) + 0.49(\psi_1^O + \psi_2^O)$.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.6: Seemingly unrelated regression estimates of the effect on employment of Recovery Act spending, subregion spatial spillovers LM_{601} for all control specifications

	No Controls	Add Labor Market Controls	Add Wage Trend Controls	All Controls (Baseline)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Large County Subregion				
ARRA obligations in large	2.29**	1.83**	2.20**	2.13**
county Subregion ($1 million p.c.)	(0.94)	(0.92)	(0.88)	(0.84)
ARRA obligations in satellite	10.80***	9.53***	10.12***	10.07***
subregion ($1 million p.c.)	(2.24)	(2.18)	(2.09)	(2.03)
Satellite Subregion				
ARRA obligations in large	3.20***	2.29***	2.38***	2.38***
county Subregion ($1 million p.c.)	(0.80)	(0.77)	(0.76)	(0.76)
ARRA obligations in satellite	0.84	-0.56	-0.39	-0.68
subregion ($1 million p.c.)	(1.91)	(1.84)	(1.81)	(1.84)
Labor Market Conditions	-	Yes	Yes	Yes
Job Trend Controls	-	-	Yes	Yes
Census Region Dummies	-	-	-	Yes
Implied Total Obligation Effect	8.480	6.485	7.084	6.884
p-value	0.000	0.000	0.000	0.000
P(x = Agg. Est.)	0.000	0.000	0.000	0.000
N	506	506	506	506
R^2 (Large County)	0.053	0.125	0.201	0.286
R^2 (Satellite)	0.031	0.129	0.154	0.177

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations estimated by SUR with varying sets of controls. The implied total obligation effect is taken as the appropriately weighted sum of direct and spillover estimates. Large counties received roughly 60% of ARRA funding. Thus, the implied effect $= 0.51(\psi_1^L + \psi_2^L) + 0.49(\psi_1^O + \psi_2^O)$.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.7: Two-stage least squares estimates of the employment response along different degrees of agglomeration

Number of LMs	Job-Years Estimate	SE	F-Statistic	Implied Bias of Estimate
(1)	(2)	(3)	(4)	(5)
150	14.32	6.26	7.22	-0.09
200	14.64	5.37	7.51	-0.07
250	23.46	7.69	11.87	0.50
300	22.95	5.86	45.78	0.46
372	21.44	4.18	131.49	0.37
601*	15.69	6.46	98.87	0.00
709†	14.45	6.50	68.41	-0.08
750	12.59	6.00	139.25	-0.20
904	10.74	5.89	169.24	-0.32
1054	12.34	5.12	167.37	-0.21
1293	14.19	5.24	148.14	-0.10
2500	9.20	3.35	94.17	-0.41
3000	9.30	3.35	109.45	-0.41
All counties	11.91	3.52	89.04	-0.24

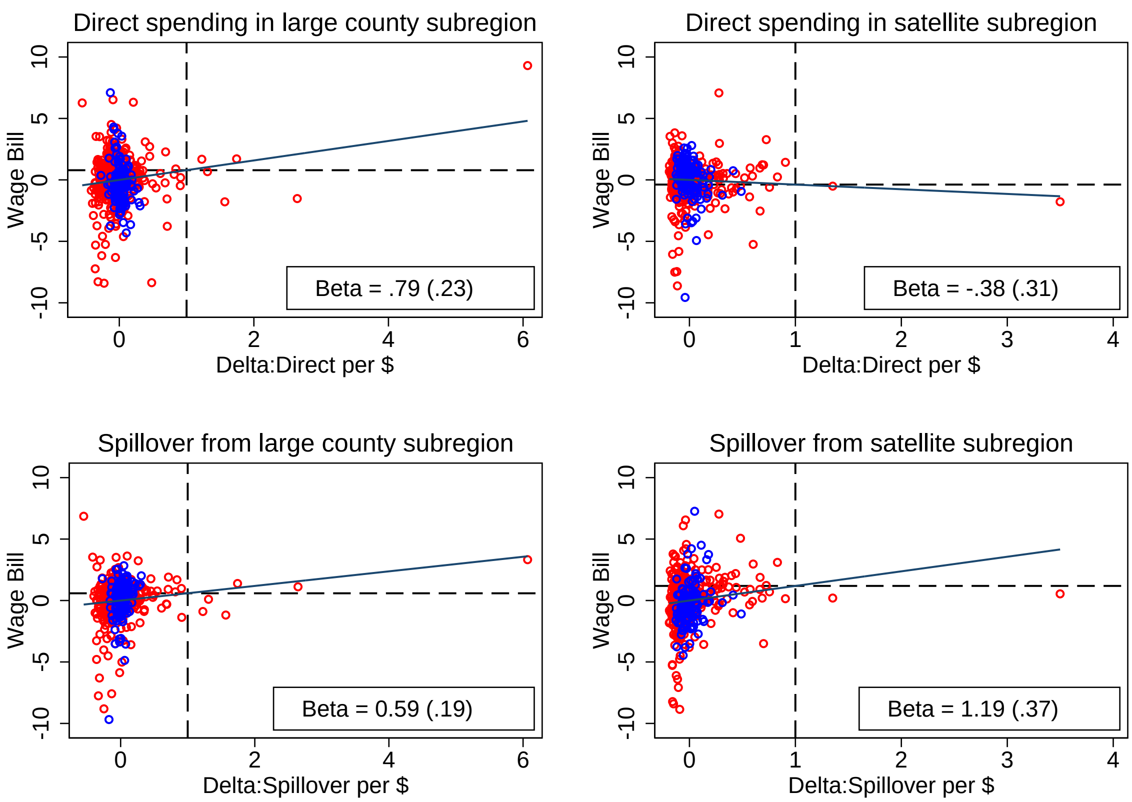
Notes: Estimates use full set of controls, exclude Alaska, and are estimated with Huber-White robust errors.
* Indicates baseline agglomeration, LM_{601}.
†LM_{709} corresponds to the official USDA commuting zone delineation for 2000.

Table A.8: Three-stage least squares estimates of the effect on employment of Recovery Act spending: randomly assigned subregion spatial spillovers LM_{601}

	Large County Subregion Job-Years	Satellite Subregion Job-Years
	(1)	(2)
	Coef./SE	Coef./SE
ARRA obligations in large	8.58**	3.36
county Subregion ($1 million p.c.)	(3.37)	(12.93)
ARRA obligations in random	-2.24	0.23
satellite subregion ($1 million p.c.)	(5.23)	(22.03)
Full set of controls	Yes	No
P(X = Benchmark)	0.748	
N	506	
R^2	0.161	-9.077
Partial F-statistic	10.347	11.094

Notes: The estimates above exclude Alaska and regional markets with fewer than 25,0000 residents. Equations estimated with the full set of controls. We test whether the direct effect for the large county subregion is statistically different from the estimate derived in our benchmark spillover specification.

Figure A.1: Partial regression plots of wage bill response for direct & spillover estimates from three-stage least squares results



Notes: The estimates above exclude Alaska and regional markets with fewer than 25,000 residents. Equations are estimated by 3SLS with varying sets of controls. Regional markets with fewer than 500,000 people are indicated in red; larger regional markets are indicated in blue.