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Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?[†]

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A geographic cross-sectional fiscal spending multiplier measures the effect of an increase in spending in one region of a monetary union. Empirical studies of such multipliers have proliferated. I review this research and what the evidence implies for national multipliers. Based on an updated analysis of the ARRA and a survey of empirical studies, my preferred point estimate for a cross-sectional multiplier is 1.8. The paper also discusses conditions under which the cross-sectional multiplier provides a rough lower bound for the national, no-monetary-policy-response multiplier. Putting these elements together, the cross-sectional evidence suggests a national no-monetary-policy-response multiplier of 1.7 or above. (*JEL* E32, E52, E62, H54, H76, R53)

A geographic cross-sectional fiscal spending multiplier measures the effect of an increase in spending in one region in a monetary union. The past several years have witnessed a wave of new research on such multipliers. By definition, estimation uses variation in fiscal policy across distinct geographic areas in the same calendar period. This approach has a number of advantages, most notably the potential for much greater variation in policy across space than over time and variation more plausibly exogenous with respect to the no-intervention paths of outcome variables. At the same time, cross-sectional multipliers differ in important dimensions from the national government spending multiplier to which they are often compared. Recognition of these differences has led to pessimism regarding whether cross-sectional multipliers provide any guidance for the effects of other types of policies.[1]

In this paper, I assess what we have learned from this research wave. I find the retreat regarding the literature's informativeness for other interventions to be premature. Drawing on theoretical explorations, I argue that the typical empirical cross-sectional

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[1] As part of her review article of fiscal multipliers, Ramey (2011a) concludes: "More research is needed to understand how these local multipliers translate to aggregate multipliers." In a more recently published paper, Fishback and Kachanovskaya (2015, 126) states: "The state multipliers cannot be easily translated into a national multiplier because of spillover effects outside each state's boundaries and because the same state multiplier can lead to a broad range of estimates of the national multiplier under a reasonable set of assumptions in a macroeconomic model." Many studies include similar caveats.

multiplier study provides a rough lower bound for a particular, policy-relevant type of national multiplier, the closed economy, no-monetary-policy-response, deficit-financed multiplier. The lower bound reflects the high openness of local regions, while the "rough" accounts for the small effects of outside financing common in cross-sectional studies. I then review empirical estimates and find a cross-study mean of about 1.8. Putting these two elements together, cross-sectional studies imply a lower bound on the appropriate national multiplier of roughly 1.7.

The paper starts in Section I by reviewing the econometrics of cross-sectional multipliers. I discuss a typical approach and compare with the time series literature to highlight the benefits of relying on cross-sectional variation.

Section II develops the lower bound argument, following closely theoretical results in Shoag (2016); Nakamura and Steinsson (2014); and Farhi and Werning (2016). Much of the pessimism regarding the informativeness of cross-sectional studies arises because in the vast majority of cases the spending does not affect the present value of local tax burdens (for example, the spending is paid for by the federal government). I therefore first consider how the effects of outside-financed spending compare with local deficit-financed spending. Standard economic theory postulates a small quantitative difference between the two when the spending is transitory. Intuitively, Ricardian agents increase their private spending by the annuity value of a transfer, which for transitory spending implies only a small increase relative to the direct change in government purchases. Spending by rule-of-thumb, myopic, or liquidity-constrained agents does not depend at all on the present value of the tax burden; instead, for non-Ricardian agents the comparison of outside-financed spending with local deficit-financed spending (rather than with local tax-financed spending) is crucial, since otherwise there is an offsetting decline in output caused by the contemporaneous higher taxes.

Next, a cross-sectional, deficit-financed government spending multiplier differs from a national multiplier because the cross-sectional multiplier "differences out" other national policy responses, such as a monetary policy reaction, and because of the greater openness of local regions. The quantitative importance of the monetary policy reaction for national multipliers is well known (Woodford 2011; Christiano, Eichenbaum, and Rebelo 2011). Comparing the local multiplier to a national multiplier when monetary policy does not react eliminates this difference between the two multipliers. A binding zero lower bound provides a leading case where monetary policy does not react, with the important caveat that the comparison requires that nominal interest rates not react at any horizon and not just that the short rate be at zero. Greater expenditure switching and income leakage reduce local multipliers relative to the relevant aggregate multiplier, while greater factor mobility can raise them. Since fixed reallocation costs limit factor mobility in response to transitory spending changes, the balance of these elements suggests the national no-monetary-policy-response multiplier exceeds the locally financed local multiplier. Combining these arguments, in empirically relevant cases, the cross-sectional multiplier provides a rough lower bound for the closed economy, no-monetary-policy-response, deficit-financed aggregate multiplier.

Section III deals with an important technical issue. Largely for reasons of data availability, many empirical studies report employment multipliers rather than output multipliers. Comparing across studies and to theoretical models requires a conversion between these two concepts. I show using a simple framework that for the United States a rough translation from an employment to output multiplier is to divide output per worker by the cost per job.

Sections IV and V review empirical cross-sectional multipliers. In Section IV, I conduct original analysis drawing on three earlier studies of the effects of the 2009 American Recovery and Reinvestment Act (ARRA). The section illustrates many of the econometric concepts and provides a template for future studies. Applying a common econometric framework to instruments from each of the three studies, I consistently find a cost per job of the ARRA of roughly \$50,000. Using newly available gross state product data, I estimate an output multiplier of 1.5.

Section V reviews the recent empirical literature more broadly. The first part of the section groups together a set of papers that have examined various components of the ARRA. These studies all exploit variation homogeneous along the dimensions of the outside nature of the financing and the short persistence of the intervention and also all focus on employment rather than output effects of spending. The cost per job across these studies ranges from roughly \$25K to \$125K, with around \$50K emerging as a preferred number. Using the relationship between employment and output multipliers developed in Section III, this magnitude translates loosely into an output multiplier of about two. The central tendency of these magnitudes closely matches the results from the example in Section IV. I then turn to papers using other sources of variation, many quite creative. The diversity of outcome variables and policy experiments makes reaching a synthesized conclusion across these studies harder; nonetheless, those that estimate a cost per job find numbers around \$30K, and, with one or two notable exceptions, those that estimate income or output multipliers find numbers in the range of 1–2.5.

Section VI summarizes what we have learned. After adjusting for spending persistence, the mean cross-sectional output multiplier is 1.8. Applying the rough lower bound result, a cross-sectional multiplier of 1.8 implies a no-monetary-policy-response deficit-financed national multiplier of about 1.7 or above. This magnitude falls at the very upper end of the range found in a recent review article based mostly on time series evidence (Ramey 2011a). Thus, cross-sectional multiplier studies suggest the national multiplier can be larger than often assumed. In addition, many studies find higher multipliers in periods and regions with greater economic slack, pointing to the presence of forces such as lower factor prices or congested labor markets in generating state-dependent multipliers.

Finally, Section VII offers suggestions to help increase the impact of future cross-sectional multiplier studies, including how to further bridge the gap to the national multiplier relevant in actual circumstances.

I. Econometrics of Cross-Sectional Multipliers

Consider the relationship

$$D_{t,t+h} Y_s = \alpha_{h,t} + \beta_h^{xs'} F_{s,t} + \gamma_h' X_{s,t} + \epsilon_{s,t+h}, \tag{1}$$

where Y_s is an outcome such as output or employment in geographic area s, $D_{t,t+h}$ is a difference operator defined as $D_{t,t+h} Y_s = Y_{s,t+h} - Y_{s,t}$, $\alpha_{h,t}$ is a time fixed effect, $F_{s,t}$ is a vector of components of fiscal policy such as government spending and taxes, and $X_{s,t}$ is a vector of covariates.[2] The coefficient vector $\beta_h^{xs'}$ measures the horizon h response of Y to F. The time fixed effect $\alpha_{h,t}$ in equation (1) characterizes β_h^{xs} as a cross-sectional multiplier (xs for cross-section) because identification of β_h^{xs} comes only from variation in fiscal policy across space within the same calendar period. For the regression estimate $\hat{\beta}_h^{xs}$ to consistently estimate the true β_h^{xs}, there must be variation within a calendar period in $F_{s,t}$ uncorrelated (conditional on $X_{s,t}$) with the trajectory of economic activity across areas. This requirement mirrors the "parallel trends" assumption of difference-in-difference estimation.

A. *Typical Approach*

The typical cross-sectional econometric study starts by identifying some vector of variables $Z_{s,t}$, which satisfy the conditions for an excluded instrument: $Z_{s,t}$ is correlated with fiscal policy and the researcher can make an a priori plausible case for the exclusion restriction $E[Z_{s,t}\epsilon_{s,t+h} | X_{s,t}] = 0 \; \forall h$, or, in words, that the variables $Z_{s,t}$ are conditionally independent of local economic trends.[3] Estimation proceeds by using $Z_{s,t}$ as an instrument.

In some instances, $Z_{s,t}$ does not have a monetary representation. For example, $Z_{s,t}$ might consist of a metric of the restrictiveness of state-level balanced budget requirements. In other cases, $Z_{s,t}$ consists of some component of government spending and researchers estimate reduced form responses to this component. For example, suppose federal government spending per capita in state s, $G_{s,t}$, consists of a part constant across states, $\bar{G}_t$, a part that responds endogenously to a state's economy, $\tilde{G}_{s,t}$, and a part $\hat{G}_{s,t}$, which is as good as randomly assigned, where without loss of generality the cross-sectional means of $\tilde{G}_{s,t}$ and $\hat{G}_{s,t}$ are equal to zero. Clearly, the common component $\bar{G}_t$ provides no variation across states and, by assumption, $E[\tilde{G}_{s,t}\epsilon_{s,t+h}] \neq 0$. Therefore, a researcher might set $F_{s,t} = G_{s,t}$ and $Z_{s,t} = \hat{G}_{s,t}$. In the first-stage regression of a 2SLS estimate (abstracting from included instruments other than the time fixed effect, i.e., setting $X_{s,t}$ to empty),

$$G_{s,t} = \Pi \hat{G}_{s,t} + \xi_t + u_{s,t}, \tag{2}$$

the coefficient Π has a probability limit of 1 because by assumption of as good as random assignment $E[\hat{G}_{s,t}\tilde{G}_{s,t}] = 0$. With a first-stage coefficient of one, the

[2] The notation $F_{s,t}$ is meant to be quite general. For example, the vector could include expectations of future spending and taxes. Some studies drop the t subscript and implement equation (1) as a pure cross-sectional regression, while others drop the difference operator on the dependent variable but add an area fixed effect. Because the econometric issues involved with panel fixed effects estimation are similar, I focus on equation (1) for clarity.

[3] Formally, if $F_{s,t}$ is a $K \times 1$ vector of components of fiscal policy, $Z_{s,t}$ an $M \times 1$ vector, and $X_{s,t}$ an $L \times 1$ vector: (i) $M \geq K$ (order condition), (ii) $\text{rank}\left\{E\left[\left(Z'_{s,t} \quad X'_{s,t}\right)'\left(F'_{s,t} \quad X'_{s,t}\right)\right]\right\} = K + L$ (rank condition), and (iii) $E[Z_{s,t} \epsilon_{s,t+h} = 0] \; \forall t, h$ (exclusion restriction). The last condition is stronger than strictly necessary.

second stage estimate of β_h^{xs} is asymptotically equivalent to the reduced-form coefficient obtained from simply replacing $F_{s,t}$ with $Z_{s,t}$ in equation (1). Alternatively, if $Z_{s,t}$ is not independent of the rest of spending $F_{s,t} - Z_{s,t}$, then the two approaches will yield different multipliers.[4]

Finally, rather than reporting the impulse response function traced by $\beta_h^{xs\prime}$, many studies collapse equation (1) into a single regression cumulating the effects across horizons:

$$\left[\sum_{h=0}^{H} D_{t,t+h} Y_s\right] = \alpha_t + \beta^{xs\prime} F_{s,t} + \gamma' X_{s,t} + \left[\sum_{h=0}^{H} \epsilon_{s,t+h}\right], \tag{3}$$

where $\alpha_t = \sum_{h=0}^{H} \alpha_{h,t}$, $\beta^{xs\prime} = \sum_{h=0}^{H} \beta_{h,t}^{xs\prime}$, and $\gamma' = \sum_{h=0}^{H} \gamma'_{h,t}$. Intuitively, the individual coefficient $\beta_h^{xs\prime}$ gives the impulse response of variable Y at horizon h; summing over these impulse responses gives the cumulative additional increase in Y. In many instances total output or total employment per \$1 of government spending provides a convenient summary measure of the multiplier path. Collapsing these effects into a single dependent variable makes calculations of standard errors straightforward.

B. *Comparison to Time Series Regression*

It is informative to compare equation (1) to a typical time series regression (*ts* for time series) used to estimate a fiscal multiplier:

$$D_{t,t+h} Y = \alpha + \beta_h^{ts\prime} F_t + \gamma_h' X_t + \epsilon_{t+h}, \tag{4}$$

where $Y_t = \sum_s Y_{s,t}$, $F_t = \sum_s F_{s,t}$, and X_t is a vector of covariates.

Two main challenges arise in estimating equation (4). First, fiscal policy may adjust in response to a changing economic trajectory. This reverse causality affects both discretionary fiscal policy and automatic stabilizers. Researchers must then identify some subset of changes in F_t, which are orthogonal to ϵ_t. Popular approaches include war spending (Barro 1981; Ramey and Shapiro 1998; Hall 2009), narrative cataloging of policy changes taken for reasons unrelated to business cycle management (Romer and Romer 2010), and VAR recursive or sign restrictions (Blanchard and Perotti 2002; Mountford and Uhlig 2009).

The second challenge comes from policy variables that coincide with or respond to changes in the researcher's measure of fiscal policy. The response of monetary

[4] If $Z_{s,t}$, the component of spending that satisfies the exclusion restriction, is correlated with the rest of spending, there may be reason for concern that the variation underlying $Z_{s,t}$ is truly as good as randomly assigned. In two cases such concern is not warranted. First, other categories of spending may endogenously respond to the randomly assigned part. Then in the terminology of applied microeconomics, the reduced-form coefficient measures the intent-to-treat and the 2SLS coefficient the effect of the treatment-on-the-treated. Second, the researcher may have identified only a subset of the randomly assigned part of spending. Expanding the example in the text, let $\hat{G}_{s,t} = \hat{G}_{s,t}^1 + \hat{G}_{s,t}^2$, $Z_{s,t} = \hat{G}_{s,t}^1$, and suppose $\mathrm{corr}[\hat{G}_{s,t}^1, \hat{G}_{s,t}^2] = \rho > 0$. Then the first-stage coefficient $\Pi = 1 + \rho\sqrt{\mathrm{var}(\hat{G}_{s,t}^2)/\mathrm{var}(\hat{G}_{s,t}^1)} > 1$, the exclusion restriction remains valid, and only the 2SLS coefficient has a meaningful interpretation.

policy and what happens to other spending or taxes provide leading examples.[5] Thus, an estimate of β^{ts} to exogenous changes in government spending gives the average effect over the behavior of current and future monetary policy and taxes in the researcher's sample, and may provide a poor out-of-sample guide to the effects of government spending under alternative monetary or fiscal regimes.

The cross-sectional approach impacts both of these issues. The time effect $\alpha_{h,t}$ in equation (1) removes the direct concern of endogenous fiscal response at the highest (e.g., federal) level. Instead, the researcher need only find a valid reason why $F_{s,t}$ varies across geographic areas. Importantly, the time effect does not immediately absolve the researcher of all concerns of countercyclical federal fiscal policy; targeting of a federal intervention toward geographic areas more impacted by the recession would violate the requirement that the areas be otherwise on similar economic trajectories. The time effect also absorbs any monetary policy response or change in other federal fiscal variables. This consequence of cross-sectional estimation creates both opportunities and challenges. On the one hand, removing the effect of the endogenous response of monetary policy or taxes makes the estimate of β_h^{xs} more directly tied to primitives of the economic environment and, hence, potentially more stable across studies, a point emphasized by Nakamura and Steinsson (2014). On the other, it creates some distance between the cross-sectional multiplier β_h^{xs} and the aggregate multiplier β_h^{ts}, an issue I turn to next.

II. Theory of Cross-Sectional Multipliers

The objective of this section is to develop a relationship between the cross-sectional multiplier and a judiciously chosen theoretical construct, the closed economy, no-monetary-policy-response, deficit-financed national multiplier. Many of the concepts arise in the static Old Keynesian model and its open economy counterpart Mundell-Fleming; others affect intertemporal budget constraints and arise only in more modern treatments. The discussion in the text focuses on key economic concepts which do not depend on a particular model environment. The online Appendix presents an example of a complete algebraic model of a cross-sectional multiplier based on Farhi and Werning (2016).[6] Shoag (2016) and Nakamura and Steinsson (2014) also develops many of these points formally.

I start by introducing a convenient theoretical counterpart to β_h^{xs} in equation (1). To fix ideas, consider the following setting. A closed national economy consists of a unit continuum of local areas that share a common currency. At time t a new path of government spending is announced for a single local area s with deviation at horizon h of $\Delta G_{s,t+h}$. I defer for the moment discussion of the financing of the new path of spending. The path of government spending in the rest of the economy remains unchanged. Because area s is infinitesimal, changes in spending in s do not

[5] Theories emphasizing the codetermination of monetary and fiscal policy suggest these two cases are one and the same (Leeper 1991). In principle, F_t could include the expected paths of government spending and taxes, but it rarely does.

[6] Relative to that paper, the presentation in the online Appendix makes a few functional form assumptions at the outset and provides sufficient algebraic detail to allow an uninitiated reader to follow along with minimal interruption.

measurably affect the whole economy. The difference-in-difference in outcomes at horizon h is therefore $(Y_{s,t+h} - Y_{s,t}) - (Y_{t+h} - Y_t) = D_{t,t+h} Y_s - D_{t,t+h} Y$, where now $Y_t = \int_s Y_{s,t}\, ds$ is the average value of Y in the economy. Again letting $F_{s,t}$ denote some measure of the increase in spending (for example, the contemporaneous increase $\Delta G_{s,t}$ or a present value), the counterpart to equation (1) is

$$\beta_h^{xs} = \frac{D_{t,t+h} Y_s - D_{t,t+h} Y}{F_{s,t}}. \tag{5}$$

I argue that in empirically relevant cases β_h^{xs} provides a lower bound for the effect of increasing spending in the entire economy when monetary policy remains passive. I proceed in two steps. First, I show when an outside-financed local multiplier approximately coincides with a deficit-financed local multiplier. Second, I review standard economic channels familiar to the open economy literature that make local deficit-financed multipliers a lower bound for the no-monetary-policy-response aggregate multiplier.

A. *Relationship to Deficit-Financed Currency Union Spending Multiplier*

The multiplier defined in equation (5) has a close relationship to deficit-financed stimulus policies by individual states or countries operating inside a monetary union. For example, the consequences of fiscal austerity by members of the euro area have received a great deal of attention. The possible difference between such policies and the cross-sectional multipliers reviewed below arises because in the vast majority of cases the spending used to identify cross-sectional multipliers does not require higher contemporaneous or future local taxes. For example, when the federal government directs additional highway funds into a particular state, the tax burden associated with paying for the additional spending falls on residents of all states equally. I refer to such examples as financed by outside transfers, although in practice they may also involve windfalls generated by other factors such as pension fund abnormal returns, as in Shoag (2016).

To understand the difference between multipliers financed by outside transfers and deficit-financed spending, it helps to further fix some terminology. Let $\beta_h^{xs,\,transfer}$ denote the cross-sectional multiplier at horizon h when the spending is financed by external transfers, and $\beta_h^{xs,\,deficit}$ the cross-sectional multiplier when spending is locally deficit-financed. One can think of outside-financed spending as comprising an increase in a path of spending that is locally deficit-financed by issuance at date t of a perpetuity bond and the immediate purchase and cancellation of the perpetuity by the central government. The present value of the increase in spending, or equivalently the present value of the transfer from the central government to cancel the higher debt, is equal to $V = \int_0^\infty e^{-rj} \Delta G_{s,t+j}\, dj$, where r is the real interest rate. Let $\beta_h^{transfer}$ denote the multiplier associated with the resources used by the central government to cancel the locally issued debt. It follows that

$$\beta_h^{xs,\,transfer} F_{s,t} = \beta_h^{xs,\,deficit} F_{s,t} + \beta_h^{transfer} V. \tag{6}$$

I next consider two cases, one an economy inhabited by fully rational agents who can borrow and lend freely and where Ricardian equivalence holds, and the other economies with non-Ricardian agents. In the first, $\beta_h^{transfer} V$ is small as long as the increase in spending is transitory and the local economy is not too closed. In the second, $\beta_h^{transfer} V$ can go to zero. These cases clarify the conditions under which a transfer-financed, cross-sectional spending multiplier closely or exactly resembles a deficit-financed, cross-sectional multiplier.

When Ricardian Equivalence Holds.—If Ricardian equivalence holds, the wedge between the outside-financed multiplier and the local deficit-financed multiplier depends on the size of the transfer, which in turn depends on its persistence, and on the region's openness. A simple calculation helps to illustrate. Suppose spending increases by $\Delta G_{s,t}$ on announcement and then decays exponentially at rate ρ, $\Delta G_{s,t+j} = e^{-\rho j} \Delta G_{s,t}$, and is financed by the federal government. Then the present value of the transfer is $V = \Delta G_{s,t} \times 1/(r+\rho)$. The annuity value, equal to the per period interest payment on a perpetuity bond with face value V, is $rV = \Delta G_{s,t} \times r/(r+\rho)$. For a log utility permanent income agent, the partial equilibrium effect of a wealth transfer on consumption expenditure equals this annuity value.[7]

When the transfer is transient (ρ is large), the annuity value rV is small relative to the increase in government purchases. The small partial equilibrium response to a transfer to pay for transient spending explains why the term $\beta_h^{transfer} V$ can be small in the Ricardian case. Conversely, the partial equilibrium effect of a permanent increase in outside-financed spending ($\rho \to 0$) is to immediately raise expenditure by local agents by fully the amount of the increase in government spending. Openness matters because in general equilibrium the local output multiplier depends on the extent to which local residents concentrate their expenditure on locally-produced output.

The online Appendix derives a simple expression combining these elements for the increase in nominal expenditure on local output, $\beta_h^{transfer,\,nominal}$, in a fully intertemporal, Ricardian setting:

$$\beta_h^{transfer,\,nominal} V = \left(\frac{1-\alpha}{\alpha}\right)\left(\frac{r}{r+\rho}\right) \Delta G_{s,t}, \tag{7}$$

where α is the share of purchases from other regions in local expenditure (see equation A.39 in the online Appendix). Equation (7) has the following interpretation. The transfer causes a direct, partial equilibrium increase in expenditure on local output of $(1-\alpha) rV$, the product of the home expenditure share and the total partial equilibrium expenditure increase. The resulting increase in local income of $(1-\alpha) rV$ causes a "second round" increase in expenditure on local output of $(1-\alpha)^2 rV$, and so on. In general equilibrium, therefore, expenditure on domestic output rises in response to the transfer by $\left[(1-\alpha) + (1-\alpha)^2 + \cdots\right] rV = \left[(1-\alpha)/\alpha\right] rV$

[7] That is, for an agent with intertemporal preferences over consumption c given by $U_t = \int_0^\infty e^{-rj} \ln(c_{t+j})\, dj$ and a budget constraint $\int_0^\infty e^{-rj} p_{t+j} c_{t+j}\, dj = W$, optimization requires $p_{t+j} c_{t+j} = rW \; \forall j$. The annuity value is also the required per period transfer from the federal government to the local region to absolve the local region of ever needing to raise taxes to pay for the spending.

$= \left[(1-\alpha)/\alpha \times r/(r+\rho)\right]\Delta G_{s,t}$.[8] Thus, nominal expenditure jumps upon announcement of the transfer and remains at the same higher level thereafter. With sticky prices the local price level does not jump, so the impact transfer multiplier on real output is also given by equation (7). Choosing for illustrative purposes $\alpha = 1/3$, $r = 0.03$, and $\rho = 0.8$ (the last implies about 80 percent of the increased spending occurs by date $t = 2$), the fact that the spending is outside financed raises local output on impact by only $0.07\Delta G_{s,t}$. Setting $F_{s,t} = \Delta G_{s,t}$ in equation (6), in this example $\beta_{h=0}^{xs,\,transfer} = \beta_{h=0}^{xs,\,deficit} + 0.07$, a small difference relative to empirical estimates of $\beta^{xs,\,transfer}$ discussed below. As the price of local output rises in response to the higher demand, the transfer exerts an ever smaller and, with wealth effects on labor supply, eventually negative effect on local output (see equation A.35 in the online Appendix). Thus, the impact effect of 0.07 gives the maximum increase in the cross-sectional spending multiplier due to outside financing at any horizon in this calibrated example.

Failures of Ricardian Equivalence.—Failures of Ricardian equivalence can drive $\beta^{transfer} \rightarrow 0$ such that the outside-financed and locally deficit-financed multipliers exactly coincide. The reason stems crucially from the comparison of outside-financed spending with deficit-financed rather than tax-financed spending. For non-Ricardian agents, there is an exact analog between having agents in future periods pay for current spending and having agents in other areas pay for current spending.

It is informative to consider three leading reasons for the failure of Ricardian equivalence. In the first, private agents do not internalize the prospect of higher future taxes to pay for current spending into their budget constraints due to life-cycle considerations and non-altruistic motives (Weil 1987). If agents do not incorporate future tax liability into their private intertemporal budget constraints, then the outside-financed and locally deficit-financed multipliers coincide. Liquidity constraints provide a second leading reason Ricardian equivalence may fail.[9] If households consume and firms invest based on current income rather than permanent wealth, then $\beta^{transfer} = 0$ and an increase in temporary income resulting from a deficit-financed stimulus package will have equivalent effects to an outside-financed increase in spending. A third failure stems from myopic or boundedly rational beliefs (Gabaix 2017). If agents ignore the intertemporal aspect of their spending problem, then the outside-financed and locally deficit-financed multipliers again coincide. Similarly, if agents do not know their region has received an outside transfer, then their private

[8] Equivalently, the increase in domestic nominal income (denominated in the national price level) equals $rV + \left[(1-\alpha)/\alpha\right] rV = (1/\alpha) rV$, exactly the amount required for domestic agents to purchase an additional rV of output produced in other regions and keep the current account balanced. The difference between outside and locally financed multipliers vanishes as the economy becomes fully open, since then private spending by local agents does not fall disproportionately on local products.

[9] Evidence for liquidity constraints comes from households' responses to one-time stimulus payments (Johnson, Parker, and Souleles 2006; Sahm, Shapiro, and Slemrod 2012; Parker et al. 2013; Hausman 2016), from direct examination of households' liquidity positions (Lusardi, Schneider, and Tufano 2011; Kaplan, Violante, and Weidner 2014), and from firms' responses to temporary cash flows (Fazzari, Hubbard, and Petersen 1988; House and Shapiro 2008; Zwick and Mahon 2016).

spending cannot react to the transfer. The low salience case appears plausible in many instances. In the context of studies of national increases in spending with differential increases across regions, households would have to know the geographic spending pattern in order to react to any transfer component.

These examples make clear that in the non-Ricardian case the coincidence result requires comparing outside-financed spending to a deficit-financed stimulus package. Otherwise, there is an offsetting decline in private spending from the contemporaneously higher taxes that does not occur in the outside-financed case.

Quantitative Magnitude.—How much could the transfer component matter quantitatively? In models similar to that described in the online Appendix in which private agents internalize all future taxes into their budget constraints and calibrated to match approximately the openness and persistence of government spending in many of the studies reviewed below, Nakamura and Steinsson (2014, table 8) and Farhi and Werning (2016, table 1) both find outside financing raises multipliers by less than 0.1, that is, a locally deficit-financed multiplier of 1.2 would become a multiplier of about 1.25 if outside-financed. This magnitude matches the illustrative calculation reported above. Intuitively, low persistence of stimulus spending and fairly open local regions mean that the increase in purchases of local output in response to the transfer component is small. Farhi and Werning (2016) find this difference remains small even in the presence of non-Ricardian hand-to-mouth agents as long as the comparison remains to a local deficit-financed multiplier.

B. *Relationship to Closed Economy No-Monetary-Policy-Response Multiplier*

Multipliers associated with spending by one entity in a currency union differ from closed economy multipliers. This section discusses the most important reasons why: absence of the possibility of a reaction by monetary policy, relative price effects which cause agents to expenditure-switch toward output produced in other regions, changes in private spending by local agents fall partly on output produced in other regions, and factor mobility. I conclude that the balance of these forces likely makes the local deficit-financed multiplier a lower bound for a particular national multiplier, the closed economy no-monetary-policy-response multiplier.

Monetary Policy Reaction.—The first difference—offsetting interest rate changes by monetary policymakers that reduce the national multiplier—can matter substantially. However, there exists a leading case when monetary policy cannot react to national fiscal policy—when the zero lower bound binds at all horizons. Indeed, determining the national fiscal multiplier when nominal interest rates cannot react is of particular interest to policymakers. I call this multiplier the no-monetary-policy-response multiplier. For many models, such a multiplier provides an easy moment to target. In reality, there is not an exact equivalence with the zero lower bound because monetary policy can choose not to react to fiscal policy even outside the zero lower bound, because central banks have tools (forward guidance, quantitative easing, negative interest rates) even after the policy rate reaches zero, and because expectations of future monetary policy can change without explicit guidance from the central bank. I intentionally use the phrase "no-monetary-policy-response" rather than "zero lower bound" or "liquidity trap" to remind the reader that in actual practice a short-term policy rate of zero is not by itself a sufficient condition for the lower bound result developed in this section to apply.

Expenditure Switching.—By purchasing local output, government spending may cause the price of local output to rise relative to goods produced in other regions. Such price increases could reflect increases in factor prices, markups, or diminishing returns to scale. As a result of this terms-of-trade effect, both local and external consumers and businesses shift expenditure toward output produced in other regions, causing total private purchases of locally produced output to fall. This effect makes the cross-sectional multiplier smaller than the closed economy multiplier. Its magnitude depends on factors such as the nature of price and wage setting, the degree of segmentation between goods purchased by government and the private sector, and the substitutability between locally produced and externally produced goods.[10] I elaborate briefly here on three elements where future research might contribute to a better quantitative understanding. The online Appendix provides algebraic detail.

First, the expenditure-switching channel requires that higher government spending actually causes local prices to rise. Absence of high-frequency, high-quality local price measures has made estimating the relative price effect difficult. In the context of spending multipliers, Nakamura and Steinsson (2014) find no evidence of local consumer prices responding to government spending. The stability of inflation throughout the Great Recession has also led to some suggestions of a recent divorce between output and inflation dynamics (Hall 2011). On the other hand, using geographic variation in local demand caused by factors other than government spending, Fitzgerald and Nicolini (2014); Stroebel and Vavra (2014); and Beraja, Hurst, and Ospina (2016) all find evidence of local prices responding to local demand conditions.

Second, by assumption, government spending concentrates on goods and services from the local region; otherwise the cross-sectional multiplier experiment lacks variation in treatment across regions. Even if the higher government demand for local goods increases their relative price, however, this price increase must spillover into goods and services purchased by private agents to affect their spending. Such spillovers can happen either through competition in output markets (for example, if government and private agents purchase the same goods), or through competition in input markets (for example, due to labor mobility across sectors and a common wage). Segmentation on either dimension will dampen the amount of expenditure switching.

[10] The magnitude does not depend monotonically on the openness of the local region (see equation A.44 in the online Appendix or Farhi and Werning 2016, 2446). On the one hand, when local agents purchase a large share of their consumption from local producers, their desire to reduce total consumption when the price of a unit of utility (i.e., the real interest rate) is temporarily high causes a larger direct reduction in demand from local producers. On the other hand, this reduction in demand by local purchasers mitigates the rise in the relative price of locally produced output, which, in turn, mitigates the decline in demand from external purchasers. As a result, the increase in the relative real interest rate emphasized by Nakamura and Steinsson (2004) is not strictly necessary to generate a reduction in private demand for local goods. In fully open regions with a private sector "home bias" share of zero, consumption baskets and consumer price indexes of local and external consumers coincide, and, hence, real interest rates coincide, but total private demand for local output still falls because of the relative rise in the local producer price index.

Third, the transmission from relative price changes to expenditure switching depends on the elasticity of substitution between locally produced and externally produced goods. For temporary government spending shocks, the short-run elasticity is most relevant.

Income Effects.—The local multiplier also depends on total private spending by local agents, as any increase in demand "leaks" into other areas. For example, liquidity-constrained workers whose labor income rises in response to the increase in government spending increase their consumption of both locally produced and external goods. Complementarity in the utility function between consumption and hours worked would induce the same effect. For firms, excess sensitivity of investment to cash flow or increased purchases of intermediate inputs may cause local firms to increase purchases from both local and external suppliers. This channel is distinct from expenditure switching because it does not require any change in relative prices to occur. Once again, however, leakage makes the cross-sectional multiplier a lower bound for the aggregate closed economy multiplier.

The importance of income effects depends on both the rise in purchases by domestic agents and the openness of the local area. For example, with rigid relative prices and a mechanical marginal propensity to consume (mpc) and to import (mpm), the local government spending multiplier equals $1/(1-(mpc-mpm))$. In most settings, the smaller the local area, the larger the share of purchases from outside the region. Therefore, this effect suggests the cross-sectional multiplier may increase in the level of the geographic unit, i.e., it is larger for states than for counties. Recognizing this fact, some cross-sectional studies that examine variation at a county level enlarge the region covered by the dependent variable to capture some of the spending leakage.

Factor Mobility.—In contrast to the expenditure switching and income channels, high factor mobility may push up local multipliers relative to national multipliers. For example, as local government spending causes local labor demand to rise, workers may move in from other areas. The population influx further raises local employment and output as the immigrants consume non-tradeable output and push down wages in tradeable sectors. More generally, supply constraints may be less likely to bind at the local level.

Because of fixed costs of moving, the importance of the migration channel rises with the persistence of the spending. Likely for this reason, none of Farhi and Werning (2016), Shoag (2016), or Nakamura and Steinsson (2014) allows for net migration in their theoretical model of cross-sectional multipliers. In contrast, studies of longer run changes or more persistent policies treat population spatial equilibrium as a key force.[11] With fixed costs the importance of factor mobility also

[11] See, e.g., Moretti (2010) for analysis of long-run employment multipliers and Glaeser and Gottlieb (2008) for a formal spatial equilibrium model. Moretti (2010) estimates the additional aggregate local employment caused by an additional job in different sectors, at decadal frequency. Assuming that immigration makes the local labor supply elasticity larger than the national, he argues that the employment multiplier of an additional job in a non-traded sector provides an upper bound of the national spending multiplier, and the multiplier of an additional job in a traded sector provides a lower bound. This argument also implicitly assumes changes persistent enough to induce

depends on the size of the local geographic unit, as migration of workers or capital across neighboring counties engenders smaller costs than migration across states. Shoag (2016) and Nakamura and Steinsson (2014) each estimate the cross-state population response to local government spending and find economically and statistically insignificant responses. Thus, for temporary increases in local government spending the empirical relevance of the migration channel appears small.

Other Channels.—Other potential differences between local currency union and national multipliers are hard to quantify or even sign. Confidence provides one example. By passing a countercyclical fiscal stimulus, a national government might raise consumer and business confidence in the government's competence or more nebulously trigger "animal spirits." Alternatively, if private agents view the spending as an insufficient response to the circumstances or contaminated by political favoritism, confidence might fall. Looking further ahead, the political reaction to national spending might affect outcomes of future elections and, hence, a host of other policies. Because these channels have ambiguous sign and vary with specific circumstances, they resist incorporation into a general framework. Put differently, local multipliers can inform only about a national multiplier for which channels such as confidence in the national government do not play a role.

C. *Summary and Discussion*

As described in Section IIA, multipliers for transitory increases in local spending not financed locally map roughly into locally deficit-financed currency union multipliers. Section IIB argued that locally financed currency union spending multipliers provide a lower bound for closed economy no-monetary-policy-response multipliers due to the dominance of the expenditure-switching and leakage effects. Combining these two results, standard theory suggests that in empirically relevant cases cross-sectional multipliers provide a rough lower bound for closed economy, deficit-financed, no-monetary-policy-response multipliers.[12] While shared by Nakamura and Steinsson (2014) and Farhi and Werning (2016), this conclusion is sharply at odds with much of the conventional wisdom extant at the start of this wave of research.[13]

migration, and that output and employment are not demand-constrained. While possibly reasonable assumptions for the decadal frequency Moretti examines, failure of these assumptions at shorter horizons make the bounds inapplicable to short-run spending multipliers.

[12] A recent literature has questioned the plausibility of some of the forward-looking elements of the New Keynesian model, which give rise to potentially very large closed economy multipliers when monetary policy does not react (Carlstrom, Fuerst, and Paustian 2015; Del Negro, Giannoni, and Patterson 2015; McKay, Nakamura, Steinsson 2016; Kaplan, Moll, and Violante 2016). The rough lower bound result does not depend on these particular features. Indeed, aspects that make the New Keynesian model less forward-looking also rule out one case discussed by Farhi and Werning (2016) in which closed economy, deficit-financed, no-monetary-policy-response spending may generate a contemporaneous multiplier of less than the locally financed currency union multiplier, wherein the presence of liquidity constrained agents results in expectations of a recession in the future at the time taxes rise, thereby generating in the closed economy case a deflationary spiral that reduces current expenditure by unconstrained agents. Enough price rigidity also rules out this outcome.

[13] For example, Giavazzi (2013, 144) writes that "local multipliers deliver an *upward* biased estimate of total spending multipliers" (emphasis mine). Ramey (2011a, 681) provides a widely cited example where this conclusion holds. In Ramey's example, all agents have a mechanical marginal propensity to consume (*mpc*)

I conclude this section by returning to assumptions made at the outset concerning the size of the local region and the national economy's openness. The assumption that spending occurs in a single area s of infinitesimal size highlights an important difference between the issues that affect the mapping from β_h^{xs} to β_h^{ts} and the no-interference stable unit treatment value assumption (SUTVA) often made in analyses of clinical trials and other randomized experiments. SUTVA states the condition that for the difference between treated and untreated units to provide a valid estimate of the causal effect of treatment, treatment of one unit must not affect outcomes of the non-treated units. When s is infinitesimal, the spillovers from higher local spending, arising *inter alia* from expenditure switching, income effects, and migration, are infinitesimal relative to the rest of the economy, and SUTVA holds. Nonetheless, the local multiplier estimated from the difference in outcomes between the single region s and the whole economy may differ from the effects of spending in the entire economy because economic integration has first-order effects on outcomes in s.[14]

This discussion makes clear two additional issues. First, when s is not infinitesimal, SUTVA will not hold, and β_h^{xs} measures the effects of spending on outcomes in s relative to the effects in other areas. Thus, if the cross-sectional multiplier based on spending in one local area understates the national no-monetary-policy-response multiplier, the cross-sectional multiplier based on increasing spending in a randomly chosen half of all US states would further understate the national multiplier. In practice, however, most studies consider sufficiently small geographic units that the infinitesimal assumption likely provides a reasonable approximation. Second, while adding spending in other areas to equation (1) can potentially incorporate some of the spillovers, it does not turn β_h^{xs} into a national multiplier.

Finally, what if the national economy is not closed? Openness of the national economy does not affect the local multiplier when s is infinitesimal because the national economy does not respond to changes in s. However, as international macroeconomists have known since Mundell (1963) and Fleming (1962), national multipliers depend on the openness of the national economy for reasons similar to those discussed in Section IIB. The comparison to a closed economy multiplier simply reflects the absence of information from a cross-sectional multiplier for the difference between the multiplier in national closed versus open economies. As a result, while the concept of a closed economy, no-monetary-policy-response multiplier

of 0.6, and households in Mississippi receive a government transfer of \$1 financed by a contemporaneous lump-sum tax levied on households in other states. Then, as Ramey points out, the increase in output in Mississippi and, hence, the local multiplier, equals $mpc/(1-mpc) = 1.5$, but the national multiplier is 0. (Following Ramey, this calculation assumes that all consumption is of locally produced output.) Changing the example slightly, however, suppose instead that Mississippi financed the transfer by issuing debt purchased by foreigners. Then the local deficit-financed multiplier also equals 1.5, the same as the outside-financed multiplier and the national deficit-financed multiplier. Thus, the rough lower bound result differs from Ramey's conclusion because it compares local multipliers to national deficit-financed multipliers, whereas her example compares the local multiplier to a national tax-financed multiplier.

[14] Formally, suppose the national economy has population normalized to one and consists of N equally sized regions. The effects of economic integration on the local region s and the rest of the economy are both of order $1/N$. As $N \to \infty$, the effect of spending in s on the national economy vanishes, but the effect on the local region remains of the same order of magnitude as the region's size, $1/N$. Note also that while related empirically, the concepts of region size and openness are theoretically distinct; a region of size $1/N$ may sell an arbitrary fraction α of its output to other regions.

offers a convenient theoretical construct, there may be a gap between this object and the multiplier that applies in actual circumstance.

III. Relationship between Employment and Output Multipliers

Many geographic cross-sectional studies report employment multipliers rather than output multipliers. This outcome reflects necessity as much as choice; the Bureau of Economic Analysis only began in December 2015 to publish real gross state product (GSP) at a quarterly frequency and measures of output at the county level remain in development. In contrast, the Bureau of Labor Statistics publishes monthly employment by state or county based on high-quality administrative payroll tax data. The availability of high-quality employment but not output data at a local level holds true in many other countries as well. Before turning to the empirical studies, therefore, I derive a mapping between employment and output multipliers.

Let β_h^Y denote the output multiplier and β_h^E the employment spending multiplier. That is, for a deviation in spending of ΔG_t, by definition

$$\Delta Y_{t+h} = \beta_h^Y \Delta G_t,$$

$$\Delta E_{t+h} = \beta_h^E \Delta G_t,$$

where Y_t is GDP, E_t is employment, G_t is government spending, Δ denotes the deviation from some baseline path, and I drop the geographic subscript for simplicity. Let $e_{t+h} = \Delta E_{t+h}/E_t$ denote the percent change in employment caused by the spending, $y_{t+h} = \Delta Y_{t+h}/Y_t$ the percent change in output, and $g_t = \Delta G_t/Y_t$ the deviation of spending as a share of output. It will be useful to write

$$e_{t+h} = \beta_h^E \frac{Y_t}{E_t} g_t.$$

I assume a production function relating outputs and inputs $Y_t = A(N_t E_t)^{1-\xi}$, where N_t denotes hours per worker. Implicitly, this functional form assumes capital does not adjust in the short run. Let $n_t = \Delta N_{t+h}/N_t$. Then,

$$\beta_h^Y = \frac{y_{t+h}}{g_t} = \frac{y_{t+h}}{e_{t+h}}\frac{e_{t+h}}{g_t} \approx (1-\xi)(1+\chi)\frac{Y_t}{E_t}\beta_h^E, \tag{8}$$

where $\chi = n_t/e_t$ denotes the elasticity of hours per worker to total employment. For the United States, $\xi \approx 1/3$ and $\chi \approx 0.5$, yielding a combined multiplicative factor of $(1-\xi)(1+\chi) \approx 1$.[15]

As an alternative to the above approach, Nakamura and Steinsson (2014, table 2) reports estimates of both β^Y and the combined factor $\beta^E \times Y/E$, the latter being the coefficient from a regression of e_t on g_t. The ratio of these two estimates is also close to unity. I find a similar conversion factor in the next section for directly estimated

[15]The estimate of $\xi = 1/3$ based on factor income shares is standard. Okun (1962) provides an early estimate of the relative movement of hours per worker and employment and Elsby, Hobijin, and Sahin (2010) an updated estimate.

employment and output multipliers. Therefore, for the United States a rough translation from employment to output multipliers is to divide output per worker Y/E by the cost per job $1/\beta^E$, taking care to make sure that Y/E and β^E correspond to the same calendar length of time.[16]

IV. Example of a Cross-Sectional Multiplier

I illustrate the cross-sectional approach by presenting a unified set of results based on cross-state variation generated by the American Recovery and Reinvestment Act (ARRA). Enacted in February 2009, the ARRA included new spending, transfers, and tax reductions totaling roughly \$800 billion. As legislation proposed by the incoming president with the explicit intent of mitigating the recession already underway, the ARRA offers little useful time series variation for assessing the consequences of fiscal policy.[17] Instead, researchers have identified aspects of the spending allocation that resulted in geographic variation plausibly exogenous to economic trends. Crucially, more than half of the budgetary outlays went either to contractors directly or to subnational governments, and an unusual provision of the bill, section 1512, tracked such spending by requiring federal agencies to report outlays in each state and all prime recipients to report the funds received. The combination of the variation in geographic entitlement in many of the act's programs and the detailed data collection facilitated research efforts.

A. *Econometric Choices*

I implement equation (3) as a purely cross-sectional 2SLS regression:

$$\left[\sum_{h=0}^{H}\left(Y_{s,t+h} - Y_{s,t}\right)\right] = \alpha + \beta^{xs} F_s + \gamma' X_s + \epsilon_s, \tag{9}$$

$$F_s = \Pi_0 + \Pi_1' Z_s + \Pi_2' X_2 + \nu_s, \tag{10}$$

where Y_s is either annualized employment (normalized by the adult population) or gross state product (GSP):

$$Y^{emp}_{s,t+h} - Y^{emp}_{s,t} = \frac{1}{12}\left(\frac{Employment_{s,t+h} - Employment_{s,t}}{Working\ age\ population_{s,t}}\right),$$

$$Y^{GSP}_{s,t+h} - Y^{GSP}_{s,t} = \frac{GSP_{s,t+h} - GSP_{s,t}}{GSP_{s,t}}.$$

[16] Congressional Budget Office (2009) prefers a larger combined adjustment factor based on the historical relationships among the output gap, the unemployment rate, and labor force participation. A possible reason may be that the historical relationships do not take account of the reason for the movement in the output gap.

[17] A few papers have used historical time series patterns to study the ARRA (Romer and Bernstein 2009; Cogan and Taylor 2012; Carlino and Inman 2013).

I set t, the start of the treatment period, to December 2008. As emphasized by Ramey (2011b), agents may start responding to a fiscal shock at the moment of announcement and estimates of multipliers should incorporate these anticipation effects. Chodorow-Reich et al. (2012) argue that important components of the ARRA became apparent in that month. Roughly three quarters of the total ARRA had been outlaid by December 2010, with the remainder spread over a number of years. The endogenous variable F_s is therefore total ARRA outlays through 2010:12 and is expressed either as a ratio to the adult population or to GSP to match the normalization of the dependent variable. I set H to 24 months to match the duration over which I measure spending. Thus, equation (9) measures the effect of \$1 of outlays on either cumulative output or cumulative employment, with the latter expressed in terms of the number of "job-years" by dividing the summation of additional monthly employment by 12.

I consider three measures of $Z_{s,t}$ used in prior studies. Each follows the logic that allocating \$800 billion in a short legislative timespan required Congress to use existing spending formulas that did not particularly target areas hardest hit by the recession. The first, *FMAP*, comes from Chodorow-Reich et al. (2012). Roughly \$90 billion of federal aid to state governments came in the form of an increase in the federal share of Medicaid expenditure (the Federal Medical Assistance Percentage, or FMAP), effectively giving larger grants to states with higher secular per capita Medicaid spending. Because the increase in the FMAP also depended on the state's unemployment rate, which is clearly endogenous to the economic trajectory, Chodorow-Reich et al. (2012) use pre-recession Medicaid spending as an instrument for the FMAP transfer component. The second proposed $Z_{s,t}$, *DOT*, comes from Wilson (2012) and Conley and Dupor (2013), who note that the distribution of \$27 billion of highway construction spending depended on pre-recession formulas using as inputs pre-recession total lane miles of federal highway, total vehicle miles traveled on federal highways, tax payments paid into the federal highway trust fund, and Federal Highway Administration obligation limitations. I follow Wilson (2012) and use a linear combination of these four factors as an instrument for Department of Transportation spending. Dupor and Mehkari (2016) take the idea of identifying spending allocated according to pre-recession formulas to its logical extreme and aggregate all components of the ARRA that fit this description. The components identified by Dupor and Mehkari (2016) constitute the third proposed $Z_{s,t}$, *DM*.[18] I normalize each $Z_{s,t}$ by either the adult population or GSP to match the normalization of the dependent variable.

I include three measures of economic conditions in X_s: the employment change from 2007:12 to 2008:12, the growth rate of GSP from 2007:IV to 2008:IV, and the 2008:12 employment level, where the employment variables are normalized by

[18] The Dupor and Mehkari (2016) use of local recipient reporting means that their list of programs excludes the FMAP increase and the highway spending. I make the following changes to the instruments used in these papers. For Wilson (2012), I update the projection of ARRA highway obligations on the four formulaic components to include obligations in 2010. For Dupor and Mehkari (2016), I use the agency-reported spending in their identified programs rather than the spending as reported by recipients.

dividing by the 2008:12 adult population.[19] A good control variable should correlate either with the outcome variable ("economic trajectory" controls) or the instrument ("exclusion restriction" controls), where of course these sets may overlap. In this case, controlling for pretreatment trends both reduces standard errors by absorbing residual variation in the dependent variable and weakens the exclusion restriction by making the as good as random assignment conditional on the preexisting economic conditions. Nonetheless, with 50 observations and many seemingly sensible control variables, the choice of covariates can matter quantitatively. Such sensitivity may just reflect in-sample over-fitting or less innocuously suggest a violation of the exclusion restriction. Each of Chodorow-Reich et al. (2012), Wilson (2012), and Dupor and Mehkari (2016) contains a more exhaustive set of control variables than used here. In Table B.2 of the online Appendix I augment X with some of these controls and find the results reported below do not change much. Such sensitivity analysis is common in the cross-sectional multiplier literature and an example of how readily the many tools developed in applied microeconomics for validating research designs can be imported (see Athey and Imbens 2017 for a recent survey).

B. *Results*

Table 1 reports the results. Columns 1–3 report the second-stage coefficient for the employment multiplier using each instrument separately. While the correlation coefficient between the *DOT* and *DM* instruments is fairly high (0.74), explaining in part the large first-stage coefficients in columns 2 and 3, neither variable is highly correlated with the *FMAP* instrument (0.04 for *DOT* and 0.14 for *DM*).[20] The estimated employment effect is remarkably stable across columns. Column 4 groups the instruments together. The coefficient of 2.01 has the interpretation that an additional \$100K of ARRA spending in a state increases employment by the equivalent of 2.01 jobs, each of which lasts for one year, or a "cost-per-job" of $\frac{\$100K}{2.01\text{ jobs}} = \$49{,}750$. Using the delta method, the 90 percent confidence interval for the "cost-per-job" is (\$25,500, \$73,900). The first stage R^2 rises substantially in column 4 and the second stage standard error falls, indicating improved efficiency by combining the instruments together. The J-statistic fails to reject exogeneity.

[19] The employment data come from the Bureau of Labor Statistics Current Employment Statistics (CES), an establishment-based count of payroll jobs based (after annual benchmarking) on monthly administrative counts of employees covered by the unemployment insurance program. I translate all employment variables into per capita by dividing by the civilian noninstitutional population 16+ in 2008:12 as reported in the BLS Local Area Unemployment Statistics.

[20] The first-stage coefficient in column 1 should differ from one because the endogenous variable covers spending over two years while the instrument corresponds to Medicaid spending in one year, because the ARRA increased the FMAP by only 6.2 percentage points, and because Medicaid spending in 2009 and 2010 was higher than Medicaid spending in 2007.

Table 1—ARRA Example

	Dependent variable				
	Job years per \$100K spent				GSP
	(1)	(2)	(3)	(4)	(5)
Endogenous variable:					
Total ARRA spending	2.29	2.22	1.82	2.01	1.53
	(0.71)	(1.22)	(0.69)	(0.59)	(1.19)
Instruments	FMAP	DOT	DM	ALL	ALL
Estimator	2SLS	2SLS	2SLS	2SLS	2SLS
First-stage coefficient	0.36	1.66	6.76	–	–
First-stage F-statistic	35.9	9.8	52.0	46.1	129.3
First-stage R^2	0.40	0.23	0.55	0.73	0.87
Hansen J-statistic p-value				0.76	0.34
Observations	50	50	50	50	50

Notes: The table reports cross-state regressions of the effect of ARRA spending on employment (columns 1–4) or gross state product (column 5) during 2009 and 2010. ARRA spending is instrumented using pre-recession Medicaid spending ($FMAP$), Department of Transportation formula (DOT), and other pre-recession forumlae (DM) as described in the text. All specifications also control for the employment change from December 2007 to December 2008 normalized by the December 2008 population 16+, gross state product (GSP) growth from the fourth quarter of 2007 to the fourth quarter of 2008, and the December 2008 ratio of employment to the population 16+. In columns 1–4 Total ARRA spending and the instruments are normalized by the December 2008 population 16+. In column 5, Total ARRA spending and the instruments are normalized by 2008:IV GSP. Eicker-White standard errors in parentheses. Following AEA guidelines, symbolic indicators of significance are omitted.

Column 5 uses the newly available GSP data to estimate an output multiplier of 1.53, although with less precision than the employment results.[21] I use the approach outlined in Section III to compare the employment multiplier estimate in column 4 with the GSP multiplier in column 5.[22] Output per worker Y/E in the national income accounts was \$105K in 2009. Applying this number to the cost-per-job estimated in column 4 yields an implied output multiplier of around two, close to and not statistically different from the direct estimate of the output multiplier of 1.53 in column 5.

[21] In column 5, I normalize the endogenous variable and the instruments by the level of GSP in 2008:IV such that the coefficient on total ARRA spending has the interpretation of a dollar-for-dollar multiplier. The degree of measurement error in real GSP data—especially as compared with state employment data, which derive from administrative tax records—may explain the larger standard error for the output multiplier. The methodology underlying the real GSP data further invites caution in their use for studying multipliers. For example, their construction does not allow for a local price response to increased government purchases (Cao, Siebeneck, and Woodruff 2016), an issue of potential importance as discussed above. Table B.1 in the online Appendix reproduces the estimates shown along with the coefficients and standard errors for the included covariates.

[22] The approach in Section III assumed an elasticity of hours per worker to employment of 0.5 based on historical business cycle co-movement. Multiplying nonfarm employment by the state-level measure of private sector hours per worker from the CES and using the resulting product as the dependent variable in a specification otherwise identical to column 4 yields a coefficient of 3.12 (SE $=$ 1.32). The ratio of the total hours multiplier to the total employment multiplier provides an alternative estimate of $1+\chi$ of 1.55, nearly identical to the value of $1+\chi$ of 1.5 I assumed above.

C. *Interpretation*

To relate the multipliers in Table 1 to the outside-financed spending multiplier studied in Section II, I return to two issues raised above which also recur in many of the studies reviewed next. The first concerns the timing of the employment and output effects. The multiplier obtained from estimating equation (9) characterizes the cumulative effect on employment or output over H periods. If the effect on Y remains positive past H periods, $\beta_h \neq 0$ for $h > H$, then the infinite-horizon multiplier would exceed the H period multiplier. Plotting the impulse response coefficients can help to assess the sufficiency of truncating the response at H periods, and I do so in online Appendix Figure B.1 for the employment response for 0 to 48 months. In this case, the coefficients remain positive past the 24-month horizon, gradually declining to 0 by month 48. A complication arises, however, because some ARRA spending continued after 2010, and because of additional stimulus enacted after the ARRA. For the latter, Council of Economic Advisors (2014, 101) lists additional measures totaling \$709 billion in spending, transfers, or tax reductions, or nearly the magnitude of the initial ARRA, with the vast majority outlaid in 2011 and 2012. Some of these measures directly extended components of the instruments used in Table 1. Because the enactment of most of these measures occurred late in 2010 or after, they had limited impact on employment or output through 2010. Interpreting the Table 1 multipliers as capturing the infinite-horizon employment effects, however, requires an assumption that the effects past month 24 stem solely from spending in 2011 and 2012.[23]

The second issue concerns the choice of endogenous variable F_s. Equation (9) follows much of the literature and uses total ARRA outlays. Yet, even if some states received more ARRA outlays for essentially random reasons, total government purchases in those states need not have increased dollar-for-dollar with the ARRA component. States could instead have used federal transfers to reduce taxes or increase balances in their rainy day funds. Even direct federal purchases (as used in some of the studies reviewed below) can crowd out or crowd in state and local spending. Data on state and local finances can distinguish among these possibilities.[24] In Appendix Table B.3, I use these data to estimate that an additional \$1 of ARRA transfers during 2009 or 2010 increases state and local expenditure by a total of \$1.22 (SE $=$ 0.63) during FY2009 and FY2010 and reduces taxes by \$0.11 (SE $=$ 0.37). The near dollar-for-dollar increase in expenditure and small effect on taxes imply the multiplier in Table 1 approximates the local outside-financed spending multiplier.

[23] Whether one wants to know the short or infinite-horizon multiplier depends on the circumstance. For example, policymakers might care about effects on output over a shorter horizon, such as 24 months, perhaps because they expect monetary policy to eventually regain traction. To assess the possible bias in the infinite horizon multiplier from truncating after 24 months, I have re-estimated the specification in column 4 of Table 1, allowing the employment effects to continue through September 2011 and including ARRA spending (plus the extension of the FMAP increase enacted in August 2010) through that month. I find a coefficient of 2.23, not statistically different from the coefficient of 2.01 reported in the table.

[24] Unfortunately, publication of such data by the Census Bureau occurs with a multiyear lag with the consequence that many studies do not make use of them. Leduc and Wilson (2017) is an important exception and finds crowding in by ARRA highway grants.

V. Summary of Empirical Cross-Sectional Multipliers

I now review recent empirical studies of geographic cross-sectional multipliers.[25]

A. *Evidence from the ARRA*

Because studies based on the geographic distribution of funds under the 2009 ARRA all cover roughly the same time period and intervention, I treat them as a separate group. Importantly from the lens of the theoretical discussion, these studies all involve outside financing of spending of the same persistence, the approximately two-year time frame of payouts from the bill.

Table 2 summarizes the results from these papers. As a concise summary measure, the final column reports the number of job-years associated with an additional \$100K of ARRA spending implied by each study. Where possible, I report the 90 percent confidence interval for this number in brackets.

The largest cross-state estimated employment effects come from the Chodorow-Reich et al. (2012) study of aid to state governments through the Medicaid matching program described in Section IV. Two aspects of the program may have led to high-employment multipliers. First, fungible aid allows state governments to direct the funds to their best use. Chodorow-Reich et al. (2012) report a concentration of effects in reduced layoffs of workers in sectors funded by state and local government. Second, states began receiving money under this program immediately after the bill's passage, in contrast to other programs such as highway construction reimbursements, most of which came one to two years later. Thus, states received the Medicaid matching transfers exactly when state budget shortfalls first materialized. On the other hand, the employment multiplier estimated in Chodorow-Reich et al. (2012) exceeds that in column 1 of Table 1, which uses similar variation but a slightly different specification.[26] Dupor (2013) emphasizes the importance of keeping the specification fixed when comparing the employment effects of different types of ARRA spending.

Conley and Dupor (2013) reports the smallest employment effects. They construct two endogenous fiscal policy variables: ARRA spending and lost tax revenue plus increased Medicaid spending; and two instruments: the formulaic component of highway spending and state tax revenue cyclicality. In their *fungibility-constrained* specification, the endogenous variable is collapsed into ARRA spending net of lost tax revenue, such that the employment effect of a dollar of ARRA aid is constrained to have the same employment effect as an additional dollar of tax revenue. This

[25] A closely related literature, and one that predates many of the papers reviewed here, studies the direct effect of various fiscal stimulus policies using cross-sectional variation in the eligibility or timing of the policy at the level of the individual recipient. See, e.g., Johnson, Parker, and Soules (2006); House and Shapiro (2008); Parker et al. (2013); Mian and Sufi (2012); Hausman (2016); and Zwick and Mahon (2016). Unlike geographic cross-sectional multipliers, these studies contain no general equilibrium effects and thus pose a distinct challenge for mapping to a national multiplier. Davis, Loungani, and Mahidhara (1997) and Hooker and Knetter (1997) are examples of earlier papers that estimate a specification similar to equation (1). Zidar (2017) estimates a similar specification for tax changes.

[26] The changes in specification include the sample period, whether DC is included, and whether the endogenous variable is FMAP or total ARRA.

Table 2—ARRA Papers

Study/Journal	Identification	Geography	Headline result	Job-years per $100K
Chodorow-Reich et al. (2012), *AEJ Policy*	Pre-recession Medicaid spending instruments state fiscal relief	State	$100K increases employment by 3.8 [1.2, 6.4] job-years	3.8 [1.2, 6.4].
Conley and Dupor (2013), *Journal of Monetary Economics*	ARRA highway obligations and state tax revenue cyclicality instrument ARRA spending net of change in tax revenue	State	$100K increases employment by 0.5 [0.05, 0.94] job-years if fungibility between ARRA and lost tax revenue imposed; 0.76 [−0.1, 1.64] job-years if fungibility not imposed.	0.76 [−0.1, 1.64].[a]
Dube, Kaplan and Zipperer (2014), *Unpublished*	County-level fixed effects regression with state × year fixed effects and Bartik and demographic controls	County	$100K increases employment in own county by 0.76 [0.39, 1.12] job-years and in all counties within 120 miles of county by 3.28 [1.73, 4.83] job-years. Employment effects larger in counties with greater excess capacity.	3.28 [1.73, 4.83].[b]
Dupor and McCrory (2018), *Economic Journal*	Formulaic Recovery Act spending by federal agencies not targeted to harder hit regions	Subregional spillovers within local labor markets	$100K increases employment by 1.03 [0.39, 1.66] and 0.85 [0.39, 1.31] job-years in own and neighboring subregion jobs, respectively, and increases wages by $64K [$28K, $100K] and $50K [$22K, $78K], respectively.	1.85[c]
Dupor and Mekhari (2016), *European Economic Review*	Formulaic Recovery Act spending by federal agencies not targeted to harder hit regions	Local labor markets	$100K increases employment by 0.95 [0.45, 1.46] job-years and wage bill by $102K [$48K, $156K].	0.95 [0.45, 1.46].
Feyrer and Sacerdote (2012), *Unpublished*	Mean seniority of a state's Congressional delegations instruments ARRA spending	State	$100K increases employment by 2.16 [0.99, 3.33] (IV) or 0.93 [0.42, 1.44] (OLS) jobs in October 2010.	1.99 [0.73, 3.21].[d]
Wilson (2012), *AEJ Policy*	Pre-recession Medicaid spending, statutory determinants of highway spending allocation, and schooling age population instrument ARRA spending	State	$100K of funding announcements increases employment in February 2010 by 0.81 [0.23, 1.39] jobs; $100K of funding obligations increases employment in February 2010 by 1.02 [0.43, 1.61] jobs.	1.75 [0.58, 2.9].[e]

[a] Fungibility, not imposed specification.
[b] Based on specification including spillovers.
[c] Summing direct and spillover effects. The covariance between the two is not reported.
[d] Feyrer and Sacerdote (2012) baseline IV regression re-estimated with the dependent variable $Y_s = \frac{1}{12}\sum_{t=2009:3}^{2010:10}\left(\frac{Employment_t}{Population_t} - \frac{Employment_{2009:2}}{Population_{2009:2}}\right)$. The corresponding range for the OLS specification is 0.98 [0.42, 1.53].
[e] Wilson (2012) baseline regression re-estimated with the right-hand side variable outlays through March 2011 and the dependent variable $Y_s = \frac{1}{12}\sum_{t=2009:3}^{2011:3}\left(\frac{Employment_t}{Population_t} - \frac{Employment_{2009:2}}{Population_{2009:2}}\right)$.

specification gives rise to a cost-per-job estimate of $200K. But as discussed in Section IIA, economic theory dictates, at most, equivalence between state spending financed by ARRA transfers and a deficit-financed increase in state spending; the fungibility assumption instead imposes equivalence between ARRA-financed spending and a balanced budget increase in state spending. Since the presence of either Ricardian or hand-to-mouth agents will deflate the balanced budget multiplier relative to ARRA-financed spending, theory suggests the constrained cost-per-job is too high. In their second specification, which does not collapse the endogenous variable, Conley and Dupor (2013) finds an employment multiplier 50 percent larger and closer in magnitude to other papers.

Wilson (2012) develops three formulaic allocation instruments: pre-recession Medicaid spending as in Chodorow-Reich et al. (2012), the schooling age population which partly determined the allocation of spending by the Department of Education, and the highway instrument described in Section IV. He reports a headline cost-per-job of \$125K. A complication arises in comparing this number to other studies, however, because it corresponds to additional employment in February 2010 relative to total announced ARRA state-level spending allocation by that month, while much of the actual spending occurred later. Using instead actual spending or spending obligated to specific entities results in lower cost-per-job estimates because spending as of February 2010 is correlated with spending after February 2010. A simple alternative specification which elides this problem follows the approach in Section IV and estimates the integral of additional jobs through some terminal date as a function of spending by that terminal date. Using March 2011 as the terminal month—the last month in the Wilson (2012) dataset and after more than 80 percent of the ARRA had been outlaid—but keeping the specification and control variables otherwise identical to Wilson (2012), I estimate a jobs coefficient of 1.75 (SE $=$ 0.71). This estimate translates into a cost-per-job of \$57K (\$100K/1.75).[27]

The last cross-state study of the ARRA is Feyrer and Sacerdote (2012). The paper reports estimates from OLS regressions of employment on ARRA and from IV estimates where ARRA transfers are instrumented using the mean seniority of a state's congressional delegation. The paper finds employment effects more than twice as large when using IV.[28] To obtain a result comparable to other studies, I re-estimate the Feyrer and Sacerdote (2012) IV specification using their data but replacing the dependent variable using equation (3) and find a jobs coefficient of 1.99 (SE $=$ 0.74), which translates into a cost-per-job of \$50K.

A few studies have examined employment effects of the ARRA at a sub-state level. Unlike the state-level studies whose data come from reporting by federal agencies of the state allocation of all ARRA outlays, allocating spending at the sub-state level requires using the recipient reporting of spending and the location of the recipients. Spending reported by recipients likely corresponds more closely to the national accounts definition of direct government purchases than does the full ARRA, which includes transfers to both individuals and state governments. Dube, Kaplan and Zipperer (2014) use panel regressions at the county level. Unlike the other studies reviewed, their identification comes solely from controlling for a large set of determinants of county economic conditions. They find a cost-per-job in the

[27] Using, instead, funding announcements through March 2011, the jobs per \$100K spent falls only slightly to 1.42 from 1.75.

[28] The instrument in Feyrer and Sacerdote (2012) is the mean seniority of the entire Congressional delegation, where House members are ordered 1–435 and Senate members 1–100, and not the mean seniority of the state's House delegation as reported in the paper. Using either seniority measure separately does not predict spending allocation. Boone, Dube, and Kaplan (2014) also investigate the political economy of the distribution of ARRA spending and find little evidence of legislative seniority mattering.

recipient county of \$100K but substantial spillovers across counties, with a cost-per-job including all counties within 120 miles of the recipient of \$30K.[29]

Finally, Dupor, and Mehkari (2016) and Dupor and McCrory (2018) develop an instrument for county-level recipiency of ARRA funds based on the formulaic components of the ARRA. Their instrument forms the basis for the "DM" instrument in Table 1. Similar to Dube, Kaplan, and Zipperer (2014), Dupor and McCrory (2018) reports evidence of substantial geographic spillovers, with the employment effect of \$100K in spending rising from one job-year in the recipient's region to 1.85 when including employment effects in other subregions belonging to the same local labor market. Dupor and Mehkari (2016) finds a smaller employment effect of 0.95 job-years at the local labor market level.

Summing up, the estimate of two job-years per \$100K in column 4 of Table 1 appears broadly representative. Put on common footing, the ARRA studies find estimates in the range of 0.76 to 3.93, with a cross-study mean of 2.1 and median of 1.9. With the exception (barely) of Dube, Kaplan, and Zipperer (2014), the confidence intervals of the ARRA studies all overlap.

B. *Other Evidence*

Estimation of geographic cross-sectional multipliers has proceeded in numerous other directions, making use of clever identification strategies and developing new datasets. Table 3 summarizes these studies.

Shoag (2016) builds a dataset of idiosyncratic returns of state pension funds. These returns relax state budget constraints and empirically predict increased government spending (but not lower tax revenue). Shoag (2016) therefore uses the pension returns as an instrument for state spending and finds a \$1 increase in spending raises personal income by \$2.12 and that \$100K of spending raises employment by 2.9 job-years.[30] While the first stage indicates states spend roughly 50 percent of the windfall in the first year, Shoag (2016) argues that private agents are unlikely to react to the windfall component other than due to the government spending because of an absence of publicity of state pension returns.

Suárez, Serrato, and Wingender (2016) starts from the observation that a multitude of federal transfers to local governments depend on local population, but censuses of population by area occur only every ten years. In the interim, the Census Bureau estimates local population growth using birth and death records and migration flows. The benchmarking to the census count every ten years then induces jumps in federal payments to a local area caused by the sudden dissipation of measurement error. Suárez, Serrato, and Wingender (2016) studies the response of local private income and total employment to these jumps in payments and finds an income multiplier of

[29] The ARRA reporting system may partly explain the estimates of large cross-county spillovers. As pointed out by Garin (2016), vendors reported spending in the county where a project occurred rather than the county containing the payroll office of the vendor. County-level employment datasets including the *Quarterly Census of Employment and Wages* and *County Business Patterns* attribute employment to the county of the vendor's payroll office.

[30] Shoag (2016) argues that personal income closely tracks output, but provides a more reliable measure of state-level economic activity over his sample.

Table 3—Non-ARRA Papers

Study/Journal	Identification	Geography/financing/persistence	Result
Acconcia, Corsetti, and Simonelli (2014), *American Economic Review*	Provincial expenditure cuts in Italy following expulsion of mafia-infiltrated city council members	Province/outside financing/transitory	Impact output multiplier of 1.55 [0.84, 2.26], cumulative multiplier of 1.95
Adelino, Cunha, and Ferreira (2017), *Review of Financial Studies*	2010 Moody's recalibration of US municipal bond ratings scale	Municipality/outside financing/persistent	\$100K spending increases employment by 5.10 (0.58 [0.21, 0.94] government and 4.52 [1.97, 7.07] private) job-years; income multiplier of 1.9. Effects are larger when slack is higher.
Brückner and Tuladhar (2014), *Economic Journal*	System GMM on annual Japanese prefecture spending data controlling for lagged output and prefecture fixed effects	Prefrecture/mixed financing/transitory	Public investment multiplier of 0.93 [0.63, 1.23], local government expenditure multiplier of 0.78 [0.45, 1.11]
Bucheim and Watzinger (2017), *Unpublished*	German stimulus targeted to improving energy efficiency of school buildings	County/outside financing/transitory	€100K increases employment by 4.0 [0.2, 7.8] job-years
Clemens and Miran (2012), *AEJ Policy*	State balanced budget rules	State/local financing/transitory	"On-impact" multiplier of 0.29 [−0.22, 0.79]
Cohen, Coval, and Malloy (2011), *Journal of Political Economy*	Changes in congressional committee chairmanships instrument state-level federal expenditures	State/outside financing/throughout chairman term	1 percent increase in annual earmarks causes 0.8 [0.6, 1] percent reduction in the representative firm's capital expenditures. Crowding out smaller when slack is higher.
Corbi, Papaioannou, and Surico (2017), *Unpublished*	Population-based discontinuity in federal transfers in Brazil	Municipality/outside financing/transitory	Output multiplier of two
Fishback and Kachanovskaya (2015), *Journal of Economic History*	Shift-share instrument—sensitivity to changes in federal spending	State/outside financing/transitory	Multiplier of 0.96 [0.31, 1.61] when transfer payments are excluded and 0.83 [0.39, 1.27] when transfers are included
Hausman (2016), *American Economic Review*	1936 veteran's bonus	State and city/outside financing/one-time	An additional veteran in a state associated with 0.3 [0.20, 0.41] more new cars sold; An additional veteran in a city associated with \$200 [\$73, \$327] more residential building
Leduc and Wilson (2012), *NBER Macroannual*	Panel local projection on revision to present value of federal highway transfer funds	State/mixed financing/present value	Impact multiplier of 1.4. Cumulative multiplier of 6.6
Nakamura and Steinsson (2014), *American Economic Review*	Regional variation in military buildups	State and region/outside financing/transitory	State GDP multiplier of 1.43 [0.84, 2.02]; region GDP multiplier of 1.85 [0.90, 2.80]; state employment multiplier per percent of GDP of 1.28 [0.80, 1.76]. GDP multiplier is larger when slack is higher.
Porcelli and Trezzi (2016), *Unpublished*	Allocation of reconstruction grants to municipalities following the 2009 "Aquilano" earthquake	Municipality/outside financing/one-time	One year "grants multiplier" of 0.15 [0.05, 0.25] and of 0.36 [0.21, 0.52] when earthquake damages are instrumented
Shoag (2016), *Unpublished*	Windfall component of returns on state's defined-benefit pension plans	State/outside financing/transitory	Income multiplier of 2.1; \$100K spending increases employment by 2.89 [1.25, 4.54] job-years. Effects are larger when slack is higher.
Suárez, Serrato, and Wingender (2016), *Unpublished*	Federal spending due to errors in local population estimates	County outside financing/permanent	Local income multiplier of 1.7–2; \$100K spending increases employment by 3.25 [0.35, 6.15] job-years

1.7–2 and a cost-per-job of roughly \$31K. Notably, while measurement error offers an appealing source of exogenous variation in spending changes, the persistence of these transfers is quite high since future federal funds are also higher as a result of an upward revision to the population estimate.

Nakamura and Steinsson (2014) adapts the time series approach of measuring the response of output to increases in federal purchases associated with defense buildups (Barro 1981; Ramey and Shapiro 1998; and Hall 2009) to a cross-sectional setting. In particular, when defense purchases rise, they rise by more in states with larger concentrations of defense contractors. Nakamura and Steinsson (2014) implements a version of equation (1) where the endogenous variable $F_{s,t}$ consists of federal defense purchases in state *s* in year *t*, and the instruments are state-specific loadings on the growth of national defense purchases. Their identifying assumption then becomes that the federal government does not engage in a defense buildup because of economic weakness concentrated in regions more heavily dependent on defense contracting. They estimate a state output multiplier of roughly 1.4 and a multiplier of 1.9 when expanding the geographic unit to the region level. The persistence of the purchases is similar to the persistence of a defense buildup, that is, higher than in a one-time stimulus bill, but lower than a population update.

Two studies use historical variation from spending during the 1930s. Fishback and Kachanovskaya (2015) examines New Deal spending and transfers using a state-year panel and a shift-share instrument for spending in a state. They find income multipliers of close to but below one. Hausman (2016) uses variation in the geographic distribution of World War I veterans interacted with the large, one-time Veteran's bonus payment in 1936. While lacking an overall measure of private spending, he finds substantial increases in auto purchases and new building in states and cities with more veterans.

Adelino, Cunha, and Ferreira (2017) exploits a change in borrowing costs resulting from a recalibration of municipal bond ratings by Moody's. They find a local income multiplier of 1.9 at the county level and a cost-per-job of \$20K. While the recalibration implies a persistent lowering of borrowing costs, the magnitude of the decline in interest payments appears too small for a response of private consumption to the relaxation of the county's budget constraint to explain the large employment effects.

Leduc and Wilson (2012) studies the response of state output to innovations in the present value of federal highway grants. They find large output multipliers, but with the caveat they cannot precisely estimate the response of state spending to the federal grants. Using their most conservative results, they find an impact response of \$1.40 of state GDP to an increase in present value of spending of \$1 and a cumulative multiplier of 6.6. The persistence of the output response suggests part of the cumulative multiplier reflects higher productivity from the capital improvements in addition to any short-run demand effects.

A few studies have used data from outside the United States. Acconcia, Corsetti, and Simonelli (2014) exploits the introduction of an anti-corruption law in Italy that resulted in the dismissal of city councils and their replacement by external commissioners who reduced public expenditure. They estimate an output multiplier of 1.6 to 2.0, where the higher number includes lagged government spending effects. Because the central government finances most local expenditure, these estimates correspond to outside-financed multipliers despite the determination of spending at the local level. Brückner and Tuladhar (2014) uses a system GMM estimator to study variation in annual spending across prefectures in Japan in the 1990s. Effectively, identification comes from a timing assumption, similar to that in Blanchard and Perotti (2002), that fiscal policy not have a forward-looking component. They find multipliers below but close to one. Interestingly, they find larger multipliers for locally financed than for centrally financed public investment. Porcelli and Trezzi (2016) exploits discontinuities in the provision of reconstruction grants to municipalities following the 2009 L'Aquila earthquake in Italy. While their "grants multiplier" of 0.3 is lower than most other studies, if one assumes municipalities would have engaged in the same rebuilding effort with or without the grants, then this 0.3 estimate corresponds more directly to a pure windfall transfer multiplier and, as such, is only slightly larger than the calibrated estimates discussed in Section IIA. Corbi, Papaioannou, and Surico (2014) exploits several discontinuities in the formula mapping local population to transfers from the federal government in Brazil. They estimate a cost-per-job-year of roughly \$8,000, with three-quarters of the additional employment in the private sector. Using the approach developed in Section III, this magnitude translates into an output multiplier of roughly two.

Finally, two important studies find much smaller or even negative effects of local spending. Clemens and Miran (2012) uses variation in the strictness of state balanced budget requirements and finds a spending multiplier with a point estimate close to 0 and an upper bound of 0.8. They interpret the smaller estimated multiplier as reflecting the absence of a windfall transfer since, while a laxer balanced budget requirement allows a state to run a temporarily larger deficit, it does not affect the local region's intertemporal budget constraint. Even so, the transfer component of the other studies reviewed appears by itself too small to explain the difference, suggesting other econometric or institutional factors may also matter. Cohen, Coval, and Malloy (2011) exploits the increase in federal spending in a state when a member of the state's Congressional delegation becomes the chair of an important committee. They estimate statistically significant *negative* effects of spending on investment, employment, and sales at publicly traded firms headquartered in the state. Cohen, Coval, and Malloy (2011) interprets their results as reflecting a wealth effect from the windfall transfer. However, they also report negative, albeit imprecisely estimated, effects on overall state output, which would require more than just a labor supply response to justify.

VI. What We've Learned

Informativeness for National Multiplier.—Cross-sectional multipliers can be large. A cross-study average necessarily ignores aspects such as persistence of spending and regional openness, which differ across studies and likely affect the estimated multiplier. Nonetheless, using the approach developed in Section III to translate employment multipliers into output multipliers and aggregating over all studies described in Tables 2 and 3 for which I could calculate an output multiplier, the mean output multiplier is 2.1 and the median is 1.9.[31] This magnitude closely matches the updated estimates based on the ARRA in Section IV. Removing the two studies (Suárez, Serrato, and Wingender 2016; and Adelino, Cunha, and Ferreira, 2017) that measure responses to persistent increases in spending, the mean (median) multiplier is 1.8 (1.9). Restricting to studies already published in peer-reviewed journals as a crude quality filter gives a mean (median) multiplier of 1.6 (1.6).

According to the theory reviewed in Section II, a deficit-financed, cross-sectional multiplier provides a lower bound for the closed economy, deficit-financed, no-monetary-policy-response multiplier. Accounting for the outside financing of spending in many of the studies might reduce the lower bound by about 0.1. Thus, using the mean estimate of 1.8 for the studies based on transitory spending, the cross-sectional evidence suggests a closed economy, deficit-financed, no-monetary-policy-response multiplier of about 1.7 or above.

Is a national multiplier of 1.7 large? In a recent review article, Ramey (2011a) concludes that the multiplier for a deficit-financed increase in government purchases similar to the ARRA, that is, the multiplier for a temporary, deficit-financed increase in spending when monetary policy is constrained, "is probably between 0.8 and 1.5. Reasonable people can argue, however, that the data do not reject 0.5 or 2.0." If this range serves as a prior, then the evidence from cross-sectional multiplier studies ought to move posteriors toward the upper end of the range.

Two factors may explain the larger multiplier implied by cross-sectional studies than that based on time-series evidence. First, most cross-sectional studies explicitly identify quasi-experimental variation in spending. These studies may therefore use cleaner variation than is available in the time series. Second, a "lean against the wind" monetary policy dampens the national multiplier, and it may be difficult to extract the no-monetary-policy-response multiplier from time-series studies that span diverse monetary policy regimes. In this sense, cross-sectional variation may offer a better laboratory for studying what happens when monetary policy does not react.[32]

State-Dependence.—Many of the studies also shed light on an important debate on whether and why multipliers may be state dependent. Here again, the time-series literature has not reached a consensus (Auerbach and Gorodnichenko 2013; Ramey and Zubairy 2018). Cohen, Coval, and Malloy (2011); Shoag (2015); Nakamura and Steinsson (2014); Dube, Kaplan, and Zipperer (2014); and Adelino, Cunha, and Ferreira (2017) all test for and find evidence of higher multipliers or less crowd-out in regions and periods with more unused resources. Because the cross-sectional studies hold the response of monetary policy fixed, less responsive monetary policy in slack periods, as emphasized in Christiano, Eichenbaum, and Rebelo (2011)

[31] Providing a confidence band for the cross-study mean or median is complicated by the possibility of correlation across studies, especially for papers studying the ARRA. The studies reviewed in Tables 2 and 3 and excluded from the cross-study mean and median are Cohen, Coval, and Malloy (2011); Hausman (2016); Leduc and Wilson (2012); Porcelli and Trezzi (2016); and Bucheim and Watzinger (2017).

[32] One recent study of national multipliers in Japan, which explicitly distinguishes zero lower bound episodes, indeed finds higher multipliers in such periods, with a magnitude in line with the cross-sectional evidence (Miyamoto, Nguyen, and Sergeyev 2017).

and Woodford (2011), cannot explain the findings of state-dependent multipliers in these studies. Instead, other forces related to slack such as lower factor prices or less congested labor markets appear also to matter, as in the model of Michaillat (2014).

ARRA Revisited.—Around the time of its passage and implementation, the economic effects of the ARRA generated much debate. What should we conclude in light of the accumulated evidence reviewed above? According to Council of Economic Advisers (2013), \$263 billion of ARRA outlays through the end of 2010 consisted of direct government spending or spending intermediated by state governments, or about 0.9 percent of GDP per year. Applying the rough lower bound result and assuming completely unresponsive monetary policy during 2009 and 2010, an output multiplier of 1.7 and a cost per job of \$50,000 imply average output would have been lower by at least 1.5 percent of GDP, and average employment lower by 2.63 million jobs during 2009 and 2010, absent these components of the ARRA. Two important caveats to these calculations require mention. First, they ignore the impact of the tax and personal transfer components of the ARRA.[33] Second, if the ARRA caused the Federal Reserve to use its unconventional monetary tools less aggressively than it would have otherwise, or if the ARRA changed expectations about future monetary policy (such as estimates of when the Federal Reserve would begin to raise the federal funds rate), then the cross-sectional evidence overstates the actual impact of the ARRA because the no-monetary-policy-response condition does not hold. Indeed, Swanson and Williams (2014) use bond yield reactions to economic news to argue that one and two year interest rates remained sensitive to economic conditions during 2009–2010, implying a smaller national multiplier.

Other Shocks.—Cross-sectional multipliers inform the effects of a broader set of shocks than just national counter-cyclical stimulus. For example, high and uneven unemployment in the euro area has renewed interest in further fiscal integration. How effective as counter-cyclical stimulus would be spending by the European Union in targeted regions with high-cyclical unemployment? Cross-sectional multiplier studies provide a direct and generally optimistic answer to this question.

The evaluation of place-based policies offers another example. Similar to many of the cross-sectional studies, place-based policies direct federal resources toward particular geographic areas.[34] On the other hand, place-based policies typically combine grants for spending with targeted hiring incentives and other business tax breaks, involve very persistent interventions, and apply to very small geographic areas. Relative to cross-sectional multiplier studies, the small geographic concentration reduces the effects of transfers into the region on local output, but the longer persistence means the transfers are larger. The persistence has also led the place-based literature to analyze spatial equilibrium models that allow for a migration response, an aspect ignored in the theoretical treatments of cross-sectional multipliers, but at

[33] These components have received less empirical attention, perhaps because the individual nature of receipt determination makes a geographical research design more difficult. Sahm, Shapiro, and Slemrod (2012) uses survey responses to find that one major tax element of the bill, the Making Work Pay credit, had a small effect on spending.

[34] Empowerment Zones are the most well known. See Glaeser and Gottlieb (2008) and Neumark and Simpson (2015) for recent surveys of place-based policies.

the expense of abstracting from short-run demand effects. These differences aside, the evidence from cross-sectional multiplier studies appears more optimistic of the scope for positive local effects than are many studies of placed-based policies. Both literatures would benefit from greater integration.

Last, both the theory and empirics of cross-sectional studies may provide guidance for the aggregate effects of other local demand shocks. The study of such shocks has also proliferated in recent years, with Autor, Dorn, Hanson (2013), and Mian and Sufi (2014) two prominent examples.

VII. Directions for Future Research

While much progress has occurred, there remains scope for further integration of empirical and theoretical investigations of cross-sectional multipliers. One aspect concerns empirical studies of natural experiments in which spending rises without a concomitant increase in the local tax burden. These studies should quantify the magnitude of the outside transfer or windfall. A useful summary metric is the ratio of the annuity value of the transfer to the contemporaneous increase in government spending. These studies should also discuss the salience of the windfall component. Did private agents plausibly understand that they had received a transfer of resources or a windfall? On the theory side, the rough lower bound result depends on the small difference between outside-financed and locally financed multipliers in modern macroeconomic models. Future research should explore and try to quantify other mechanisms which might amplify this difference.

Another critical area for future research concerns the differences between deficit-financed local multipliers and national multipliers when monetary policy does not respond. While the theory reviewed in Section II gives a lower bound result, it does not provide quantitative guidance on how large the difference might be. Further empirical analysis of the individual channels that give rise to cross-region spillovers could help. For example, we have little evidence of how relative prices change in response to local government spending shocks. While regional price data in the United States is haphazard, studies of euro area members, each of which collects its own CPI, may prove more fruitful.

Finally, while the dependence of "the" government purchases multiplier on other variables, such as the monetary policy response, is widely recognized, empirical studies have paid less attention to heterogeneity stemming from what the government actually purchases.[35] This aspect may matter even more in cross-sectional studies, where the source of variation is the quasi-randomness of the allocation of a particular government program; even if the estimation uses instrumental variables with total spending as the endogenous variable, the LATE theorem (Imbens and Angrist 1994) means that the estimated multiplier depends on the source of variation. On the other hand, budgetary fungibility would negate such differences. Where relevant, future studies should highlight the source of transfers or increase in pur-

[35] Boehm (2015) is a recent exception.

chases both to better compare themselves to the literature and to facilitate future research into the effects of different types of policies.

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