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ABSTRACT

This paper provides new estimates of local job multipliers, the ratio of total jobs generated to some initial number of jobs created from a demand shock. Multipliers greatly affect benefits versus costs of local job-creation policies. These new estimates rely on improved methodology and data. The methodology better captures dynamic effects of demand shocks, specifies the model so that demand shocks are more comparable, and is more general in the types of demand shocks that are considered. The data has more industry detail than that used in previous studies. The local job multipliers estimated tend to be about one-quarter lower than typically estimated local multipliers, closer to 1.5 than to 2.0. In addition, demand shocks to all industries matter, not just to tradable industries. Multipliers are similar across different types of geographic areas, with county multipliers being only one-quarter below commuting zone multipliers and state multipliers only one-quarter above commuting zone multipliers. Multipliers are not larger for larger commuting zones, but they increase in commuting zones that have lower initial employment to population ratios. Multipliers are higher for high-tech industries, particularly in commuting zones with a larger initial high-tech share. In such high-tech local economies, high-tech multipliers may be close to 3. While our high-tech multipliers are greater than for other industries, our estimated high-tech multipliers are less than in some prior studies.

JEL Classification Codes: R11, R23

Key Words: Multipliers; agglomeration economies; congestion effects; high-technology industries

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Local job multipliers—the ratio between total local jobs created and the initial jobs created due to a local labor demand shock—are important in evaluating local economic development policies. If local economic developers use incentives to attract a new plant, benefits depend on the how many total jobs are created locally. Will each additional job in the new plant lead to 0.5, 1.5, or 5.0 added jobs in other firms (e.g., a total local jobs multiplier of 1.5, 2.5, or 6.0)? More local job creation implies more local benefits; therefore, larger multipliers imply a larger benefit-cost ratio. In a simple model, the ratio of gross benefits to costs of an incentive policy would have an elasticity of 1 with respect to the multiplier; if the job multiplier doubles, so will the incentive policy's ratio of gross benefits to costs.[1]

Local job multipliers also provide indirect evidence of agglomeration economies, which are increases in local business productivity associated with either greater size of a local economy or larger local clusters of some industries.[2] Furthermore, multipliers provide indirect evidence of urban congestion effects, for example, higher land prices and higher nominal wages associated with the greater size of a local economy or industry cluster. If a demand shock to local jobs increases agglomeration economies, this increased productivity may increase local job multipliers by attracting new business activity. If a demand shock to a local economy's jobs (and the consequent population growth) increases congestion effects, however, this increased congestion may lower local job multipliers. Therefore, the local job multiplier will in part reflect the net effects of agglomeration and congestion. If the local job multiplier shows a changing

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[1] More complex models likely yield larger effects of local job multipliers. Because higher local job multipliers lead to positive fiscal feedbacks that add net benefits, such interaction effects are likely to further boost gross benefits. For example, in the model developed in Bartik (2018), the baseline model assumptions yield a gross benefit-cost ratio for incentives of 0.44 at a multiplier of 1.5, 1.22 at a multiplier of 2.5, and 3.98 at a multiplier of 6. The benefit-cost ratio at a multiplier of 6 is 9 times the benefit-cost ratio at a multiplier of 1.5, not 4 times.

[2] We discuss why such productivity effects might occur later in the paper.

pattern with a city's size or industry clusters, this is indirect evidence about the varying magnitude of net agglomeration and congestion effects.

Previous studies have estimated local job multipliers, using varied estimation approaches. As we will review, different estimation approaches include input-output models, more general structural regional econometric models, matching models, and reduced-form econometric models. These varied estimation approaches frequently yield multipliers at the state level from 2.5 to 4.0, at the local labor market level from 1.9 to 2.6, and at the county level of 1.6 and above. Multipliers for particular industries have sometimes been estimated to be much higher. For example, Moretti (2010) estimated a local job multiplier of 5.9 for high-tech manufacturing at the metropolitan area level.

In this paper, we provide new estimates of local job multipliers.[3] Our new estimates have three advantages: better data, better econometric techniques, and consideration of more factors affecting multipliers. Our data on industry employment, which include information on 6-digit NAICS industries at the county level, are more detailed. Our reduced-form econometric model is a dynamic model that explicitly estimates short-run versus long-run multipliers, thereby avoiding possible biases due to summarizing growth over a lengthy period. We consider multipliers over a wider variety of geographic areas, from counties to different definitions of local labor markets to states. We also consider how multipliers vary with local area characteristics, the type of industry that is shocked, and their interaction. As we will explore, average local job multipliers are sensitive to the areas considered and the industries shocked.

[3] As will be clear, the particular variant of local job multiplier explored in this paper, and in the reviewed literature, is the effect on total local jobs of a demand shock to local jobs. In the regional economics literature, there are other multipliers, for example, "income multipliers" that reflect the change in local incomes with respect to either a demand shock to local income or local output. There also are some "local job multipliers" that show the change in total local jobs for a demand shock to local output. This paper does not provide estimates for these other definitions of local multipliers or of local job multipliers. See Stevens and Lahr (1988) and Coughlin and Mandelbaum (1991) for discussions of these other types of multipliers.

Our estimated local job multipliers, compared to prior estimates, are lower. At the local labor market level, our estimated long-run average local job multipliers range from 1.3 to 1.8. State level multipliers are higher, at 1.9 to 2.0, and our county-level multipliers are lower, at 0.9 to 1.2.

Local job multipliers are surprisingly local. State multipliers tend to be at most one-quarter higher than local labor market multipliers. Compared to local labor market multipliers, county multipliers are one-quarter to one-third lower. Job multipliers vary surprisingly little with a geographic area's size. As size changes, the various forces, such as agglomeration economies and congestion effects, which make for larger or smaller multipliers, apparently are roughly offsetting on average. Local job multipliers increase in areas with a lower prime-age employment rate. Even though this increase is modest, it is another factor that raises benefit-cost ratios for labor demand policies in areas with excess labor supply.

Local job multipliers are significantly higher for some categories of high-tech industries. However, average high-tech multipliers are still moderate, at perhaps 1.9 to 2.0 for local labor markets. Multipliers as large as 5.9 are not supported in these estimates. High-tech multipliers are higher (close to 3) in areas that start out with a greater cluster of high-tech industry, but they appear to be subject to some threshold effects, beyond which advantages of a more concentrated cluster diminish. They increase rapidly when we consider areas with a slightly above-average high-tech cluster, and then they level off. Areas significantly below the average industry concentration of high-tech industries have modest high-tech multipliers.

THEORY

Four factors determine local job multipliers: 1) indirect effects on local supplier industries, 2) induced effects of higher local worker earnings on demand for locally produced goods and services, 3) agglomeration economy effects, and 4) congestion effects. All of these start with the direct effects of a labor demand shock on a local industry. Due to either national factors (increase in national demand for the output of some industry) or local factors (the local area becomes more attractive for that industry, for example, due to higher local incentives), the local industry has some direct increases in output and local jobs.

The indirect effects are due to the directly shocked industry increasing purchases from local supplier industries. The magnitude of such effects depends on whether this industry extensively uses suppliers in the production process and whether it chooses to use local suppliers. The increase in jobs in local supplier industries may in turn lead to increased demand for second-tier local suppliers to the first-tier local suppliers and so on.

The induced effects are due to the increased earnings (from higher employment or higher wages) of workers in local industries, both those directly affected by the demand shock and the various tiers of local suppliers. Some of these increased earnings of local workers may be spent on locally sold goods and services, which in turn may involve some local production or distribution jobs. These induced effects depend upon how shocks to jobs and output in these industries affect the earnings of local workers, how these workers choose to spend their increased earnings, and how local businesses selling to local workers choose to produce and distribute their products.

Agglomeration economy effects are productivity spillovers resulting from either a larger local economy or a larger local concentration of some industry or industry cluster.

Agglomeration economies associated with the overall size of the local economy are called "urbanization economies," and economies associated with industry concentration are "localization economies." Agglomeration economies are thought to be mainly due to two factors: thick market externalities and knowledge or skill spillovers. A larger city or industry cluster may create larger markets ("thick markets") for certain specialized supplies and skills, leading to greater availability or better matching of those supplies and skills, thereby enhancing productivity. Larger cities or industry clusters may lead to firms being better able to steal good ideas and workers from one another, thereby enhancing productivity.

The existence of agglomeration economies is not in serious doubt; otherwise, how can one explain the survival of Silicon Valley, despite its high costs? However, whether agglomeration economies lead to higher job multipliers remains a question. For this to occur, the expansion of overall city or industrial cluster size must lead to a further marginal increase in agglomeration economies. Furthermore, the resulting higher productivity must lead to output expansions sufficient to increase jobs.

One can imagine that agglomeration economies might be subject at some point to some threshold effects and diminishing returns. That is, the productivity and job effects of increasing the size of a city or industry cluster may be large up to a particular size, but then there may be some diminishing returns to further increases in the size of the city or industry cluster. However, there is little direct evidence of such diminishing returns.

Congestion effects occur because an increase in the size of a city or industry cluster will lead to some increased costs. Land prices, wages, and other local prices may increase. In addition, there may be some reduction in available land sites or the availability of non-employed labor; that is, local prices and wages may not completely describe the local availability of factors of production.[4] Additional costs may also arise from negative externalities, such as increased pollution or increased traffic congestion, which may make the local economy less attractive to businesses directly by lowering productivity and indirectly by making it more costly to attract workers to the local area. Increased costs may even occur in the long run, with more complete migration responses of labor; even in the long-run, local land is inherently in limited supply, and negative externalities may persist. Industry clusters may also increase land prices in the long run, for example, one can imagine that wealthier high-tech workers may demand more land for housing than the average worker.

Higher costs cause negative feedback effects on the local job multiplier. Higher costs decrease location and expansion decisions of export-based or tradable industries that sell to national markets because higher costs decrease local competitiveness. Higher costs may also hurt jobs in industries that sell locally, as local workers and businesses may substitute non-local goods in response to higher prices of locally produced goods and services.

These four factors lead to multipliers that vary over time. In the short run, there will be both some direct effects of a job shock and indirect and induced effects due to increased demand for local supplier industries and retail industries. These effects may increase somewhat in the medium run as firms respond to increased demand for their products by expanding or setting up new businesses. But in the longer term, increased costs due to congestion effects will become more important, reducing the local jobs multiplier. If agglomeration economies increase due to some marginal jobs increase, the agglomeration effects may offset any congestion effects. The prediction is that local jobs multipliers will increase somewhat over time before diminishing to

[4] Beaudry, Green, and Sand (2018) provide a theoretical and empirical model in which both local wages and local employment rates have independent effects in determining local labor demand.

some long-run equilibrium levels. The adjustment to a long-run equilibrium due to increased costs may take some time because business activities do not instantly adjust to cost changes.[5]

General equilibrium considerations may also govern how the local job multiplier is observed to vary with overall city size. If the net local job multiplier, allowing for all of these four factors, varies drastically with city size, it seems plausible that the existing city size distribution will not be an equilibrium. For example, if cities of different sizes were subject to random direct labor demand shocks, a disparate distribution of the jobs multiplier with city size would lead to the city size distribution changing over time. The city size distribution may change over time until for different size classes of cities, with different circumstances, the negative effects of congestion effects versus the positive effects of other multiplier determinants lead to similar local job multipliers across cities of different sizes.[6]

REVIEW OF PRIOR RESEARCH LITERATURE

Local job multipliers have been estimated with four approaches: 1) input-output models, 2) more general regional structural econometric models, 3) reduced-form matching models, and 4) reduced-form general demand-shock econometric models. The first two approaches are different types of structural models of regional economies that include equations that are estimated. In these models, however, the local job multiplier is not directly estimated. It is inferred from a combination of the model's structure and the structural equations' estimates.

[5] For example, in regional studies, it is common to see that the coefficient on lagged employment, in a model of current employment, is close to 1, which implies considerably lagged adjustment to the long-run equilibrium (e.g., the classic article by Helms [1985]; more recent similar results are in Bartik [2017]).

[6] The four determinants of local job multipliers may lead to multipliers varying with many factors other than city size, for example, with restrictions on housing supply (Saiz 2010), which may be greater in coastal regions (Osei and Winters 2018). We hope to explore how multipliers vary with many other city characteristics in future work.

Reduced-form approaches seek to directly estimate the effects of demand shocks to local jobs on other local jobs. Matching models consider a specific demand shock in a specific local area; general demand-shock models consider a wider variety of areas and demand shocks.

Of the four determinants of local job multipliers, input-output models only seek to estimate indirect and induced effects on business and worker local purchases. Input-output models do not seek to estimate the price responses represented by congestion effects. They also do not estimate the effects of agglomeration economies. More general regional structural models generally incorporate some price effects and may incorporate agglomeration economies. Reduced-form approaches (both matching and more general models) reflect all four determinants of local job multipliers, without trying to separately identify these determinants.

Input-Output Models

Input-output models can estimate local job multipliers by tracing how one industry's local expansion leads to changes in business and worker purchases from other industries.[7] Regional input-output models typically do not have direct evidence on regional purchasing patterns. Instead, regional input-output models start with national data on the pattern of business and worker purchases by industries and combine this national data with assumptions or estimates of the percentage of these industry purchases that are local, which are called "regional purchase coefficients." These input-output models are Keynesian-style quantity models that ignore any responses to relative prices or other variables. Purchase patterns are implicitly assumed to be fixed, even if prices change.

[7] See Miller and Blair (2009) for a review.

Prominent regional input-output models with multiplier estimates include the RIMS II model of the U.S. Bureau of Economic Analysis (BEA) and private consulting models from IMPLAN and EMSI. Regional input-output models often result in sizable job multipliers, at least for manufacturing industries and other frequent targets of state and local economic development programs. For example, in one recent study of a particular incentive program in Michigan, using RIMS II multipliers, the average state-level multiplier of projects provided with state incentives was 3.7 (Bartik et al. forthcoming). If other states had incented a similar job mix as in Michigan, the estimated job multiplier would be 3.8 in Ohio, 3.2 in Texas and North Carolina, and 2.8 in Minnesota. Using a state-level version of the IMPLAN model, the Foxconn plant in Southeast Wisconsin was estimated to have a job multiplier of 2.7 for Wisconsin (Ernst and Young 2017). A study of Foxconn that focused on the local impacts of Foxconn, within a 100-mile radius, and adjusted downwards for spillovers into Illinois, found a local multiplier between 1.9 and 2.4 using the EMSI model (Baker Tilly 2017). A study of the recent Amazon location decision in Arlington in Northern Virginia, which adjusted for spillovers into Washington, DC and Maryland, used the RIMS II model to find a Virginia multiplier of 1.8 to 1.9 (Fuller and Chapman 2018). Therefore, it is not uncommon for regional input-output models to find job multipliers of 2.5 to 4 at the state level and of around 2 when the focus is more local.

Regional input-output model estimates of local job multipliers will likely be biased upwards because they ignore congestion effects. The models ignore local price effects, but increased local prices will reduce the market share of local industries that sell to national markets. Increased local prices will also reduce the regional purchase coefficients of both local businesses and workers. On the other hand, regional input-output model estimates of local job multipliers will likely be biased downwards because they ignore agglomeration economies.

Knowledge and skill spillovers, and thick-market effects, may cause an increase in output and jobs in one tradable-product firm in a local economy to increase the productivity and competitiveness of other local firms.

More General Regional Structural Econometric Models

Either price effects or agglomeration economy effects (or both) can be incorporated into more general structural regional econometric models. Of these models, by far the most prominent current model is the REMI model, which is a generic regional model that can be fine-tuned to apply to a particular state, metropolitan area, or county.[8] In its current form, REMI incorporates price effects both on the competitiveness of tradable industries and on regional purchase coefficients, as well as incorporating some agglomeration economy effects by allowing local labor variety to affect productivity. The REMI model also yields sizable state and local job multipliers. For example, in simulations with the REMI model in Michigan, the average state job multiplier for a job incented by the state's former MEGA program was 3.9 (Bartik and Erickcek 2014). In an analysis of the 2018 Amazon location decision (reversed in 2019) in New York City, the REMI model estimated a state-level job multiplier of 2.7 (REMI 2018). REMI in its current form seems to yield similar multipliers to some of the input-output models, with agglomeration economies appearing to roughly offset congestion effects.

A recent regional econometric model by Bartik (2018) estimates how input-output multipliers are reduced by local price and wage effects. Using estimates of how increases in local prices and wages might lower the competitiveness of traded industries, the model finds that such price feedbacks reduce multipliers by one-quarter to one-third, with the offset being higher for

[8] See the REMI website at www.REMI.com for some documentation on the current structure of the model.

higher input-output multipliers. More specifically, simulations with the model suggest that if the input-output multiplier is 1.5, the net multiplier after allowing for price effects will be 1.13 (25 percent lower); if the input-output multiplier is 2.5, the net multiplier after congestion effects is 1.77 (29 percent lower); and if the input-output multiplier is 6.0, the net multiplier after congestion effects is 4.01 (33 percent lower). The model does not estimate agglomeration economy effects, which might offset some or all these congestion effects.

Reduced-Form Models of Multipliers: Matching Models

One reduced-form approach is based on estimating the effects of a specific demand shock for a specific area. For example, an area may have been known to have had a major plant opening, such as an automobile assembly plant (Adams 2016). Or an area may have been known to have had a large boost to a specific industry, such as fracking-related jobs (Munasib and Rickman 2015). The effects of such a specific demand shock to a specific area are estimated by comparing subsequent employment in the specific area to matched areas. Areas can be matched using propensity score matching (Adams 2016) or by creating synthetic control groups (Munasib and Rickman 2015).

These reduced-form matching models find some hints that local job multipliers may be less than assumed by regional input-output models (Adams 2016; Munasib and Rickman 2015). However, with the demand-shock treatment only occurring in one area or in a few areas and a few industries, standard errors tend to be wide when correctly estimated. That is, it is challenging to pin down the local job multiplier with much precision when using the matching approach.

Reduced-Form Models of Multipliers: Econometric Models for More General Shocks

Many papers have used reduced-form econometric approaches that consider more general demand shocks in a wide variety of local areas. These models estimate how national demand shocks to a wide variety of local industries affect overall local employment across many local areas.

Most of these papers use a "share-effect" instrument that predicts each local area's job growth based on the area's base-period mix of industries and national growth trends. This instrument was popularized by Bartik (1991) and Blanchard and Katz (1992). Bartik (1991) argued that this instrument proxied for changes in national demand for an area's tradable or export-base industries, and he showed that it put the greatest weight on industries whose location quotient varies greatly across different local areas. Within labor economics, this share-effect instrument is commonly used to identify labor demand shocks, for example, in estimating how demand shocks to local employment growth affect local wages or employment rates.[9] But the first stage of this regression implicitly identifies multipliers because the "demand shock" for an area's export-base industries, which this instrument measures, will generally yield an effect that is greater than one for one on an area's overall employment. Four prominent papers that use this first stage to estimate job multipliers are Moretti (2010), van Dijk (2018), Partridge, Rickman, Olfert, and Tan (2017), and Tsvetkova and Partridge (2016).

Moretti (2010) uses the share-effect prediction only to predict demand shocks for tradable industries, which he assumes to be the manufacturing sector. Moretti estimates the effects of demand shocks to tradables on the non-tradable sector and uses this to infer the total employment

[9] For some recent examples, see Beaudry, Green, and Sand (2018) and Amior and Manning (2018).

effect.[10] He uses the demand shock prediction as an instrument for the actual change in manufacturing employment and only reports this final second stage estimate. Moretti's sample is 232 metro areas, and the change in employment is measured for two 10-year time periods, 1980–1990 and 1990–2000. Moretti finds an average multiplier of 2.59 for manufacturing. For high-tech industries, Moretti estimates a multiplier of 5.9.

Because Moretti only reports the final two-stage estimated multiplier, his estimated multiplier only estimates part of the agglomeration economy and congestion effects. The coefficient estimates show the effect of the realized change in manufacturing employment on non-manufacturing employment. Any agglomeration economy or congestion effects on manufacturing employment will already be reflected in this realized change and will not be shown in the final two-stage coefficient.

For example, suppose an area has a national demand shock that would boost its manufacturing employment by 1,000 jobs. Suppose input-output multiplier effects would be expected to boost non-manufacturing by one job for every actual one job change in manufacturing (i.e., the total input-output multiplier is 2.0). Suppose also there are no agglomeration economy effects, but there are congestion effects solely for the manufacturing sector. Higher local land prices and wages due to growth have an adverse effect on

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[10] Moretti relates the change in the natural log of non-tradable jobs to the change in the logarithm of tradable jobs. This functional form is not what is implied if the multiplier is a constant ratio to the initial shock to tradable jobs. For example, if we assumed that on average manufacturing had a multiplier of 2, then the percentage effect of a manufacturing shock on non-manufacturing in Moretti's specification should vary greatly across the sample, depending upon the initial manufacturing share. For example, in an area with half the average overall manufacturing share, the expected effect of a shock to ln(manufacturing) on ln(non-manufacturing) would be expected to be half as great. In other words, his model suggests that the coefficient he is estimating varies greatly across his sample. The multiplier estimates he provides are calculated at the sample mean shares of manufacturing and non-manufacturing. The problem is not that Moretti uses logarithms. The problem is that the dependent variable and the right hand-side variables use logarithms that have quite different relative scales in different areas. This problem would not arise if each variable was expressed as a shock to the logarithm of the same variable, for example, total employment. That is, Moretti could have expressed both the change in manufacturing and non-manufacturing by what these changes did to the overall logarithm of employment. Our approach scales the different variables so that they are relative to the same overall employment base as discussed below.

manufacturing growth. But these higher local costs do not affect the regional purchase coefficient, and hence, do not affect the ratio of the non-manufacturing job change to the manufacturing job change. For manufacturing, however, the higher local costs cause 250 other manufacturing jobs to be destroyed. The net effect on manufacturing jobs is an increase of 750 jobs, and non-manufacturing jobs will also go up by 750 jobs.

In this example, Moretti's methodology will estimate the second-stage relationship between the predicted demand shock of 750 jobs and the non-manufacturing job change of 750 jobs. His methodology will conclude the multiplier is 2.0. Yet, from the viewpoint of an economic developer or policymaker who wants to know the ultimate job effect of bringing in a manufacturing plant with 1,000 jobs, the true multiplier is only 1.5—the job shock of 1,000 jobs will destroy 250 other manufacturing jobs through congestion effects and create 750 non-manufacturing jobs via input-output effects. Due to omitting congestion effects within the manufacturing sector, the two-stage estimation procedure will overstate the relevant multiplier. The bias could go in the other direction, however, if agglomeration effects within the manufacturing sector are important.

Moretti's methodology does incorporate limited congestion and agglomeration economy effects—but only to the extent they affect the ratio of non-manufacturing jobs to manufacturing jobs. For example, if higher local prices reduce the proportion of non-manufacturing goods and services that are purchased locally, Moretti will detect this effect as a lowering of the multiplier compared to what would be predicted by a pure input-output model. Similarly, if agglomeration economies with city size cause the local non-manufacturing sector to become more productive due to this shock, then Moretti will detect this effect as an increase of the multiplier over what would be predicted by a pure input-output model.

This limitation of Moretti's methodology can be avoided by directly estimating the reduced form, that is, directly estimating the effect on total local jobs of the national demand shock to local jobs. In the current example, this would relate the total change in jobs of 1,500 jobs to the national demand shock to local jobs of 1,000. This yields a local job multiplier of 1.5, which is the relevant multiplier for policymakers, reflecting net effects of local demand shocks after considering congestion effects and agglomeration economies. Our paper, as well as most other papers in this general reduced-form literature on multipliers, focus on these reduced-form estimates of national demand shocks to local jobs.

Van Dijk (2018) explicitly tries to improve upon Moretti's estimates, using similar data but somewhat different methodologies. He defines tradable industries more broadly, based on how concentrated an industry's employment is by metropolitan area. In the results he judges to be most accurate, he estimates the effects of shocks to tradable industries upon non-tradable industries, using data on metropolitan areas for three 8-year time periods: 1990–1998, 1998–2006, and 2006–2014.[11] Unlike Moretti, he reports results both for his final two-stage estimate of the multiplier and reduced-form estimates that simply estimate the relationship between the change in non-tradable employment and the predicted change in tradable employment based on the share-effect instrument.[12] The estimated two-stage multiplier is 2.40, and the estimated reduced-form multiplier is 1.88.[13] The former multiplier is more likely to be consistent with

[11] These are his estimates using BLS place of work data (his Table 2).

[12] In addition, van Dijk uses a functional form in which the percentage change in tradable and non-tradable employment is calculated out of total employment, which is more consistent with multiplier theory. We would expect this coefficient to be the same across metro areas with different shares of tradable vs. non-tradable employment.

[13] We omit discussion of van Dijk's results when, instead of using a share-effect instrument, he directly estimates effects of base period industry shares on tradable growth using the "Regress-M method" of Detang-Dessendre, Partridge, and Piguet (2016). This Regress-M method is more likely to include effects of local industry shares that are due to possible correlations of local industry shares with variables affecting local supply shocks, as argued by Goldsmith-Pinkham, Sorkin, and Swift (2018). In any event, the Regress-M estimates in Van Dijk's paper vary widely in a way that is hard to explain.

input-output multipliers. The latter multiplier is more likely to be relevant to policymakers wanting to know the ultimate local jobs impact of a jobs shock brought about by policy, for example, jobs attracted by incentives. In addition, the lower value of the reduced-form multiplier than the second-stage multiplier suggests that, for overall demand shocks, congestion effects outweigh agglomeration economy effects.

The two other papers, Partridge et al. (2017) and Tsvetkova and Partridge (2016), focus on county job multipliers and provide separate estimates for metropolitan counties and non-metropolitan counties. Both papers include a variable measuring the overall demand shock to employment using all industries, and they estimate its effect on overall employment, a type of reduced-form estimate of the average industry multiplier. The Partridge et al. (2017) paper estimates this overall multiplier for metro counties as 2.1 for the 1990–2000 period and 1.6 for the 2000–2010 period. For non-metro counties, the estimated overall demand-shock multiplier is 1.4 for the 1990–2000 period and 0. 9 for 2000–2010.[14] In the paper by Tsvetkova and Partridge (2016), the long-run effects of overall demand shocks,[15] pooling data from 1993–2003 and from 2003–2013, imply a reduced-form local jobs multiplier of 1.9 in metro counites and 1.6 to 1.7 in non-metro counties.[16]

In sum, these reduced-form results also suggest that local jobs multipliers are sizable. From Moretti and van Dijk, metropolitan area job multipliers appear to be in the range from 1.9 to 2.6. From Partridge et al. and Tsvetkova and Partridge, county multipliers appear to be 1.6 to 2.1 for the larger metro counties and 0.9 to 1.7 in the smaller non-metro counties.

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[14] They also see if shocks to exports or imports have different effects to overall shocks and find no significant differences on job multipliers. There are some differential effects of export and import shocks on population responses and employment to population ratios.

[15] They also find some evidence that energy industry shocks have lower multipliers than other types of demand shocks.

[16] This is from the 10-year differences columns in their Table 1.

Limitations of Current Research on the Local Job Multiplier

What should be critiqued in this literature? First, the input-output and regional econometric models ultimately have both the virtues and limitations of structural models. The structural models enable one to simulate how different forces affect local job multipliers. But the structure also imposes assumptions on the data and does not directly estimate local job multipliers independent of those structural assumptions.

The reduced-form models directly estimate local job multipliers. The matching approach to reduced-form models has at least two limitations. First, the estimates generally tend to be imprecise because only a few specific shocks are examined. Second, the focus on specific shocks in specific areas, while it may be useful for a benefit-cost analysis of policies related to that specific shock, may not be broadly generalizable to other areas, let alone other types of shocks.

The current papers that estimate a more general econometric version of reduced-form models also have several limitations. First, these models generally rely on industry data down only to the 3- or 4-digit level, which may bias results. For example, if a favorable industry mix at the 3- or 4-digit level is correlated with a favorable industry mix for more detailed industries within those categories, as we might expect, then the reduced-form prediction variable will understate the true local demand shock and hence overstate the multiplier.

For example, suppose an area has an above-average share of high-tech industries at the 3- and 4-digit levels. We would then expect that the same area would have an above-average share of high-tech industries in more detailed categories within the 3- or 4-digit classification. If high-tech industries tend to do better nationally over some time period, they are likely to do better at both the 3- or 4-digit level and at finer levels of industry detail. The same economic forces making for higher national demand will be working at various levels of industry detail.

Therefore, a larger high-tech demand shock to this area at the 3- or 4-digit level will tend to predict a larger high-tech demand shock at more detailed industrial categories. If the estimation is done at the less detailed 3- or 4-digit level, part of the estimated local demand shock will reflect not input-output effects or agglomeration economies, but rather will be due to this correlation with the national demand shock at the more detailed industrial categories. This estimate therefore suffers from a form of omitted variable bias. When defined at the less detailed 3- or 4-digit level, the demand-shock variable is positively correlated with the demand shock at the more detailed level, which is omitted from the regression. This positive correlation with an omitted variable that has a positive effect on local jobs will bias upwards the estimated multiplier if we use less detailed industrial categories.

Second, these models generally infer long-run multiplier effects from estimated effects over some relatively lengthy time interval, say 8 or 10 years. The models do not directly use dynamic methods to estimate short-run and long-run multipliers. But, in general, it need not be the case that the estimated job multiplier over an 8- or 10-year period will approximate the long-run local job multiplier. Indeed, this estimated job multiplier can lie outside the interval of short-run versus long-run job multipliers.

For example, suppose local job multipliers do, as predicted, tend to increase over time as local suppliers and retailers expand in response to demand, but then decrease due to congestion effects. Suppose specifically that the cumulative multiplier is 1.5 after one year, 2.0 after two years, and then 1.5 after three years. In general, the multiplier using demand shocks and employment changes over a three-year period will depend upon the pattern of employment changes over each of those three years. If employment changes are positively correlated over time, all is well and good—the three-year multiplier will be between the short-run and long-run multipliers. If employment changes have some negative correlations over time, however, then we can easily get some strange results.

A case where the three-year interval "works" would be if the local demand shock to jobs was 100 jobs in the first year, 100 in the second year, and 100 in the third year. The total demand shock over a three-year interval is 300 jobs. The cumulative multiplier effect over three years will add up to 150 + 200 + 150 = 500. The estimated multiplier will be 500/300=1.67, which is between the short-run and long-run multipliers.[17]

But suppose the local demand shock to jobs was 200 in year one, a job loss of 200 (i.e., –200) in year 2, and a job gain of 200 in year 3. Then the total demand shock over three years would be 200. The cumulative multiplier effect over three years would add up to 300 – 400 + 300 = 200. The estimated multiplier would be 1.0, which is not between 1.5 and 2.0.

Alternatively, suppose the local demand shock to jobs was –100 in year 1, 300 in year 2, and –100 in year 3. The cumulative demand shock after three years would be 100 jobs. The cumulative multiplier effect over three years is −150 + 600 – 150 = 300 jobs. The estimated multiplier would be 3.0, also outside the range of short-run and long-run multipliers.

Lest it be thought that this is a theoretical oddity, it is in fact common in U.S. data to have local areas that do better than the nation as a whole sometimes and worse other times. Manufacturing areas have traditionally had periods of boom and bust, as have energy-industry-dependent areas. Bartik (1993) found a case where this same type of bias clearly occurred in

[17] If we extend the time period to 10 years and imagine a steady 10-year pattern of 100 jobs per year, then the estimated multiplier using a 10-period sample would be closer to the long-run multiplier. The direct job effects over 10 years would be 1,000 jobs. The multiplier jobs would be 150 + 200 + 1,200 = 1,550. The estimated multiplier would be 1.55. This is a general result. If differential local shocks are consistently positively correlated over time, estimated multipliers over longer time periods will increasingly resemble long-run multipliers.

estimating effects of job growth on unemployment rates, due to changes in the relative fortunes of oil-tied cities such as Dallas and Houston.[18]

As we will explore in the next section, when estimating "average" local job multipliers, we need to be cognizant that how the sample is chosen and weighted may change the weighting of different industries. In general, the "average multiplier" across different regression samples or different weightings is not in fact estimating the same average multiplier.

Another important point is that the current local job multiplier research literature does not explicitly address what we see as one of the most important issues, both for policy purposes and for a better understanding of local growth dynamics: what local characteristics affect the size of the local jobs multiplier? Is the local job multiplier truly higher in larger geographic areas, as we would expect, say in states versus local labor markets and local labor markets versus counties? What are the spillovers? Is the local jobs multiplier larger for larger areas of the same type, for example, large states versus small states or larger metropolitan areas versus smaller metropolitan areas? Are multipliers higher, particularly for high tech, in areas that have more pre-existing technology clusters? Are multipliers higher when an area has greater availability of a local supply of labor, as represented by higher rates of unemployment or non-employment? The answers to these questions affect which policymakers would find it in their best interest to most vigorously pursue local job growth. They also shed light on the factors driving local growth, such as agglomeration economies and congestion effects.

Finally, the multiplier of 5.9 that Moretti finds for high-tech calls for independent examination. If high-tech multipliers are so high, this would enormously increase the local

[18] As discussed in Bartik (1993), this critique can be developed more formally as a form of omitted variable bias. The econometrician is using the cumulative change in the demand shock as a right-hand side variable, when really the individual demand shocks within that interval should be included. The problem is that the individual demand shocks in some particular sample might be negatively correlated with the cumulative change.

economic benefits of attracting and growing high-tech industry. As a result, state and local policymakers could rationalize very large incentives or other economic development programs to attract high tech.

MODEL

To provide better estimates of multipliers, we estimate both short-run and long-run effects of national demand shocks to local industries. We use panel data, with observations being on a geographic area/year cell, and a flexibly estimated dynamic model. The current percentage growth in jobs in a local economy from last year to this year is regressed on current and lagged share-effect annual growth shocks, along with controls for national year effects.

Compared to prior research studies, we use more detailed industry data, at the 6-digit NAICS level. We estimate average multipliers for different types of geographic areas, ranging from counties to commuting zones (CZs) to metropolitan areas to U.S. BEA Economic Areas to states. We focus most of our attention on CZs, which are defined for the entire country, and in our view are the most objective approach to using commuting data to define a local labor market.

We estimate how average local job multipliers vary with two key characteristics of a local labor market, specifically its size and its availability of non-employed labor. We also estimate multipliers for various definitions of high-tech industries and how high-tech multipliers vary with the strength of the local area's high-tech cluster.

Choice of Geographic Area

One important preliminary issue is the choice of geographic unit to analyze. From the perspective of regional economics, the most natural unit to examine is a local labor market: a geographic area with sufficient commuting flows that we might expect wages and employment rates for similar types of workers to show similar trends throughout the geographic area. Labor market benefits are the major benefits of economic development policies, which seek to increase local employment rates and real wages rates by boosting local jobs. The boost to local jobs will in part be determined by local job multipliers. Therefore, we want to focus most of our attention on understanding local job multipliers for an appropriate definition of local labor market area.

Metropolitan areas are the most familiar type of local labor market area, but they are not defined for the entire country. Furthermore, metropolitan areas are not defined purely based on objective commuting data. Rather, local political officials can weigh in on whether their area should be made part of a broader metropolitan area. In contrast, CZs, which have been defined by economists from the U.S. Department of Agriculture, include all U.S. counties and are defined based purely on local commuting data.

Alternative local labor market areas are BEA Economic Areas, which are larger than metropolitan areas, and have been defined by the BEA by adding surrounding rural counties to metropolitan areas or micropolitan areas. One BEA criterion is newspaper circulation, which seems problematic, especially under current conditions in the newspaper industry.

States and counties are natural geographic units for which data are available. States are also the political unit that delivers most dollars for economic development incentives. However, neither states nor counties are typically local labor market areas. States are usually bigger than local labor market areas, and counties are usually smaller.

We estimate models for all these geographic units. But because CZs are the most all-encompassing and objectively defined local labor market areas, we emphasize them in our empirical results. We use CZ definitions for the year 2000.

The Shift-Share Model and Regression Weighting

In determining how best to construct our models, we will start by going back to basics and seeing how the share-effect model compares with the original shift-share decomposition of local economic activity. As we will see, this analysis helps clarify what is being estimated when we regress a local area's growth on share-effect instruments and national year effects. This analysis will also show that estimates of local job multipliers depend crucially on how one selects or weights the sample. Prior multiplier studies have differed on whether to use weights.

The shift-share decomposition can be written as

$$(1) \qquad G_{mt} = G_{nt} + \sum_j P_{jm} * \left(G_{jnt} - G_{nt} \right) + \sum_j P_{jm} * \left(G_{jmt} - G_{jnt} \right)$$

where G_{mt} is percentage job growth in area m from year $t-1$ to t, G_{nt} is percentage national growth from year $t-1$ to year t, $\sum_j$ indicates we are summing various weighted growth terms over all industries j, P_{jm} is the proportion of employment in industry j in area m in year $t-1$, G_{jnt} is percentage national growth in industry j from year $t-1$ to year t, and G_{jmt} is percentage growth in area m and industry j from year $t-1$ to year t. This works as an identity only if we are summing over all industries j.

In this decomposition, the first term is the national growth effect, the second term is the share effect, and the third term is the shift effect. Bartik (1991) pointed out the implications that arise because the share-effect term can be rewritten as

$$(2) \qquad \sum_j P_{jm} * \left(G_{jnt} - G_{nt} \right) = \sum_j \left(P_{jm} - P_{jn} \right) * \left(G_{jnt} - G_{nt} \right)$$

where P_{jn} is the proportion of the nation's employment in industry j in year $t-1$. This holds because the sum over all industries of each industry's national growth, when weighted by each industry's national base share, must exactly equal national growth.

The right-hand side expression for the share effect can also be rewritten as

(3) $$\sum_j \left(P_{jm} - P_{jn}\right) * \left(G_{jnt} - G_{nt}\right) = \sum_j \left(P_{jm} - P_{jn}\right) * G_{jnt}$$

Either Equation (2) or Equation (3) clarifies that the share effect weights either each industry's growth rate (Equation [3]) or each industry's differential growth rate (Equation [2]) by the *differential* of each area's share of employment in each industry from the national average share. In other words, all industries' national growth rates are considered, but the industries that are weighted the greatest are those whose shares vary a great deal across areas, which tend to be "export-based" or "tradable" industries.

Rewriting Equation (1) then, we can say that

(4) $$G_{mt} - G_{nt} = \sum_j \left(P_{jm} - P_{jn}\right) * G_{jnt} + \left(Shift\ Effect\right)$$

The intuitive idea of doing a regression to explain local job growth is based on Equation (4). We can look at differentials of an area's growth from the national average and explain this as a function of national growth by industry, weighted by each industry's local differential share from the national average. The local shift effect is omitted from the regression. But we would expect the share-effect variable to affect the local shift effect. The local shift effect will reflect the various multiplier effects: input-output effects, agglomeration economy effects, and congestion effects. If the share effect shows a favorable shock due to local industry shares and national trends, area industry supplier and retail industries will tend to exhibit faster than average national growth; that is, they will exhibit a positive shift effect.

The actual regression that is commonly done, however, is not precisely like that of Equation (4). Instead, we regress local growth on year dummies and the sum over some industries local industry shares times national growth rates, or

(5) $$G_{mt} = \left(Year\ Effect\right) + \sum_{j} P_{jm} * G_{jnt} + \left(error\ term\right)$$

The equation can be estimated without weights (e.g., van Dijk 2018) or with weights, for example, by using base period total employment for each area *m* (Moretti 2010).

As is well known, when including year dummies, the regression coefficients on the remaining variables could be derived by regressing the difference of the dependent variable from year means on the difference of the right-hand side variables from year means. If the regression is weighted, the differences would be from weighted means.

In the present case, if we used as weights the proportion of total employment in each area *m* in year $t-1$, then the dependent variable will end up being the difference of local growth from overall national mean growth, or equal to the left-hand side of Equation (4). The right-hand term will end up with the sum over all industries of the national growth rate in each industry multiplied by the difference between each industry's share in area *m* and that same industry's average share in the nation, or the first term in Equation (4).

If instead, we used an unweighted regression, or if the weights were different from national average shares (e.g., we only included metropolitan areas, which omit parts of the nation or weight non-metro areas at zero), then the differences are being taken from unweighted means in whatever sample of areas we are using. This is a perfectly valid regression because we are relating differences of an area's growth from some national average to the sum over all industries of each industry's national growth rate weighted by the differential of each industry's local share from some national average share.

However, the consequence of using unweighted regressions is that different regressions in different samples will end up having different industry weights. Consider, for example, using unweighted regressions to estimate how this share-effect variable affects overall job growth in a sample that includes all states versus a sample that includes all CZs. Suppose we consider a state and a CZ that each happen to have the same proportion of employment in the base year in each industry. Even though this state and this zone have the same industry mix, the estimated demand shock to growth will be different because the unweighted mean across states of industry shares, and the unweighted mean across CZs, will generally be different. In contrast, if we use base period employment weights for each state or CZ, and if both the state and CZ samples comprise the nation or the same portion of the nation, then if a particular state and a particular CZ have the same industry shares, then they will be estimated to have the same demand shock.

This different weighting of different industry shocks will not be important if all industries have the same multipliers. But if industries have different multipliers, then different weights on different industry growth rates may lead to different estimated multipliers.

Therefore, regressions using a sample of CZs and a sample of states will be using more comparable industry weights if we use weighted regressions than if we use unweighted regressions. If we are trying to determine whether different geographic areas have different multipliers for the same type of demand shock, weighted regressions provide a better comparison.

For regressions comparing geographic areas of different sizes, we can use either weighted or unweighted regressions, but we are more likely to get a fair comparison of multiplier size with pooled regressions rather than separate regressions by geographic size. In pooled regressions, the share effect weights national industry growth rates by the differential of each geographic unit's base period share from the same national industry share mean (weighted or unweighted, depending upon what is being estimated). In contrast, in separate regressions, each size class of geographic unit is weighting its national industry growth rates by the differential of its industry shares from the mean for that size class. If two geographic units in different size classes have the same industry shares, the pooled regression will assign them the same demand shock, whereas the separate regressions may assign them different demand shocks.

Using weights has costs. If the multiplier is similar across geographic units of different sizes, then using differential weights is an inefficient way of estimating the overall average multiplier. In addition, in estimating the effects of local characteristics on multipliers, using weights reduces the effective variation in the sample and increases the imprecision of estimates.

If there are differences in multipliers across geographic units of different sizes, then the choice of whether to use weights is more difficult. Weighted estimates show the average multiplier for the average job in the United States or for the average person. Unweighted estimates show the average multiplier for the average geographic unit in the sample. Both estimates may be of interest.

Therefore, in doing the estimates, we begin by using weights, because our beginning estimates will include comparisons across different types of geographic units. However, we also show unweighted estimates for comparison. We then do pooled estimates but, as will be shown, we find that multipliers do not vary much with the size of a given type of geographic unit. This suggests that unweighted estimates will be more efficient. Therefore, as we continue with the estimation and explore how local characteristics affect multipliers, we emphasize the unweighted estimates, although selected weighted estimates are also reported.

Estimating Equation

The actual estimating equation we use is inspired by Equation (5) but not identical to it. Our basic estimating equation uses the logarithmic growth rate in total area employment from year $t-1$ to year t as a dependent variable. The right-hand side variables include year dummies to control for general national trends affecting job growth, and the variables that will be used to calculate multipliers are current and lagged annual demand shocks to job growth. These annual demand shocks to an area's job growth are predicted demand shocks, based on combining the area's share of jobs in different industries with annual national growth by industry. The demand shocks are specified so as to predict the change in the local logarithm of employment. The cumulative multiplier effect as of a given number of years after a shock is calculated by summing coefficients on these demand shock variables up to that year.

This basic estimating equation can be written as:

$$(6) \qquad \ln\left(E_{mt}\right)-\ln\left(E_{mt-1}\right)=F_t+B\left(L\right)\left\{\ln\left(E_{mt-1}\right)\left(1+D_{mt}\right)-\ln\left(E_{mt-1}\right)\right\}$$

where E_{mt} is employment in geographic area m at year t. F_t indicates a fixed effect for the year, captured by a set of dummy variables for each year. $B(L)$ is a polynomial in the lag operator, indicating that the variable to the right has current and lagged values included, each with its own coefficient. D_{mt} is the share-effect prediction term in Equation (5), or

$$(7) \qquad D_{mt}=\sum_j P_{jmt-1} * G_{jnt}$$

This logarithmic formulation is adopted because we will be calculating cumulative multipliers after a particular number of lags.[19] The sum of effects on the logarithmic change in local employment will exactly add up, whereas raw percentage changes will not. For small values of G_{mt} and D_{mt}, a Taylor series expansion shows that G_{mt} will approximately equal $[\ln(E_{mt}) - \ln(E_{mt-1})]$, and D_{mt} approximately equals $\{\ln[E_{mt-1}(1+D_{mt})] - \ln(E_{mt-1})\}$. Therefore, Equation

[19] The logarithmic formulation done here is consistent with multiplier theory. The actual employment growth that is the dependent variable, and the demand shocks on the right-hand side are each specified so that they reflect what either actually occurred, or would be predicted to occur, to the logarithm of overall employment. This is the logarithmic equivalent to scaling actual employment change and the demand shock by dividing by last period's overall employment. This is consistent with assuming that a given demand shock to employment yields a fixed multiplier effect on overall employment, which is the essence of the job multiplier model.

(6) is a logarithmic version of Equation (5), with lags in the demand shocks also included. The (logarithmic) percentage change in jobs is explained as a function of national year effects, and current and lagged values of the predicted percentage changes in local growth due to local industry shares and national industry growth rates.

This dynamic model allows us to see the cumulative multiplier as of any number of years after a demand shock occurs. Suppose there is some demand shock. The model estimates that the immediate or short-run multiplier is the estimated coefficient on the current demand shock variable on the right-hand side. After one more year, the logarithm of employment has an additional adjustment by the coefficient on the first lag in the demand shock variable. The cumulative multiplier effect on the logarithm of local employment after one more year is the sum of the coefficient on the current demand shock variable and the first lag in the demand shock variable. This is repeated such that each cumulative effect after one more year adds in a coefficient on one more lagged demand shock variable. After the number of lags included in the model, the multiplier does not change because the model assumes that the cumulative multiplier settles down to some equilibrium value after a finite number of lags. That is, the local economy has fully adjusted to the demand shock, reflecting all input-output effects, congestion effects, and agglomeration economy effects.

This dynamic formulation has some advantages over the approaches in the current research literature, which use reduced-form approaches to look at general demand shocks. As mentioned, rather than looking at annual job growth, most of the current studies look at job growth over a much longer time period, frequently 10 years or more. Such a regression equation is equivalent to summing Equation (6) over the length of the time period the study considered. Once one looks at these other studies in this way, it is apparent that a full specification should include all the individual-year demand shocks on the right-hand side of Equation (6). A formulation that includes only one demand shock, the predicted demand shock change over whatever time period is affected, omits the dynamics captured by including the individual-year demand shock variables. As a result, these other studies will suffer from omitted variable bias by omitting the individual-year demand shock terms and using the overall demand shock over some time period as a proxy.[20]

This formulation also focuses on the reduced-form effects of demand shocks, rather than on the second-stage predicted effects of a shock to tradables on other local employment, as in Moretti (2010). As discussed previously, the reduced-form approach to estimating multipliers fully includes all agglomeration economy and congestion effects of the initial demand shock, whereas the instrumental variable/second stage estimation approach omits some of these effects.[21]

This formulation should be considered as a baseline formulation. Variants to the baseline formulation consider adding in terms that allow for multipliers to differ, either for 1) demand shocks to different industries or 2) demand shocks to different areas. However, we begin with the baseline formulation of Equation (6), which focuses in on one set of dynamic multipliers for all demand shocks and all areas, before allowing multipliers to differ.

There are several issues in how to best estimate Equation (6), including:

1) number of lags (i.e., lag length) to use,
2) level of industry detail to use,

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[20] As discussed above, the resulting bias depends upon how short-run and long-run demand shocks are correlated. For an example of bias from summing dynamic equations over longer-run time periods, see Bartik (1993).

[21] Furthermore, if all industries are included in the industry summations used to define the demand shock variables, then it is impossible to do two-stage least squares because the right-hand side variable that we would be instrumenting for would be the same as the dependent variable. This depends upon what industries are included, which we consider below.

3) whether to use all industries or only the most tradable industries, and

4) whether to include area fixed effects.

We explore each of these issues below.[22]

Lag Length

No theory tells us with what lag the multiplier occurs. As mentioned above, we might expect the multiplier to increase at first as local industries respond to increased demand, but then decline as congestion effects appear. How rapidly the economy reaches a final equilibrium multiplier response is unknown.[23]

Therefore, the optimal lag length is chosen by following the data. Various models are tested with one to six lags, and the main criterion for model selection is choosing the model that minimizes the Akaike Information Criterion (AIC).[24] Model selection also rests in part on

[22] Another issue is whether the growth rate used to form the share-effect variable should be national growth or national growth minus the own area. Empirical practice diverges on this issue. We use national growth and justify the choice in Appendix A. In practice, this choice makes little difference, given the high correlation between national growth and national growth minus the own area in a large sample of areas. For example, van Dijk (2018) finds that switching from national growth, to national growth minus own area, only changes the reduced-form multiplier from 2.72 to 2.64.

[23] Based on previous experience with estimating such equations using aggregate data, we anticipated that the number of lags might be around four. For example, see the aggregate data regression reported in Table 4A2.1 in Bartik (1991, p. 283). Similar results are found in other unreported regressions done by Bartik using the share-effect instrument.

[24] The AIC is a standard model selection criterion (see Amemiya 1985 or Greene 2017). Its goal is to minimize the out-of-sample prediction error. AIC has been shown to be asymptotically equivalent to choosing the model that will best explain the remaining observation if models are estimated randomly omitting one observation. In a regression context, the AIC rewards lowering the sum of squared residuals and penalizes adding regressors. As the sample size gets large, the AIC has been shown to asymptotically have a bias towards overfitting the model. An alternative is the Bayesian Information Criterion (BIC), which puts a greater penalty on adding regressors. The BIC aims at estimating the correct coefficient estimates, not minimizing out-of-sample prediction error. The BIC has been shown to be asymptotically consistent in estimating the right model as the sample size gets large. On the whole, in the present context, the AIC seems preferable because we are focused on the model that best predicts total jobs created outside the sample. But we are also more interested in long-run multipliers than in medium-term multipliers, so we also want to look at how the long-run job multiplier changes in different models. This is not quite the same as estimating all coefficients accurately, which is the BIC criteria.

determining whether the chosen model chooses a similar long-run multiplier to a more general model, with a longer lag length.

Much of our discussion focuses on the long-run local jobs multiplier. This is the sum of all the demand-shock coefficients on the right-hand side of Equation (6). [25] It is important to get the lag length right to correctly estimate this long-run multiplier.

Industry Detail

Using the most industry detail available might seem to be the obvious best choice, but the most detailed industry category might include some errors, which might lead to weaker predictions. For example, suppose that within a given 4-digit or 5-digit category, that firms frequently switch their 6-digit industrial classification within the broader industrial category. This could be due to measurement error by the government or possibly because the 6-digit categories do not really represent a meaningful category that defines a relevant product market. Whatever the cause, this problem of switching industrial categories might mean that national growth in a 4-digit or 5-digit category could be a better predictor of growth in a particular 6-digit industry within those categories than is the case for national growth by 6-digit industry.

Therefore, we also examine making the share-effect predictions using industrial categories ranging from the 3-digit level to the 6-digit level. The question is which level of industrial detail minimizes the AIC, which is equivalent in this case to minimizing the sum of squared residuals.

[25] Obviously, standard errors on the sum of these coefficients can be measured using the variance/covariance matrix. But one can speed things up by rearranging the growth terms so that all but the farthest lag is specified in "acceleration" form as differentials of this year's growth shock from last year's growth shock. This rearrangement makes the coefficient estimates correspond to the cumulative job multiplier at each lag.

Using All Industries or Only "Tradable" Industries in Demand Shock Predictions

Studies of the local job multiplier vary in whether they only use "tradable" industries or all industries in their demand shock predictions. For example, Moretti (2010) and van Dijk (2018) focus on tradable industries, whereas Partridge et al. (2017) and Tsvetkova and Partridge (2016) focus on all industries.

Overall local job growth might appear to only be affected by demand shocks to tradable industries, not non-tradable industries. In local economic development policy, a common argument is that bringing in a new manufacturing firm brings in some outside demand, whereas bringing in a new restaurant does not. The new manufacturing plant brings in new dollars from outside, which recirculate within the local economy, and the input-output effects generate additional local jobs. The new restaurant is argued to substitute for other local restaurants, where the new restaurant's sales largely substitute for local sales by other local restaurants. Therefore, any jobs at the new restaurant are likely to be mostly offset by lost jobs at other local restaurants. In contrast, the new manufacturing plant, if it substitutes for sales by other manufacturers, is largely destroying jobs in other areas, so local demand will go up.

This argument is valid for policies to encourage specific firm growth at the local level, but it does not apply to actual demand shocks to jobs and output in a non-tradable industry, which is what is measured by the demand shock variable. For example, suppose that national demand for brew pubs is going up. Brew pub demand might increase due to changes in consumer tastes or changes in brewing technology. If so, then if an area happens to have more brew pubs, then increased national demand for brew pubs would be associated with greater local demand. Consumer tastes and brewing technology are also likely to change in this area. Local consumers are likely to increase their demand for local brew pubs. They will substitute this demand for other goods and services, including goods and services purchased elsewhere. Overall local employment may go up.

In other words, whether increases in demand for non-tradable industries increase local jobs and whether some policy promoting firm growth in those industries will actually increase local demand are separate issues. A local policy to encourage firm-specific growth in restaurants and brew pubs might or might not increase local demand—it depends on whether this policy offers sufficient improvement in variety and quality to encourage substitution towards these local goods and away from goods produced elsewhere. On the whole, the burden of proof should be on those arguing that adding one more brew pub will really increase local demand, but this does not contradict the idea that increased national demand for brew pubs might affect local demand.

Therefore, increases in national demand for any industry may increase local demand for those industries and have local job multiplier effects. As discussed above, the share-effect formulation automatically places greater weight on industries whose location quotients differ greatly across local areas, which tend to be the more-tradable industries. It is these differences that can explain how national demand trends might cause differential growth of different local areas. Even so, it is not theoretically inevitable that the lesser-weighted demand shocks to less-tradable industries have smaller effects on a local area's differential growth.

In practice, it could be the case that less-tradable industries may have smaller job multipliers. Local brew pubs and restaurants might have fewer local suppliers than a local auto plant. Less-tradable industries frequently pay lower wages, and these lower wages could lead to smaller effects on worker demand for locally produced goods and services.

Therefore, we also consider how local job multipliers vary if we include industries with various degrees of tradability. To do this, we rank all industries by tradability,[26] and then examine how the local job multiplier changes when demand shocks are focused more on the most-tradable industries or are expanded to include less-tradable industries. If less-tradable industries in practice have local job multipliers that are small, it is possible that restricting the demand shock prediction to only more-tradable industries might have higher explanatory power. We examine this possibility by seeing how the AIC varies as we expand the demand shock calculation to less-tradable industries. Because the number of right-hand side variables is the same, this amounts to simply looking at whether the sum of squared residuals increases or decreases as we expand the demand shock variable to less-tradable industries.

Area Fixed Effects

Including area fixed effects might seem like an obviously preferred choice[27] because they could be seen as controlling for the shift-effect component that is fixed over time for a given area. However, controlling for fixed effects 1) may not be necessary, 2) increases imprecision, and 3) probably increases upward bias in estimated job multipliers.

Concerning the first point, it is unclear why unobserved area characteristics affecting area fixed effects would have any obvious positive or negative correlation with the predicted demand-share shock to growth. Areas have various industrial compositions, which pay off or not during different time periods depending upon how those industries fare nationally, and it is not obvious that an area's industrial mix and how it fares nationally will be correlated with fixed area factors

[26] Appendix B discusses the methodology for doing this.

[27] The use of fixed effects in existing studies vary. Moretti (2010) does not include them, but van Dijk (2018) does. Partridge et al. (2017) include state fixed effects in a model of county multipliers, but Tsvetkova and Partridge (2016) do not include county fixed effects.

that might affect the shift component, such as an area's business tax rates and incentives or business services.

Keep in mind that Equation (6) already implicitly controls for fixed effects that affect an area's employment *level*. Equation (6) estimates the logarithmic percentage change in local employment from one year to the next as a function of demand shocks that cause logarithmic percentage shocks to local employment from one year to the next. Any area fixed effects that affect the area's employment level will be differenced out in this formulation.

On the second point, including area fixed effects effectively ignores any information that might be gained by seeing how, for example, areas with more manufacturing versus less manufacturing have done during the post-2000 downturn in manufacturing. As a result of ignoring this information, we would expect standard errors to increase.

Finally, concerning the third point, it is well known that, in a relatively short time series, models that include area fixed effects will be biased (e.g., see Nickell 1981). The intuition is that the estimated fixed effects include too much random variation in the disturbance term, which will not converge to zero when averaged over a short panel of data.

In the current case, the results of Nickell (1981) suggest that including area fixed effects will upwardly bias the local jobs multiplier estimates. For example, the estimated long-run job multiplier will equal the true long-run job multiplier plus a bias term. If the error term is serially uncorrelated over time, this bias will be approximated by the long-run job multiplier that would be estimated by regressing the dependent variable lagged one year times (1/(the number of years observed for each area minus one)).[28] We would expect the long-run job multiplier using a

[28] This combines results from Equation (26) and Equation (19) in Nickell (1981), as well as his discussion of his Equation (25). Nickell's formulation enables us to determine the sign and approximate lower bound of the bias if the error term is not serially correlated. This is done by this imaginary auxiliary regression of the lagged dependent variable on the right-hand side variables. If the error term is highly serially correlated, then determining

lagged dependent variable and the same demand shock terms on the right to be similar to the long-run job multiplier using the current period dependent variable. If the number of years in the panel for each area is 13 (which is what we use in our main estimates), then the bias will be around one-twelfth of the long-run jobs multiplier, and it will be overstated by 8 percent. The bias will tend to go up if the error term is positively serially correlated and down if the error term in negatively serially correlated.

The estimates that do not include area fixed effects may also be biased because they can be viewed as a combination of fixed effect estimates plus estimates that look at the cross-area variation in dependent variables and independent variables. However, the biases omitting area fixed effects will be less severe.

There clearly can be some area effects that are either random effects or that might have some serial correlation pattern over time. Therefore, the estimates in this paper that omit fixed effects do allow for clustering of the error term by geographic area to get closer to the true standard errors of the coefficient estimates.[29]

DATA

The advantage of the current study in terms of data is our use of industry employment at the 6-digit NAICS level for each U.S. county. These data are available for each year from 1998 to 2016 in the Upjohn Institute's WholeData database.

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the size and sign of the bias becomes more challenging.

[29] For comparison, we sometimes present some fixed effect estimates. These fixed effect estimates are not adjusted for clustering by area. The conditions for clustering are more stringent if area fixed effects are included, as shown by Abadie, Athey, Imbens, and Wooldridge (2017). Specifically, they show that with such fixed effects, adjusting for clustering is only appropriate if 1) the treatment effect (here the multiplier) is heterogeneous in the sample and either there is some 2a) clustering in the sample or 2b) clustering in assignment of the treatment, here the demand shock. We think it is highly questionable that these three conditions would hold in the current context. In particular, clustering in the sample or assignment does not seem to hold.

These detailed data by industry and county are derived from County Business Patterns (CBP). We use software owned by the Upjohn Institute, which was originally developed and fully described in a paper by Isserman and Westerveldt (2006). This software overcomes data suppressions in the original CBP data by first using CBP information that provides a range for the suppressed industry numbers to make an initial guess for each suppressed industry and county. The software then adjusts and iterates these guesses until all state numbers add up to the nation, all county numbers add up to the state, and all industries at some more detailed level add up to industries defined at a higher level.

This NAICS estimation procedure is first done separately for each year from 1998 to 2016. There have been some changes in NAICS definitions over time, but unfortunately, the raw CBP data have not been updated to reflect changes in NAICS definitions. To overcome these changes in NAICS definitions, which are mostly minor, we combine a few minor NAICS industries and do some modest estimation to adjust all NAICS definitions to the most recent definitions.

We end up with data for each county in the United States for 1998–2016 on 979 6-digit industries, 643 5-digit industries, 290 4-digit industries, 87 3-digit industries, and 24 2-digit industries. Because the underlying data are CBP data, they only cover private non-farm employment. The data exclude agricultural production, most government employees, self-employed individuals, private household employees, and railroad employees. The data count all employees who were employed as of the pay period including March 12 of a given year.

We also include some data on industry characteristics. One industry characteristic that is used is the extent to which an industry is tradable. “Tradability” is measured by measuring the Gini coefficient of the industry across all CZs, similar to the procedure used in Jensen and

Kletzer (2005) and van Dijk (2018). Essentially, we measure how unequal each CZ's share of national industry employment is compared to each CZ's share of total national employment. More unequal national industry shares by CZ imply greater tradability.[30] Another industry characteristic included is whether the industry is "high tech." We measure whether an industry is high tech based on the extent to which its employment in science, engineering, and technical occupations exceed the national average. This topic is discussed in more detail when we consider the high-tech results.

For local characteristics, we include size. This is based on the 1998 employment in a particular geographic unit, whether it is a county, CZ, metro area, BEA Economic Area, or state. Another local characteristic included is the prime-age employment rate, which is the employment-to-population ratio for 25–54-year-olds, as measured in the 2000 Census. More details on this topic are presented in Appendix B.

We also measure the initial high-tech share in a geographic unit. This share is measured using the high-tech industry definition and the WholeData database to determine each geographic unit's share of total employment in high-tech industries as of 1998.

MODEL SELECTION AND BASIC RESULTS ACROSS DIFFERENT TYPES OF GEOGRAPHIC AREAS

We now present the empirical estimates, beginning with model selection estimates, followed by basic estimates for different types of geographic areas (CZs, states, metro areas, BEA areas, and counties). We then consider how multipliers vary by the size of some geographic area (e.g., small versus large CZs). Multipliers are then allowed to vary with a CZ's prime-age

[30] See Appendix B for more details.

employment rate. Finally, we consider how multipliers vary for high-tech industries versus other industries and how these high-tech differentials vary in different types of CZs.

The model selection procedure uses data on all 691 CZs in the United States, excluding Alaska and Hawaii.[31] These initial regressions are weighted regressions, using 1998 employment in the CZ as weights.[32] Weighted regressions are used because we will shortly turn to considering different types of geographic areas, and estimates across different types of areas are more comparable using weights.

The model selection procedure considers from zero to six lags, and it considers all levels of industry detail from 3-digit NAICS to 6-digit NAICS level in creating the share-effect demand shock growth. To clarify, a lag length of six means that the model includes the estimated demand shock for growth from year $t-1$ to year t and six lags in that variable, extending back to the estimated demand shock in growth from year $t-7$ to year $t-6$. To appropriately use AIC to inform the model selection, and to compare the long-run multipliers, all models use the same data. This requires that the dependent variable can only start in the year 2004–2005, to allow for up to six lags in growth (back to the demand shock effect on growth from 1998 to 1999). Therefore, there are 691 CZs and 12 observations for each CZ, from 2004–2005 through 2015–2016, for a total of 8,292 observations. All regressions include year dummies. When combined with the 1998 weights, this implies that the regression is explaining a CZ's differential growth in a given year from the national average growth (excluding Alaska and Hawaii), as a function of each industry's national growth, weighted by the differential of that industry's year $t-1$ share

[31] Alaska and Hawaii are excluded on the grounds that multipliers might be quite different in these states.

[32] This is not exactly the same as using base period weights, which would require weighting each observation by CZ employment in the last year. This procedure was not selected because it would imply greater weights on more recent data, which seems arbitrary, and increasing weights on more rapidly growing CZs, which seems problematic because CZ growth is endogenous. In any event, there is far more cross-sectional than time-series variation in employment weights.

from the national average share for that industry.[33] In these initial regressions, all industries are included in the demand shock calculations; we will consider restricting the demand shock to more-tradable industries a bit later.

Table 1 summarizes the results. The table reports both the AIC and the estimated long-run local job multiplier. The AIC clearly suggests that the optimal model uses all the industry detail, down to six digits. The AIC also suggests that the optimal model uses 6-digit NAICS data and has only five lags, rather than the maximum possible six.[34] The choice of five lags over six is a close one, but the long-run jobs multiplier differs little when extending the lag length from five to six lags, which reinforces the decision to use five.[35] The use of five lags also has the advantage of allowing for one more year of data on all CZs to be added to the sample; that is, the observations for each CZ can start with 2003–2004 rather than with 2004–2005.[36]

At the outset, let us briefly note that the estimated multiplier of 1.562 in the "optimal" model is somewhat lower than one might expect based on the research literature, which would be a multiplier closer to 2. This is indicative of a more general conclusion that applies throughout our empirical analysis: our estimated average multipliers tend to be less than those reported in the research literature.

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[33] More precisely, we are calculating national growth and national industry shares using 1998 weights for all years, which will yield slightly different national numbers than if year t–1 for each CZ was used to form weights.

[34] This same model is chosen by the BIC as well. In the original model that simply includes demand shock lags, and not the sums of the coefficients up to some lag, the fifth lag in the five-lag specification has a t-statistic with an absolute value that exceeds 5, which leads to adding a fifth lag being strongly preferred by virtually any criteria to using only four lags. On the other hand, in the six-lag specification, the sixth lag in the growth shock has a t-statistic with an absolute value of <0.6, which suggests that the cumulative multiplier does not change much after the fifth lag.

[35] Five lags also seems plausible based on the possibility that it takes local economies some time to fully adjust to the congestion and agglomeration effects of a demand shock.

[36] In preliminary estimates, we also chose 6-digit NAICS and five lags in unweighted models that used a sample of 255 CZs with a 2010 population that exceeded 200,000.

The unchosen alternative models show different long-run multipliers. Using less industry detail tends to increase estimated long-run multipliers. For example, at a lag length of five, the long-run multiplier is 1.779 using NAICS industries at a 3-digit level, 1.662 at a 4-digit level, and 1.562 at the 6-digit level.

These findings suggest that using less detailed NAICS data may lead to an upward bias in estimated long-run multipliers. Such bias is plausible—if a CZ's 3-digit or 4-digit distribution of industries tends to be growing relatively fast nationally, one might think that the CZ's distribution of its 6-digit industries within the broader 3-digit or 4-digit categories would also tend to have favorable national growth trends. For example, if a CZ happens to emphasize 4-digit industries that use certain skills and produce certain types of products, the same may be true for the 6-digit industries within the 4-digit industry. The result is a type of omitted-variable bias. The predicted demand shock at the 3-digit or 4-digit level is positively correlated with an omitted variable, which is the predicted relative demand shocks to 6-digit industries within that 3-digit or 4-digit category.

The estimated multiplier also changes as we consider different lag lengths. The multiplier first increases as we consider longer lag lengths, and then it decreases. This probably reflects the underlying dynamics of local multipliers, which we will explore next.

Based on this selection of a model with 6-digit NAICS data and five lags in the demand shock, we can include the 2003–2004 data for all 691 CZs, thereby adding 691 observations and bringing the number of observations up to 8,983. Table 2 reports results for this "baseline model," along with some comparisons to a CZ fixed effect model. Turning to the baseline model first, the dynamic pattern of local multiplier effects is in accord with expectations. The immediate local job multiplier is 1.487; that is, each 1,000-job demand shock immediately leads to a total of 1,487 jobs. This local job multiplier increases over the next two years to 2.030. This medium-run increase is probably due to some lag in how local businesses respond to the increased demand, both as suppliers to local business and to local workers. The multiplier then declines over the next three years to its long-run multiplier of 1.605. This decline is most plausibly explained as resulting from congestion effects, whereby the increased number of local jobs drives up local wages and prices, which discourages some local business activity. But based on the model selection results in Table 1, the multiplier stabilizes after five lags.

For comparison, we also report alternative regression results where we include CZ fixed effects. This alternative specification modestly increases estimated multipliers. For example, the long-run multiplier goes up from 1.605 to 1.695, an increase of about 6 percent. This increase, however, is not statistically significant (Table 2). The increased multiplier from including CZ fixed effects is consistent with our previously stated point that fixed effect estimates will be positively biased. In addition, adding CZ fixed effects increases the standard errors. For example, the standard error on the long-run multiplier increases from 0.105 to 0.170, an increase of about two-thirds.

As discussed, most of the reduced-form multiplier research literature does not estimate an explicit dynamic model of the effects of demand shocks. Rather, a demand shock over some time period is considered, often around 10 years. The hope is that this 10-year regression will be some weighted average of short-run and long-run multiplier effects, with a greater weight on the long-run multiplier.

In Table 3, we examine what our data shows when we consider 10-year periods, rather than estimating a dynamic model. We estimate nine regression models. Each model considers a single 10-year time period, ranging from 1998–2008 to 2006–2016. The dependent variable is logarithmic growth over the 10-year period. The demand shock variable is predicted share effect growth in log employment over the 10-year period.

As Table 3 shows, these 10-year multipliers generally are not necessarily close to the long-run multipliers estimated with the same data using an explicit dynamic model. Many of the multipliers lie outside the range of cumulative multipliers shown in Table 2. The multipliers are as low as 0.896 (for the 1998–2008 period), which is well below any of the cumulative multipliers estimated in Table 2. The 10-year multipliers also seem to be quite sensitive to the particular time period chosen, ranging from 0.896 for 1998–2008 to 1.596 for 2005–2015. These results suggest that researchers should be cautious in interpreting multipliers estimated using long differences as long-run multipliers. Some of the estimates are similar (the 1.596 estimate), whereas others are not (the 0.896 estimate). Our long-run estimates use this entire range of data from 1998–1999 to 2015–2016, so all of these 10-year estimates overlap with portions of our sample period.

So far, these estimates have used all industries in the WholeData database in the estimations, but what happens when we restrict the demand shock variable to only the more-tradable industries? To determine the effect, we redo the model 19 alternative ways that only include the "most tradable" *x* percent of all 6-digit industries in calculating the demand shock variables. (The dependent variable is the same logarithmic growth term.) The percent cut-offs are calculated by ranking all the 6-digit industries by tradability and then selecting industries in order so that the most tradable industries that add up to *x* percent of total employment are selected. The percentages of most-tradable industries considered range from 5 percent to 95 percent (at 5 percent intervals). The 100 percent regression is identical to the baseline regression.

These 19 alternative regressions show some surprising results (Table 4). First, the estimated long-run multiplier is very similar for almost all 20 regressions, although it is a bit higher if we only look at the most-tradable 5 percent of all industries.[37] This higher multiplier may occur because a CZ with favorable trends in these most-tradable industries may also have favorable trends in less-tradable industries. The other 19 regressions generally have long-run multipliers that have a surprisingly narrow range, from just 1.442 to 1.605.

The implication is that there is little reason to think that less-tradable industries have lower multipliers, on average. These less-tradable industries will receive less weight in explaining differential growth across CZs because their industry location quotients vary less across CZs. But once we adjust for this lower weight, a national demand shock to these less-tradable industries have similar multiplier effects to more-tradable industries.

A second surprising result is that the AIC suggests that the optimal model includes all industries (Table 4).[38] Given that all 20 of these models have the same number of right-hand side variables, this amounts to saying that including all industries minimizes the sum of squared residuals. Adding additional industries to the demand shock variable generally increases explanatory power.[39] In other words, we gain predictive power in explaining CZ growth from

[37] It is unclear why this occurs. Uncovering the reasons will require more work studying how multipliers vary across industries. The 5 percent most-tradable industries include 153 of the 979 6-digit industries. (This is a general trend in NAICS codes; more-tradable industries tend to be more finely divided, particularly in manufacturing.) Out of this 5 percent of total employment, 43 percent is in just one 6-digit NAICS code, 561330 (Professional Employer Organizations). This is not temporary help, but rather a case in which an employer has outsourced some of its human resources role to a separate firm. In addition, out of this 5 percent category, the remaining 57 percent of jobs are in the following types of industries: transportation equipment manufacturing (12%), agriculture-related manufacturing (10%), mining including oil and gas (8%), casino hotels (7%), and miscellaneous (20%).

[38] Because of this finding, the baseline formulation cannot do an instrumental variable estimate even if we chose to do so because the right-hand side variable that would be instrumented for with the current demand shock would be the same as the dependent variable.

[39] This is by no means an inevitability. If less-tradable industries mostly add noise, with little multiplier effect, then the sum of squared residuals could go up by adding more industries. In fact, the AIC is lower at 80 percent of all industries included than at 85 percent, but the AIC is still lower at 100 percent.

knowing whether a CZ has a lot of health-care facilities or restaurants, not just knowing whether it has a lot of auto or software companies. The most natural conclusion is that all demand shocks matter, even if tradable-industry demand shocks are weighted more in construction of the share-effect demand shock variables.[40]

We now turn to exploring how results differ when this same model is applied to different types of geographic areas, specifically CZs, states, metro areas, BEA Economic Areas, and counties. In comparing results for long-run multipliers across different geographic units, we emphasize the weighted results (Panel A of Table 5). As argued above, these weighted results use more comparable weights for different geographic units with similar industrial structures.

For the three definitions of local labor market area, the weighted results are remarkably similar. The long-run multipliers range from 1.605 for CZs to 1.710 for metro areas and 1.786 for BEA areas. Defining a broader geographic area does allow for broader multiplier spillovers on suppliers and retailers, but the effects at the local labor market level are slight.

The state-level long-run multiplier is only 27 percent greater than the CZ multiplier, so multiplier effects do not have huge spillovers into this broader political unit. This is one limitation of incentive policies that are focused at the state level—many of the benefits will not extend greatly across the entire state. The county multiplier is only 24 percent less than the CZ multiplier, so it seems that multiplier effects on suppliers and retailers are quite concentrated at the county level. This helps explain why there is sometimes such intense competition for jobs within a local labor market area.

[40] As discussed previously, this does not mean that adding in a firm in a non-tradable industry will increase local demand as much as adding in a firm in a tradable industry. The demand shocks estimated here are national demands for specific industries. It seems plausible that this is generalizable to the effects of adding in a local firm in a tradable industry, but not to adding in a local firm to a non-tradable industry.

When we move to unweighted results, multipliers are generally lower (Panel B in Table 5). For example, the CZ multiplier in the unweighted results is 1.341 versus 1.605 in the weighted results. Which type of multiplier is "right"? There is no definitive answer. The unweighted versus weighted multipliers use different weights on different industries, given that they are comparing relative geographic area growth with different national means. The weighted results put more weight on the larger geographic units, and therefore represent effects for the "average job" or "average person," whereas the unweighted results represent effects for the average geographic area.

Of note is that the disparity across geographic units expands when we use unweighted estimates. For reasons previously discussed, we do not see the comparison of unweighted multipliers across different geographic units as being informative. The estimates compare different weights of industry demand shocks. The differences could be due not to true geographic differences, but to different multipliers for different industries.

Together, the weighted and unweighted results suggest somewhat smaller multipliers than have generally been found in prior research. The state multipliers in Table 5, at 1.9 to 2.0, are less than multipliers found in input-output models and the REMI model (generally 2.5 and up). The metro level multipliers, at 1.6 to 1.7, are less than the local multipliers of 1.8 to 2.4 found from input-output models, the two-stage multiplier of 2.6 in Moretti (2010), and the reduced-form multiplier of 1. 9 in van Dijk (2018). The county-level multipliers in Table 5, at 0.9 to 1.2, are towards the low end of the range of 0.9 to 2.1 found in the studies of Partridge et al. (2017) and Tsvetkova and Partridge (2016). Of course, some of this prior research focuses on multipliers for particular types of industries, such as manufacturing, and we will have to see if these lower multipliers hold up when we consider industrial mix effects.[41]

In Panel C of Table 5, we report weighted results with geographic fixed effects. The results are consistent with the argument that geographic fixed effects lead to an upward bias in estimated multipliers. The long-run multipliers increase from 3 percent to 11 percent for the various geographic units.

As noted previously, the observed differences between unweighted versus weighted results could be due to larger CZs (or other geographic unit) having larger multipliers than smaller CZs (or other geographic unit). Alternatively, these differences could be due to different weighting of industry growth rates. To determine which is the cause, we must examine how multipliers vary for different size units, which we examine in the next section.

HOW MULTIPLIERS VARY WITH GEOGRAPHIC UNIT SIZE

We now present results for a given type of geographic unit, but allow the multiplier to vary by the size of the geographic unit. As argued above, this is best done by allowing such size variation within a single regression. This approach means that geographic units of different sizes with similar industry mixes are considered to have the same demand shock.

Table 6 shows results for CZs. The 691 CZs are sorted by 1998 employment size. After sorting, the CZs are divided into nearly equal-sized groups of 34 or 35 CZs. The highlighted results are those for a single pooled regression, with the multiplier allowed to vary across each of the 20 groups.

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[41] As noted previously, some of the reduced-form matching models also provide hints that input-output models tend to overstate multipliers, but these reduced-form matching models tend to have imprecise estimates.

As can be seen, there is some sign that the multiplier may increase as we go from the very smallest CZs (less than 10,000 jobs in 1998, the first four groups) to slightly larger CZs (10,000 and above, group 5 and above). However, once we reach a size cut-off of 10,000 jobs, there is little sign of a systematic increase in the long-run multiplier with CZ size. If anything, there is some indication that the multiplier might decline slightly at the largest CZ sizes. For example, the multiplier is 1.8 for CZs in the third-largest group (average of 267,000 jobs), 1.6 for the second-largest group (average of 515,000 jobs), and 1.5 for the largest group (average of 2.3 million jobs). The confidence intervals for these groups overlap by quite a bit, so this evidence is only suggestive of a modest decline for the largest CZs.

The results in Table 6 suggest that the difference between the weighted and unweighted results in Table 5 is largely not due to a greater weighting for larger CZs. Rather, it is probably due to the weighted and unweighted results using somewhat different weights for different industries with different multipliers.

For comparison, Table 6 also shows results when we estimate multipliers with a separate regression for each of the 20 CZ size classes. In these 20 separate regressions, the largest estimated multipliers are for the top three size classes, and by far the largest multiplier (over 2.3) is for the largest size class. When these separate regressions are compared with the pooled regression, we can see that the separate regression results are misleading. As argued in a previous section, the different multipliers in these separate regressions probably reflect different industry weightings when we separately estimate regressions by size CZ class and not true differences in the multiplier for the same industry weighting. The separate regressions by size classes are not evaluating the same relative demand shocks for the different size classes, so they are not really comparing apples with apples.

Figure 1 provides an illustration of how multipliers vary with CZ size class from the pooled regression and separate regressions. As can be seen, the separate regressions suggest an upwards trend in multiplier with CZ size. In contrast, the pooled regression mostly shows similar long-run multipliers, with a slight upside-down U shape for the very smallest and very biggest CZs.

In Table 7, we show similar results for other geographic units. For states, BEA areas, metro areas, and counties, we sort each type of geographic unit by 1998 employment size, and then estimate, in a pooled regression, how the multiplier varies by size classes. Because there are many more observed units for some types of geographic units than others (e.g., counties versus states), we use different numbers of size-class groups for the different types of geographic units: 5 groups for states, 10 groups for BEA areas, 20 groups for metro areas, and 30 groups for counties. The pooled regression is done so that, in comparing the long-run multiplier across different size classes, different geographic units with similar industrial mix will have the different industry shocks weighted similarly in calculating the demand shock variable.

Across most size classes of each type of geographic unit, there is little systematic variation of the long-run multiplier with 1998 employment size. There is some sign of a slight upside-down U-shape, with some increase in the multiplier as we go from the very smallest units to slightly bigger units, and some decrease as we go to the very largest units. This is particularly marked in the county data, which includes some very small counties. But once we get beyond the very smallest units, the variation in long-run multipliers is slight.

What is going on here? Perhaps the most persuasive theory is that, on the margin, the advantages of larger geographic unit size for increasing the multiplier are roughly offset by the disadvantages. The advantages of larger geographic unit size might include the presence of more diverse local suppliers and retailers, which might result in larger input-output effects. Larger geographic unit size might also increase the urbanization variety of agglomeration economies. The disadvantages of larger geographic unit size would be various congestion effects that increase with size, such as higher land prices and nominal wages, as well as other higher local prices.

As mentioned above, from a general equilibrium perspective, we might expect that the equilibrium configuration of geographic units at different sizes might exhibit this rough constancy in the long-run multiplier. If demand shocks are roughly similar across geographic units of different size, on average, divergences in the long-run multiplier would over time lead to a change in the distribution of geographic unit sizes, until the long-run multiplier becomes more equal across size classes.

How do we reconcile the relative similarity of the long-run multiplier with geographic unit size with the finding that states tend to have higher long-run multipliers than the various local labor market types and that the various local labor market types have higher long-run multipliers than counties? When we move from the county level to a local labor market area, we encompass more of the spillover effects on suppliers and retailers. However, congestion effects will not go up. A similar argument applies going from a local labor market area to a state. The state perspective captures effects on suppliers elsewhere in the state, as well as potential fiscal spillovers due to effects on the state budget. There are no differences in congestion effects. This is not true when considering size differences within a given geographic area type.

We now turn to more formally testing how multipliers vary with CZ size and with other CZ characteristics. However, we will now switch to putting more emphasis on the unweighted estimates. The above analysis suggests that at least above some modest CZ size, there are no significant increases in the multiplier with CZ size. This argues against putting a greater weight on the largest CZs, as weighted regression does. In addition, if the multiplier does not vary much with CZ size, using differential weights by CZ size is a less efficient regression strategy than using unweighted regressions. We will, however, report selected weighted estimates as a check on the unweighted estimates. To avoid being unduly influenced by the very smallest CZs, all subsequent estimations in this paper will be based on the 284 CZs (out of 691) that had 50,000 or more jobs in 1998. These 284 CZs encompassed 93 percent of all employment in the 691 CZs.

In Table 8, we show unweighted versus weighted results for this restricted sample. The weighted results are quite similar to what we get for the full weighted sample. The unweighted results are similar to the weighted results, but the values are a little smaller. For example, the unweighted long-run multiplier of 1.375 is about 16 percent below the weighted multiplier of 1.630. As one would expect, the unweighted estimates are a bit more precise, reflecting the more effective unweighted variation than weighted variation in the CZ sample, both for employment growth and the demand shock.

To verify that the unweighted sample of the larger CZs does not show substantial variation of multipliers with CZ size, we run an unweighted pooled regression in which we allow multipliers to vary by CZ size group. The 284 CZs are divided into 20 groups, each with 14 or 15 members (Table 9). We focus on short-run and long-run multipliers. The short-run multiplier is the immediate effect of a demand shock on total jobs, which is simply equal to the coefficient in the estimating equation on the current demand shock variable. The long-run multiplier is the final or equilibrium multiplier after five years, which is equal to the sum of the coefficients on the demand shock variables from the current zero-lag variable up to the fifth lag in the demand shock.

Neither group of multipliers shows large variations with CZ size. Short-run multipliers seem to slightly increase with CZ size, whereas long-run multipliers seem to slightly decrease with the largest CZ sizes. In the short run, larger size may increase multipliers by allowing for a greater variety of suppliers and retailers. In the long run, larger size may reduce multipliers by increasing congestion effects. The variations are slight in both cases.[42]

Figure 2 compares how long-run multipliers vary with CZ size in the unweighted versus weighted regressions. Specifically, the comparison is between the long-run multipliers in the unweighted pooled regression from Table 9 (with the larger CZs) and the long-run multipliers from the weighted pooled regression from Table 6. The pattern with CZ size is similar where the two regressions overlap. In both cases, there is a slight tendency for the multipliers to decrease in the very largest CZs.

We now turn to allowing the multiplier to vary with CZ size by interacting the demand shocks with measures of employment size. The interaction term is the natural logarithm of CZ employment in 1998. The demand shocks are interacted with this variable deviated from its sample means. We tested including only linear interactions with this variable, as well as up to quadratic interactions, cubic interactions, and quartic interactions. In the unweighted regression, the linear interaction specification minimized the AIC.

Table 10 reports this linear interaction specification. For comparison, we report both the unweighted regression and the weighted regression using CZ employment in 1998 as weights. The regression is consistent with the group dummy specification in suggesting that in the short

[42] There is no reason to think that the functional form assumptions used here lead to any of these results. In all cases, we consistently scale all variables, both the dependent variable and the demand shock variable, so they measure changes in the logarithm of employment. This is roughly equivalent to scaling both the employment change variable and the demand shock by the area's initial employment. Thus, this functional form allows for the usual form of the multiplier, where a given demand shock to the number of jobs will have some fixed coefficient relationship to total local job creation.

run, larger CZs tend to have a larger multiplier, and in the long run, larger CZs tend to have a smaller multiplier. However, these variations are slight. The standard deviation of the ln(employment 1998) interaction variable is about 1.0 in the unweighted sample and 1.2 in the weighted sample, both of which are equivalent to roughly tripling the size of a CZ. A one standard deviation variation in employment size, or a tripling of employment size, will affect the short-run or long-run multipliers by 0.08 or less, based on either the unweighted or weighted interactions.

Although the linear interaction specification minimizes the AIC, the quadratic interaction with the long-run multiplier in the unweighted specification is almost significant at the 5 percent level, with a *t*-statistic that is slightly greater (in absolute value) than 2 (Table 11). Table 12 reports the implications of this quadratic interaction specification for the long-run multiplier at different values for CZ employment in 1998. The estimates for the 20 groups from Table 9 are also shown in Table 12, and the two groups of estimates are compared in Figure 3. An employment size in 1998 of about 207,000 maximizes the long-run multiplier, but the long-run multiplier does not significantly decline until CZ employment size is in the 500,000 to 1 million range. And even there, the decline is modest.

All in all, the consistent result is that long-run local job multipliers do not vary much with CZ size. To the extent there is variation, there is some decline for the very largest CZs.[43]

[43] These results seem roughly consistent with recent results on growth and city size by Rappaport (2018). Rappaport finds that growth tends to first rise with city size and then fall for the very largest cities. This is consistent with congestion effects dominating for the largest cities. How growth varies with city size will depend only partially on the job multiplier. Growth variation in city size will also depend on the incidence of initial demand shocks with city size. So, Rappaport's result that growth initially increases with city size is quite consistent with our results that do not show this pattern for the multiplier, if we assume that the initial demand shocks tend to become stronger with city size for most of the city size distribution. Rappaport also finds different effects on growth of city size versus city density. We hope to explore the effects of density in future research.

MULTIPLIERS AND THE CZ'S PRIME-AGE EMPLOYMENT RATE

We now turn to considering how multipliers vary with the CZ's initial prime-age employment rate. This variation with prime-age employment rate is of particular interest because an important policy issue is how the benefits of state and local economic development policies (and other job growth promotion policies) vary with local economic conditions. Do incentives and other economic development policies have greater benefits in areas that might be argued to "need jobs" because they have high unemployment rates or low employment rates (employment-to-population ratios)? We know from other research that for a given shock to jobs, areas with higher unemployment or lower employment rates will have more of the job growth reflected in an increased employment rate, rather than by in-migration (Austin, Glaeser, and Summers 2018; Bartik 2015). But are there also variations with local economic conditions in how an initial demand shock affects the overall change in local jobs via the local job multiplier?[44]

To explore this question, we add the prime-age employment rate as an interaction term with the demand shock variable in the regressions explaining local job growth. We use the prime-age rate in the year 2000 in the CZ, as measured in the Census. The prime-age employment rate in a CZ is less likely to vary dramatically over time relative to the nation as compared to the local unemployment rate.

Ideally, instead of using the prime-age employment rate as of 2000, we would instead control for immediate labor availability at the time of the demand shock more closely. But this is endogenous and depends upon past demand shocks, so doing so would raise some econometric issues. The CZ employment rate as of 2000 could be seen as one important reduced-form

[44] A working paper by van Dijk (2015) looked at the effects of initial unemployment. In a non-parametric analysis, he finds some evidence that the local job multiplier goes up for areas that initially have very high unemployment.

predictor of the endogenously determined employment rates for the CZ over the time period up to 2016. We experiment with specifying the employment rate in three ways: prime-age employment rate, ln(prime-age employment rate), and ln(1-prime-age employment rate). The AIC narrowly prefers ln(prime-age employment rate).

As shown in Table 13, a lower prime-age employment rate does not boost the short-run multiplier but does boost the long-run multiplier. The most plausible explanation for this is that a lower prime-age employment rate implies more available local labor. As a result, congestion effects in the local labor market due to job growth are lower. With lower long-run congestion effects, the long-run local job multiplier is higher.

However, these prime-age effects on the long-run multiplier are relatively modest. The standard deviation in ln(prime-age employment rate) is 0.064—or about a 6 percent variation in the prime-age employment rate. Based on the unweighted estimates, a one-standard deviation reduction in the prime-age employment rate will increase the long-run local job multiplier by 0.097 (= 0.064 × 1.518). With a mean multiplier in the sample of 1.479, that represents about a 7 percent increase in the multiplier. In other words, we would expect that a CZ with a one-standard deviation lower prime-age employment rate would have a benefit-cost ratio for a local incentive policy that would be at least 7 percent higher. Based on current evidence, however, the more important implication for public policy of a lower prime-age employment rate is that local job growth will have greater effects on local employment rates.[45]

[45] The results in Austin, Glaeser, and Summers (2018) suggest that an initial prime-age employment rate that is lower by one standard deviation will increase local job growth's effect on local employment rates by 32 percent. This is derived from values presented in their Table 4, specifically by multiplying 0.064 times 2.171 and dividing by 0.433. The estimates in Bartik (2015) are for how local job growth's effects on local employment rates vary with the initial local unemployment rate, and so are not strictly comparable. However, some calculations suggest that a one standard deviation change in the initial local unemployment rate has similar effects on job growth effects on local employment rates as a one standard deviation change in the initial prime-age employment rate, although obviously of opposite sign.

In the weighted regression results, the interaction between the prime-age employment rate and the local demand shock is statistically insignificant, possibly because of the different weighting on demand shocks or on different employment size classes. Some evidence that this is due to different prime-age effects for different size classes of CZs is found if we add a double interaction, between the CZ employment size and its prime-age employment rate. In the weighted specification, adding in this "double interaction" minimizes the AIC and yields some statistically significant long-run multiplier effects of the double-interaction term. (In the unweighted specification, adding in the double-interaction worsens the AIC.)

If we do the calculation, the double-interaction specification implies similar effects of prime-age employment rates for CZs whose size is close to the unweighted mean of the 284 larger CZs, which is a 1998 employment of around 200,000 (see Table 14 and Figure 4). A lower prime-age employment rate seems to increase long-run multipliers for CZs of 500,000 or less but not above that point. One interpretation of this result is that local labor supply constraints due to higher prime-age employment rates are more important for small and medium-sized CZs. For the largest CZs, even when the prime-age employment rate is quite high, new employers can readily find additional labor.

HIGH-TECH MULTIPLIERS

We now consider how local job multipliers vary with one particular type of industry mix effect, specifically, when the local demand shock is to a "high-tech" industry. Our focus on high-tech makes sense because economic development strategies are often focused on high-tech industries. In addition, there are some good reasons to think that high-tech industries might have higher multipliers, which might be greater in larger areas or areas with a greater concentration of high tech. Finally, Moretti (2010) found an extraordinarily high local job multiplier of 5.9 for high tech, and we wanted to see whether his result holds up using a different database and methodology.

As mentioned previously, our definition of high-tech industries is derived from the high-tech industry definition used by the Bureau of Labor Statistics (BLS), which defines a high-tech industry as one that has a significantly higher percentage of its employment in science, engineering, and technician occupations (Hecker 2005). The BLS baseline definition of a high-tech industry is any industry whose science/engineering/technician occupational employment share exceeds twice the overall national average. BLS also defines more restrictive tiers of high-tech industry, using cut-offs of three times the national average and five times the national average.

BLS defines high-tech industry at the 4-digit NAICS industry level. Its information on occupational staffing patterns is estimated from 2002 Occupational Employment Statistics data. In that data, the national average for high-tech occupational employment is 4.9 percent. So, a high-tech industry is any industry whose employment in such occupations exceeds 9.8 percent, with corresponding values for the more restrictive tiers.

We wanted to update the BLS's definitions using more recent occupational employment data and to fully exploit our access to more detailed industry data at the 6-digit level. To get up-to-date occupational data for detailed industries, we looked at 2012–2016 data from the American Community Survey, as detailed in Appendix B. In this more recent data, 5.87 percent of total national employment is in science, engineering, and technician occupations, up from BLS value of 4.9 percent. On average, occupational employment has become more "high tech" across many industries.

For our basic high-tech definition, we simply followed the BLS baseline definition. We defined a high-tech industry as any industry with employment in science, engineering, and technical occupations that exceeded twice the national average. However, we used the new national average of 5.87 percent, so our cut-off was any industry whose employment in these high-tech occupations exceeded 11.7 percent. This is the "Tech 4" category in our results. In addition, we used more detailed industry data where available, down to the 6-digit level, as detailed in Appendix B.

We also use more restrictive cut-offs to match as closely as possible the BLS list of different high-tech industries. Our thinking was that the BLS to some extent cherry picked its cut-offs to include industries that corresponded intuitively to most people's perceptions of high-tech industries. We wanted to ensure that we also considered high-tech definitions that corresponded to popular intuition. To do so, we examined a list of 4-digit NAICS industries along with our estimates of the high-tech occupational employment shares for each industry (Table 15). Industries are ranked by their percentage of high-tech occupations based on the 2012–2016 ACS data. In addition, the industries are only listed down to the last 4-digit industry that BLS classifies as high-tech using its 2002 data.

The table also indicates the BLS classifications. Tier 1 is more than five times the high-tech occupational share (using 2002 data), Tier 2 is less than five but more than three times the national average, and Tier 3 is less than three but more than twice the national average. We then tried to pick cut-offs that approximated the BLS classifications. The corresponding cut-offs (using 2012–2016 ACS data) were 27.1 percent or greater employment in high-tech occupations (Tech 1), 16.9 percent or greater (Tech 2), and 13.46 percent or greater (Tech 3). These three cut-offs combined with the original Tech 4 cut-off lead to the four definitions of high tech used in our regression.

Note that although our Tech 1 through Tech 3 categories approximate the same set of industries that BLS identified in their work, the Tech 4 category goes beyond that of the BLS. In particular, although our Tech 4 definition uses the BLS rule of twice the national average in high-tech occupations, it identifies motor vehicle manufacturing as a high-tech industry, which was not the case in the BLS classification based on 2002 data. Apparently, motor vehicle manufacturing has become more of a high-tech industry in terms of the types of occupations it uses.

To get a sense of what national trends can be seen in these different high-tech industries, we refine our Tech 1 through Tech 4 definitions to define specific high-tech tiers. Tier 1 includes the Tech 1 industries; Tier 2 includes industries in Tech 2, but not Tech 1; Tier 3 includes industries in Tech 3, but not those in Tech 1 or 2; and Tier 4 includes industries in Tech 4, but not those in Tech 1, 2, or 3. In Table 16, we show national employment in these high-tech tiers for each year from 1998 to 2016. Note that we list total employment in these high-tech industries, not just employment in science, engineering, and technician occupations.

The results in Table 16 do not support the notion that there is a rapidly growing share of jobs in high-tech industries. High-tech occupations are growing, but not necessarily high-tech industries. There is some growth in Tier 1 and Tier 4 high-tech industries, but there are declines in Tier 2 and Tier 3. Over the entire time period from 1998 to 2016, the share of employment in non-high-tech industries has grown. The lack of a strong trend towards more high-tech industry may in part reflect that many high-tech industries are in manufacturing, and manufacturing job growth has not done well since 2000.

To determine the multiplier for high-tech, we run regressions in which demand shock variables are defined separately for high tech versus low tech. In other words, the demand shock variable for high tech is defined by the following two equations:

(8) $$\textit{logarithmic demand shock for high-tech} = \left\{ \ln\left(E_{mt-1}\right)\left(1 + D_{hmt}\right) - \ln\left(E_{mt-1}\right) \right\}$$

where D_{hmt} is the high-tech demand shock and is defined as

(9) $$D_{hmt} = \sum_{j=high-tech\ industry} P_{jmt-1} * G_{jnt}$$

A similar definition holds for the low-tech shock, where we sum over only the "non-high-industries" in forming the demand shock.

In estimating the high-tech and low-tech multipliers separately, we include both sets of logarithmic demand shocks in the estimation, with up to five lags in both sets of demand shock variables. This identifies the effects of demand shocks to high-tech (low-tech) industries, holding constant demand shocks to the other sector. To identify the *differential* effects of high tech, and to test whether its multiplier is different from the overall multiplier, we include the logarithmic high-tech demand shock variables along with the logarithmic shock to all industries, which is the demand shock variable we have been using in the previous regressions (Equations [6] and [7]). This formulation tests the effects of high-tech demand shocks holding constant the overall demand shock, which implicitly is a test of changing the industry mix of the overall demand shock towards high-tech industries and away from low-tech industries.

Table 17 shows the multipliers for high-tech versus low-tech industries, using the various definitions of high tech. Looking first at the unweighted results, the narrower definitions of high tech (Tech 1 through Tech 3) do not show any significant evidence that high-tech multipliers are greater than low-tech multipliers. But with these narrower definitions of high-tech, the high-tech shocks are small enough that the standard errors are large, so there is considerable imprecision in the multiplier estimates. However, with the Tech 4 definition, which matches the BLS baseline definition of twice the national average, high-tech multipliers are quite a bit higher than low-tech multipliers in the long-run. In the unweighted regression, the long-term multiplier is 1.970 for high-tech demand shocks, which is 70 percent greater than the 1.161 long-term multiplier for low-tech demand shocks. The difference between the long-run high-tech multiplier and the overall long-run multiplier is statistically significant ($t = 2.64$). Furthermore, as compared to the other three high-tech definitions, the Tech 4 definition minimizes the AIC, which in this case is the same as minimizing the sum of squared residuals.

In the Tech 4 unweighted results, the time pattern of high-tech versus low-tech multipliers is consistent with expectations. Low-tech multipliers increase for the first two years, from 1.273 to 1.761, presumably due to some lags in the demand responses of local suppliers and retailers. The low-tech multiplier then decreases to 1.161 in the long-run, presumably due to congestion effects. High-tech multipliers also increase for the first two years, from an initial value of 1.402 to a two-year value of 1.934, also presumably due to some lags in the demand responses of local suppliers and retailers. But the long-run multiplier, at 1.970, is almost the same as the two-year multiplier of 1.934. What happened to the congestion effects? A reasonable hypothesis is that the agglomeration economies of more high-tech jobs in a CZ have offset the congestion effects.

The weighted regression results are qualitatively consistent with the unweighted regression but show a lesser high-tech versus low-tech differential and are more imprecise. In the weighted regression results, the long-run high-tech multiplier is quite similar to the unweighted results, but the low-tech multiplier is higher in the weighted regressions, which reduces the differential.

One obvious question is whether these high-tech results for the Tech 4 definition reflect what are generally assumed to be higher multipliers for manufacturing, due to manufacturing's tiers of suppliers and relatively high wages. This question is addressed in Table 18, where Panel A shows the results of a specification that omits the high-tech demand shock variables but includes three other categories of demand shocks: all non-manufacturing industries, durable manufacturing industries, and non-durable manufacturing industries.[46] Panel A shows no evidence that manufacturing has any higher long-run multipliers than non-manufacturing. In fact, it appears than non-durable manufacturing tends to have lower multipliers than non-manufacturing.[47]

Panel B directly allows high tech to compete with the manufacturing variables as industry mix demand shocks that might lead to larger or smaller long-run multipliers. The regression includes both the overall demand shock for all industries and three industry-group-specific multipliers for durable, non-durable, and high-tech industries. Given this specification, the overall demand shock effect reflects demand shocks to industries that are *NOT* in manufacturing or high tech. That is, although the demand shock is calculated including all industries, the regression will calculate the effects of this demand shock holding the industry-group-specific demand shock variables constant, and so must reflect the demand shocks that fall outside the industry-group-specific categories. The industry-group-specific variables represent shocks to that industry group, holding constant the overall demand shock (and the other group-specific demand shocks). Therefore, these industry-group-specific variables reflect an increase in positive shocks

[46] To be clear, high-tech industries are included in these demand shocks if they happen to be in any of these three categories of industries.

[47] The higher multiplier for durable manufacturing than for non-durable manufacturing is consistent with van Dijk (2018), but inconsistent with Moretti (2010). Van Dijk argues that some results may have been accidentally switched in Moretti (2010), but we have not attempted our own replication.

to that industry group, offset in the overall demand shock variable by negative shocks outside the included industry group categories. As a result, these industry-group-specific variables are best interpreted as differential effects of demand shocks to these industry groups relative to demand shocks to all other industries.

Panel B shows no evidence of differential positive multipliers for manufacturing, but the results do suggest differential demand shock effects of high tech. The differential high-tech multiplier is of significant size, estimated at 0.840. Here is how to interpret that differential multiplier. For high-tech industries, the long-run multiplier will be 0.840 higher than it would otherwise be. If the high-tech industry is in non-manufacturing, the high-tech multiplier will be the non-high-tech, non-manufacturing multiplier of 1.261 plus 0.840. So, in this case, the high-tech multiplier is two-thirds higher. If the high-tech industry is in durable manufacturing, the long-run multiplier would be 1.261 plus the –0.302 durable differential plus 0.840, compared to just (1.261 – 0.302) for durable non-high-tech industries. If the high-tech industry is in non-durable manufacturing, its multiplier will be 1.261 plus –0.210 plus 0.840, compared to (1.261 – 0.210) for non-high-tech, non-durable industries.

The weighted results for the same analysis are shown in Table 19. There are no statistically significant effects of industry mix, whether high tech or manufacturing. In addition, the estimates are more imprecise than those shown in Table 18.

Overall, these results suggest that, in the average CZ, a relatively broad definition of high-tech industry has significantly and substantively larger long-run job multiplier effects, but these differential high-tech multipliers are not as high as the 5.9 value estimated by Moretti (2010). However, there is some doubt from the weighted results as to whether these differential high-tech effects extend to the largest CZs.

HIGH-TECH MULTIPLIERS AND AREA CHARACTERISTICS

We now turn to estimating how high-tech multipliers vary with a CZ's baseline characteristics. From a policy perspective, this helps identify areas where a more aggressive high-tech growth policy might have a higher payoff in terms of total jobs created.

Our expectation is that high-tech multipliers might be somewhat larger in larger CZs and in CZs with greater baseline shares of high-tech industries. The expectation is that high-tech industries both benefit from and generate various forms of agglomeration economies. Growth of high-tech industries in larger places or places with a greater concentration of high-tech industry would be expected to have greater spillover agglomeration economies, which would be reflected in higher local job multipliers.

In this discussion of local high-tech shares, keep in mind that our Tech 4 definition includes some high-tech manufacturing, such as motor vehicle manufacturing. To give a sense of the variation in the local high-tech share, Table 20 reports selected high-tech shares for selected CZs of the 284 CZs with 1998 employment greater than 50,000. The table includes the top three in high-tech share as of 1998, the bottom three, and all other CZs with a 1998 employment that exceeded 1 million.

Some of the data presented in Table 20 probably matches many readers' expectations. Silicon Valley is the most high-tech CZ, and the Washington, DC area, Seattle, Boston, and the Twin Cities all also have relatively high high-tech shares. But the high-tech share is also high in Kokomo, IN; Maury County, TN; and the Detroit area, all of which are high in high tech because of motor vehicle manufacturing. On the other side, New York is not a particularly high-tech city. In addition, it is evident from this table that the largest CZs tend to be average or above-average in their baseline high-tech shares. Therefore, it is important to control for the effects of CZ size when estimating the effect of high-tech share.

In Table 21, we report how short-run and long-run high-tech and low-tech multipliers vary with area characteristics (unweighted). Long-run high-tech multipliers increase with city size, a lower prime-age employment rate, and a higher initial high-tech share. All of these long-run effects are not only statistically significant, but substantively important. A one standard deviation change in the baseline area characteristics is associated with a change in the multiplier ranging from 0.379 to 0.762. This is a large effect relative to a mean high-tech multiplier of 1.428. The area characteristic that is most substantively important in altering the long-run high-tech multiplier is the baseline high-tech share. A CZ that has a baseline high-tech share that is higher by one standard deviation—about 39 "log percentage points"—will have a long-run high-tech multiplier that is higher by 0.762 ($t > 4$).

These high-tech interaction effects are all much smaller in the short run, which is consistent with an interpretation of these area characteristics as features that might be associated with agglomeration economy effects. A high-tech demand shock might have greater effects in larger CZs with more available labor and a larger high-tech sector. But the agglomeration economy advantages of a high-tech shock in such CZs might take time to develop, as other firms over time respond to enhanced agglomeration economies that boost productivity.

The low-tech multiplier is far less associated with these area characteristics, both in the short run and long run. This finding is also consistent with these area characteristics reflecting agglomeration economies associated with high tech. For the initial high-tech share, the low-tech multiplier, especially in the long run, is negatively associated with a larger initial high-tech share. This may reflect congestion effects of a larger initial high-tech share. A larger initial high-tech share might be associated with higher costs, which would reduce somewhat the multipliers of low-tech demand shocks. In contrast, for high-tech demand shocks, such congestion effects are offset by agglomeration economies.

For comparison, we also report the interaction results from a weighted regression in Table 22. The estimates are qualitatively similar to Table 21, but they are less precise.

To further explore how the high-tech (and low-tech) multipliers vary with a CZ's initial high-tech share, we estimate specifications with various functional forms for how the demand shocks interact with initial high-tech share. The AIC indicates a linear interaction between the demand shocks, and ln(initial high-tech share) is preferred to a quadratic interaction or to including dummies for CZs grouped by initial high-tech share. But in the quadratic interaction specification, the long-run high-tech multiplier has a *t*-statistic of −2.68, which indicates that the multiplier "curves" more with a larger initial high-tech share than is assumed by the log-linear interaction; that is, the long-run high-tech multiplier increases with the initial high-tech share, but at a declining rate.

Table 23 shows how the long-run high-tech and low-tech multipliers vary with the initial high-tech share in three different specifications: a linear interaction between the demand-shock variables and ln(initial high-tech share), a quadratic interaction, and an interaction between the demand shocks and 20 dummies for CZs sorted into groups of 14 or 15 CZs by initial high-tech share.[48] These results again suggest that the long-run high-tech multiplier increases with the initial high-tech share. However, there are signs of threshold effects. In CZs with low initial high-tech shares, there is little if any high-tech job multiplier. The high-tech multiplier increases rapidly until at least somewhere close to the average U.S. high-tech share, somewhere in the 10–

[48] These results are all from unweighted regressions. The analogous weighted regressions are similar qualitatively but are much more imprecise.

15 percent high-tech share range. Beyond an initial 15 percent high-tech share, the high-tech multiplier may go up, but the increase is more modest if it occurs at all. The long-term low-tech multiplier decreases somewhat with a higher initial high-tech share, but the decreases are modest.

Figure 5 compares the high-tech and low-tech multipliers, using the specification with dummies for CZs grouped by high-tech share. As the figure shows, the high-tech multiplier does not consistently exceed the low-tech multiplier until we get to an initial high-tech share of about 15 percent.

Overall, these results suggest that high-tech multipliers may be higher than low-tech multipliers, but this is mostly true for CZs that are at least slightly above the national average in initial high-tech share (the unweighted CZ average is 12.4 percent initial high-tech share). These results also suggest that although high tech has larger multipliers for CZs with larger high-tech shares than those with smaller high-tech shares, there are some threshold effects. Beyond a 15 percent initial high-tech share, the high-tech job multiplier does not significantly increase. This pattern could mean that agglomeration economy effects have some diminishing returns or that congestion effects become increasingly large with an increased high-tech share.[49]

[49] Further work should be done to see if these results can be reconciled with the results in Fallah, Partridge, and Rickman (2013). They look at high-tech growth and the initial high-tech concentration, whereas we look at the high-tech multiplier and the initial high-tech concentration. The multiplier could be higher due to greater high-tech spillovers on non-high-tech industries with a greater initial high-tech concentration, even if high-tech growth itself is no greater, as they find. In addition, obviously the two studies use different samples and different types of models. For example, the Fallah, Partridge, and Rickman results are not focused on national demand shocks to local high-tech growth, which might have different patterns from overall high-tech growth.

CONCLUSION

One of the most notable conclusions from these results is that many multipliers are overstated. Average multipliers should generally be assumed to be at most 2.0 at the state level and perhaps a quarter lower at the local labor market level, at about 1.6 or below. These values are significantly below many claimed multipliers.

One exception is high-tech multipliers. For areas that are above average in initial high-tech share, high-tech multipliers may be significantly higher, perhaps close to 3, at the local labor market level. These higher values are closer to many claimed multipliers, but they are still not as high as the 5.9 estimated by Moretti (2010).

By claiming multipliers that are higher than are likely to occur, studies of local economic development programs frequently will be overly optimistic. The over-estimated multipliers are due in part to ignoring, or under-estimating, the influence congestion effects have on higher local costs and reduce long-run multipliers; however, these congestion effects may sometimes be offset by agglomeration economy effects. Our estimates do find such agglomeration economy offsets for high-tech industries, but mostly in areas with an above average initial high-tech share.

We plan to continue our work on multiplier effects by examining the broader influences of industry mix, for example, by looking at high-wage versus low-wage industries. We also plan to look at how multipliers are affected by housing supply restrictions (Saiz 2010).

APPENDIX A

SHOULD THE SHARE-EFFECT INSTRUMENT USE NATIONAL GROWTH OR NATIONAL GROWTH MINUS OWN AREA?

It has become customary in many applications of this share-effect instrument to use the national growth for each industry minus the growth of the area itself in that industry (e.g., Amior and Manning 2018; van Dijk 2018). This leads to a different set of national growth rates by industry for each area in the sample. On the other hand, some authors follow the practice of Bartik (1991) and Blanchard and Katz (1992), and use overall national growth (e.g., Moretti 2010; Beaudry, Green, and Sand 2018).

The usual argument for the "leave own area out" approach is that the growth of the area itself is endogenous. Therefore, it appears that national growth minus the area's growth is more exogenous to various unobserved factors affecting local area growth. But omitting own-area growth does not solve the perceived endogeneity problem. If anything, it is likely to make the problem worse.

The purpose of this demand-shock instrument is to isolate demand-side factors that affect local industry growth. The idea is that local growth is affected by national factors affecting demand for each industry. There are also local factors affecting demand for each industry, such as the area's business taxes and incentives, and public services affecting business productivity. There are also labor supply factors affecting local industry growth, such as migration, current and past birth rates and death rates, and education. We want to focus on the national demand-side factors, which we hope are uncorrelated with the labor supply factors affecting local industry growth. Correlations with local demand-side factors would not be a problem because either local or national demand shocks would represent demand-side influences on local economic variables.

The concern is that it is possible that local labor supply factors could be correlated with national growth. For example, suppose the Silicon Valley labor force becomes more skilled or attracts more in-migrants from abroad. It is then possible that U.S. total employment in some high-tech industries would increase solely because of Silicon Valley's labor supply shock. The United States would gain market share in the international market for high-tech industries. The overall national growth shock to high-tech industries is affected by this local supply shock.

In this case, though, such labor supply shocks to Silicon Valley would surely also cause U.S. employment in high-tech industries to be reallocated from other areas to Silicon Valley. So, U.S. growth in high-tech industries minus Silicon Valley would also be affected by labor supply shocks that alter Silicon Valley's attractiveness to high-tech industries.

The issue is: which is more endogenous to a local supply shock to an area that is a major share of overall U.S. employment, the overall national growth in the industry or the national growth minus the area? A reasonable argument is that local supply-side factors in the U.S. probably have a more major effect on the allocation of jobs within the U.S. than to overall U.S. jobs. It is generally the case that business location and expansion decisions are more elastic to location differentials the narrower the geographic scope considered. So, the allocation of business activity within the United States in response to local cost differentials is likely to be more elastic than is the response of overall U.S. business activity in response to cost differentials.

To put it another way, overall U.S. employment in an industry is affected in major ways by U.S. exchange rates, as well as economic costs, demand, and other trends in a variety of countries around the world. The share of activity in one area in the United States—or in the

United States minus that one area—will always also be affected by relative costs within the United States, which will include local labor supply trends. Therefore, we follow the original form of the share-effect instrument from Bartik (1991) and use national growth rates by industry as "exogenous" demand-side factors driving local growth. We argue that this is more "exogenous" than is national growth excluding the own area.

In practice, this decision is likely to have modest effects on the estimation. For most industries and areas, the area's employment is small enough that there is little difference between overall national growth by industry and national growth excluding the own area. As noted in the text, van Dijk (2018) found that switching from national growth to national growth excluding own area reduced the reduced-form multiplier by only 3 percent. If the multiplier excluding own area is more endogenous, we might expect the multiplier to be biased downwards.[50]

[50] In van Dijk (2018), switching from overall national growth by industry to national growth excluding own area reduces the reduced-form multiplier from 2.72 to 2.64. If the multiplier excluding the own area is more endogenous, we might expect the multiplier to decrease.

APPENDIX B

DATA

The following section will briefly outline the processes involved in compiling the data and generating the variables used throughout our paper. While we use data from a variety of sources, the main datasets we use are unsuppressed CBP data from WholeData (Bartik, Biddle, Hershbein, and Sotherland 2018) and ACS microdata from IPUMS USA (Ruggles et al. 2018). We use WholeData to compute industry level Gini coefficients as a proxy for tradability as shown in Jensen and Kletzer (2005) with a few slight modifications, and we use a combination of WholeData and IPUMS to compute a measure of "high-technology" employment concentration for all levels of NAICS industries detail. Finally, IPUMS is used by itself to the compute the prime-age employment rate for each of the geography types used in our analyses.

Industry Gini Coefficients

In our paper, we use the industry employment concentration Gini outlined by Jensen and Kletzer (2005) as the measure of industry tradability. To compute this measure, we use 2016 unsuppressed CBP industry employment data aggregated to the 2000 CZ level.

The intuition of this Gini is very similar to that of the traditional income-based Gini, except the Jensen Gini measures the distribution of an industry's employment concentration across geographies instead of income across individuals. As with the traditional Gini, a coefficient approaching one denotes perfect inequality of industry employment concentration, or typically, an instance where all of an industry's employment is located within one geographic unit. At the other extreme, a coefficient of zero denotes perfect equality of industry employment concentration, which is an instance where the share of an industry's total employment located in each and every geographical unit is exactly equal to the share of overall national employment located in that unit.

Our procedure for calculating these Gini coefficients follows almost exactly from the procedure in the Jensen and Kletzer paper, so we will avoid going into too much detail. However, an important difference between our procedure and their procedure is the order in which the data are sorted prior to computing the cumulative shares of industry employment and total employment for the geographies. Jensen and Kletzer suggest that the geographical units be sorted in ascending order of the region's share of industry employment. We found that in some cases this leads to a lumpy Lorenz curve, so we sort by the industry's share of the geographical unit's total employment. These changes make little difference in practice, but they prevent odd theoretical outcomes such as negative Gini coefficients.

Industry Tech Employment

We calculate high-tech employment shares for 2- to 6-digit NAICS industries by using the 2012–2016 5-year American Community Survey (ACS) sample from IPUMS to assign high-tech employment shares to our county-level industry employment from WholeData at the finest detail possible and then aggregate upwards. We begin by calculating a high-tech employment share for each industry reported by the ACS (typically a slightly coarsened version of NAICS 3- and 4-digit detail). This share is equal to the ratio of individuals working in the BLS defined high-tech occupations[51] within each industry to the total number of people employed in the

[51] The BLS defines high-technology occupations as: computer and mathematical scientists, SOC 15-0000; engineers, SOC 17–2000; drafters, engineering, and mapping technicians, SOC 17–3000; life scientists, SOC 19–1000; physical scientists, SOC 19–2000; life, physical, and social science technicians, SOC 19–4000; computer and information systems managers, SOC 11–3020; engineering managers, SOC 11–9040; and natural sciences managers, SOC 11–9120.

industry. We restrict our IPUMS sample to individuals currently employed and without imputed values for the weeks worked and usual hours worked variables.

We then assign these industry-level tech shares to our 6-digit national industry employment data at the finest possible detail. For example, if we found that the tech share in industry 123456 is 0.14, then we assign this directly to the observation representing 2016 employment in NAICS-123456. Now let's say there is a second industry with a tech share of 0.25, but this industry, NAICS-13579, is only reported at the 5-digit level in the ACS. Furthermore, there are three 6-digit industries within this 5-digit; 135791, 135792, and 135793. We would then assign a tech share of 0.25 to each of these three observations.

Once we have complete 6-digit employment data at the national level with tech shares assigned according to the finest available level of detail, we calculate weighted averages of 6-digit tech shares, weighting by the 6-digit employment, for all other levels of industry detail. This is equivalent to calculating the estimated tech employment for each industry by multiplying the 6-digit employment by the tech share and then calculating the total tech employment and total overall employment and taking the ratio of the two. We are left with a dataset with tech employment shares for NAICS detail from 2-digit to 6-digit. See Table B1 for the 10 highest and lowest shares of high-tech among 3-digit industries.

Prime-Age Employment

Finally, we compute prime-age employment using a 2000 ACS IPUMS dataset of individuals without imputed values for employment status that has been cross walked from public-use microdata areas (PUMAs) to counties using the Missouri Census Data Center's Geographic Correspondence Engine (MABLE Geocorr). Using ACS weights and these cross-walks, we compute the number of employed individuals and total individuals between the ages of 25 and 54 for each county. Using additional crosswalks, we compute these totals for the 2000 CZ, BEA Area, Metro Area, State, and national levels. Finally, we take the ratio of the employed individuals to the total individuals, which is our prime-age employment rate.

Table B1 Highest and Lowest Share of High-Tech among 3-Digit Industries

Rank	3-Digit industry	Tech employment share
1	Data Processing, Hosting, and Related Services	0.3460
2	Computer and Electronic Product Manufacturing	0.3310
3	Other Information Services	0.3285
4	Publishing Industries (except Internet)	0.2779
5	Professional, Scientific, and Technical Services	0.2755
6	Telecommunications	0.2235
7	Oil and Gas Extraction	0.2222
8	Chemical Manufacturing	0.1929
9	Transportation Equipment Manufacturing	0.1724
10	Utilities	0.1597
	. . .	
77	Amusement, Gambling, and Recreation Industries	0.0097
78	Building Material and Garden Equipment and Supplies Dealers	0.0092
79	Motor Vehicle and Parts Dealers	0.0087
80	Gasoline Stations	0.0077
81	General Merchandise Stores	0.0070
82	Social Assistance	0.0069
83	Food and Beverage Stores	0.0058
84	Truck Transportation	0.0048
85	Nursing and Residential Care Facilities	0.0039
86	Personal and Laundry Services	0.0034
87	Food Services and Drinking Places	0.0014

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Table 1 Multiplier Tests for Industry Detail and Lag Length

PANEL A: AIC FOR EACH MODEL

	Industry detail			
Lag length	3	4	5	6
0	(42691)	(43060)	(43152)	(43241)
1	(42702)	(43093)	(43219)	(43311)
2	(42704)	(43096)	(43224)	(43324)
3	(42731)	(43117)	(43238)	(43336)
4	(42804)	(43187)	(43315)	(43394)
5	(42813)	(43217)	(43375)	**(43455)**
6	(42824)	(43221)	(43375)	(43454)

PANEL B: LONG-RUN MULTIPLIER AND STANDARD ERROR BY MODEL

		Industry detail			
Lag length		3	4	5	6
0	Multiplier	1.891	1.762	1.636	1.557
	Standard error	(0.105)	(0.093)	(0.083)	(0.081)
1	Multiplier	2.020	1.936	1.870	1.782
	Standard error	(0.132)	(0.116)	(0.103)	(0.099)
2	Multiplier	2.081	1.991	1.930	1.865
	Standard error	(0.155)	(0.132)	(0.121)	(0.112)
3	Multiplier	1.963	1.889	1.853	1.793
	Standard error	(0.146)	(0.128)	(0.126)	(0.116)
4	Multiplier	1.806	1.731	1.693	1.653
	Standard error	(0.134)	(0.120)	(0.119)	(0.115)
5	Multiplier	1.779	1.662	1.611	**1.562**
	Standard error	(0.132)	(0.116)	(0.113)	**(0.107)**
6	Multiplier	1.782	1.678	1.620	1.568
	Standard error	(0.132)	(0.119)	(0.113)	(0.108)

PANEL C: SOME DESCRIPTIVE STATISTICS

Variable	Weighted mean	Weighted standard deviation	Residualized standard deviation (from year dummies)
Dependent variable: logarithmic growth	0.0051	0.0293	0.0196
Predicted log growth, 0 lags	0.0074	0.0230	0.0052

NOTE: All these regressions use the same sample with 8,292 observations. To accommodate up to six lags, the observations are on all 691 commuting zones (CZs) in the United States (excluding Alaska and Hawaii), with the dependent variable reflecting annual growth for 12 years, from 2004–2005 through 2015–2016. The dependent variable is the shift-share predicted growth, using industry characteristics at a specified level of detail and the number of lags in predicted growth specified. The Akaike Information Criterion (AIC) should be minimized for the "optimal model," which ends up being a model with five lags at the 6-digit level of industry detail. The parentheses for the AIC values indicate negative values. The reported long-run multiplier sums coefficients over the specified lags. The standard errors are clustered at the CZ level. All regressions are weighted by each CZ's 1998 employment. All regressions include year dummies, but not CZ dummies. Standard errors are clustered by CZ. The BIC also selects the same lag length and level of detail as the AIC. Optimal values shown in bold.

Table 2 Results with Five Lags and 6-Digit Industry Specification with and without CZ Fixed Effects

PANEL A: MULTIPLIER RESULTS

Lag length		Baseline results	With CZ fixed effects	Hausman test on CZ fixed effect estimate minus baseline estimate	Ratio of CZ to baseline
0 (immediate)	Cumulative multiplier	1.487	1.512	0.025	1.0168
	standard error	(0.089)	(0.095)	(0.033)	
1	Cumulative multiplier	1.840	1.873	0.033	1.0177
	standard error	(0.095)	(0.108)	(0.051)	
2	Cumulative multiplier	2.030	2.090	0.059	1.0293
	standard error	(0.106)	(0.126)	(0.069)	
3	Cumulative multiplier	1.962	2.039	0.077	1.0391
	standard error	(0.112)	(0.147)	(0.095)	
4	Cumulative multiplier	1.830	1.923	0.093	1.0509
	standard error	(0.122)	(0.159)	(0.102)	
5 (long-run)	Cumulative multiplier	1.605	1.695	0.090	1.0562
	standard error	(0.105)	(0.170)	(0.134)	

PANEL B: DESCRIPTIVE STATISTICS

	Variable	Weighted mean	Weighted standard deviation	Standard deviation after residualizing from year dummies	Standard deviation after residualizing from year and CZ dummies
	Dependent variable: change in ln employment in CZ	0.0055	0.0287	0.0196	0.0182
	Fifth lag in predicted growth in ln employment in CZ	0.0036	0.0235	0.0066	0.0055

NOTE: These regressions have 8,983 observations for 691 CZs (all U.S. CZs, excluding those in Alaska and Hawaii). The dependent variable extends for 13 years, from 2003–2004 to 2015–2016. All regressions include year dummies. All regressions are weighted by CZ employment in 1998. Estimates have standard error clustered by CZ.

Table 3 10-Year Regression Results

PANEL A: MULTIPLIER RESULTS FOR EACH 10-YEAR PERIOD									
	1998–2008	1999–2009	2000–2010	2001–2011	2002–2012	2003–2013	2004–2014	2005–2015	2006–2016
Multiplier	0.896	0.986	1.207	1.169	1.182	1.322	1.471	1.596	1.531
Standard error	(0.170)	(0.148)	(0.135)	(0.135)	(0.149)	(0.155)	(0.196)	(0.214)	(0.197)
PANEL B: SELECTED DESCRIPTIVE STATISTICS									
Weighted mean of dependent variable (logarithmic employment growth)	0.0873	0.0102	−0.0392	−0.0335	0.0128	0.0211	0.0303	0.0430	0.0317
Standard deviation	0.0969	0.0941	0.0915	0.0842	0.0793	0.0793	0.0823	0.0804	0.0759
Mean of ln (predicted growth)	0.1144	0.0377	−0.0148	−0.0106	0.0317	0.0412	0.0476	0.0610	0.0520
Standard deviation	0.0423	0.0404	0.0372	0.0317	0.0284	0.0272	0.0259	0.0244	0.0205

NOTE: Derived from nine different regressions, each comparing a different 10-year period. Each has 691 observations. Regressions are weighted by the base period employment for each regression (e.g., 1998 for 1998–2008, 1999 for 1999–2009, etc.). Standard errors are robust.

Table 4 How Multipliers Vary with What Industries are Included in Demand Shock, Industries Ranked by "Tradability"

	Demand shock only including industries whose tradability is in the top																			
	5%	10%	15%	20%	25%	30%	35%	40%	45%	50%	55%	60%	65%	70%	75%	80%	85%	90%	95%	100%
PANEL A: MULTIPLIERS AND STANDARD ERRORS																				
Cumulative multiplier (standard error) at lag 5:																				
	1.969	1.589	1.596	1.587	1.559	1.521	1.476	1.449	1.442	1.480	1.501	1.530	1.515	1.527	1.524	1.534	1.525	1.569	1.583	1.605
	(0.224)	(0.159)	(0.143)	(0.129)	(0.128)	(0.122)	(0.113)	(0.110)	(0.107)	(0.110)	(0.109)	(0.108)	(0.107)	(0.107)	(0.105)	(0.105)	(0.105)	(0.106)	(0.106)	(0.105)
AIC	(45,674)	(45,833)	(46,013)	(46,097)	(46,115)	(46,240)	(46,365)	(46,442)	(46,456)	(46,492)	(46,662)	(46,790)	(46,836)	(46,891)	(46,938)	(46,981)	(46,968)	(47,020)	(47,038)	(47,102)
PANEL B: DESCRIPTIVE STATISTICS																				
Demand shock mean	−0.0001	−0.0007	−0.0010	−0.0009	−0.0005	0.0008	0.0013	0.0018	0.0024	0.0029	0.0038	0.0043	0.0044	0.0047	0.0052	0.0053	0.0056	0.0064	0.0070	0.0080
Demand shock standard deviation	0.0031	0.0048	0.0060	0.0071	0.0078	0.0094	0.0107	0.0118	0.0127	0.0133	0.0144	0.0158	0.0163	0.0170	0.0179	0.0189	0.0189	0.0202	0.0214	0.0223
Demand shock residualized standard deviation	0.0029	0.0039	0.0042	0.0045	0.0046	0.0047	0.0049	0.0050	0.0050	0.0050	0.0051	0.0051	0.0052	0.0052	0.0052	0.0052	0.0052	0.0052	0.0052	0.0052
Dependent variable mean	0.0055																			
Dependent variable standard deviation	0.0287																			
Dependent variable residualized standard deviation	0.0196																			

NOTE: These results are from 20 different regressions. All have same dependent variable, the change from last year to this year in the ln of employment for a commuting zone (CZ). Each regression's sample is 691 CZs. All regressions include year dummies. Regressions are weighted by 1998 employment by CZ. Standard errors are clustered by CZ. All regressions include a current predicted demand shock, and five lags in the demand shock. The observations run to 13 years per CZ, from 2003–2004 to 2015–2016. The long-run multiplier for each regression is reports, which is the cumulative effect after five lags. The regressions differ in what industries are used to define the demand shock. The 100% results are identical to the baseline results in Table 2. The other regressions only include *x*% of 2016 employment by industry, with *x*% varying from 5% to 95% in 5% intervals. The *x*% regressions only include the industries that are most tradable and just add up to more than *x*% of total employment. Tradability is judged based on the Gini coefficient of industry employment by CZ, as described in the text and Appendix B. The parentheses for the AIC indicate negative values.

Table 5 Long-Run Multipliers for Different Geographic Units: Weighted, Unweighted, and Fixed Effect Results

PANEL A: WEIGHTED RESULTS								
Cumulative multiplier after	CZ	State	Metro	BEA Economic Area	County	Ratio of state to CZ	Ratio of county to CZ	Ratio of county to BEA
5 years	1.605	2.040	1.710	1.786	1.225	1.271	0.763	0.686
Standard error	(0.105)	(0.330)	(0.119)	(0.175)	(0.065)			
PANEL B: UNWEIGHTED RESULTS								
Cumulative multiplier after	CZ	State	Metro	BEA Economic Area	County	Ratio of state to CZ	Ratio of county to CZ	Ratio of county to BEA
5 years	1.341	1.910	1.559	1.715	0.916	1.425	0.683	0.534
Standard error	(0.116)	(0.208)	(0.108)	(0.176)	(0.062)			
PANEL C: WEIGHTED RESULTS WITH GEOGRAPHIC UNIT FIXED EFFECTS								
Cumulative multiplier after	CZ	State	Metro	BEA Economic Area	County	Ratio of state to CZ	Ratio of county to CZ	Ratio of county to BEA
5 years	1.695	2.100	1.824	1.959	1.356	1.239	0.800	0.692
Standard error	(0.170)	(0.376)	(0.197)	(0.261)	(0.104)			
Ratio of multiplier with geo fixed effects (Panel C) to multiplier without fixed effects (Panel A)	1.0562	1.0298	1.0663	1.0968	1.1073			
PANEL D: DESCRIPTIVE STATISTICS								
Weighted mean dependent variable	0.0038	0.0083	0.0053	0.0060	0.0028			
Weighted standard deviation of dependent variable	0.0419	0.0257	0.0313	0.0281	0.0803			
Weighted standard deviation of dependent variable, after residualizing from year means	0.0196	0.0128	0.0188	0.0154	0.0319			
Weighted mean 5th lag in predicted growth	0.0020	0.0037	0.0034	0.0030	0.0005			
Weighted mean standard deviation of predicted growth 5th lag	0.0231	0.0231	0.0232	0.0227	0.0261			
Weighted mean standard deviation of predicted growth 5th lag, after residualizing from year dummies	0.0066	0.0043	0.0066	0.0052	0.0094			
Number of observations	8,983	637	4,680	2,301	40,340			

NOTE: These results are from 15 different regressions, with five different samples times five different specifications. The five samples are CZs, states, metro areas, BEA Economic Areas, and counties. All samples include all areas in the U.S., except Alaska and Hawaii. All samples include 13 years for all areas, except a few county observations are missing because counties were merged. Four of the samples encompass the same portion of the U.S.; the metro sample only includes metro areas. The three different specifications are weighted by 1998 employment in each geographic unit, unweighted, and weighted by 1998 employment and with fixed effects included for each geographic unit in the sample. All specifications include year fixed effects. All standard error estimates allow the error term to be clustered by the geographic unit. All specifications include current year demand shocks to growth and five lags to the demand shock variable. The reported multipliers are the cumulative effect after five lags.

Table 6 Long-Run Multipliers Across 20 CZ Size-Classes

Group number	Weighted mean 1998 employment in CZ group	Pooled regression results: Long-run multiplier (after 5 years); standard error below	Separate regressions for each of 20 groups: Long-run multiplier (after 5 years); standard error below
1	1,258	1.036	0.584
		(0.275)	(0.275)
2	3,172	1.418	1.379
		(0.338)	(0.700)
3	5,392	1.198	0.393
		(0.264)	(0.483)
4	8,571	1.482	1.482
		(0.326)	(0.552)
5	11,086	1.728	1.340
		(0.229)	(0.247)
6	14,493	1.553	0.671
		(0.200)	(0.355)
7	17,965	1.718	1.839
		(0.216)	(0.279)
8	21,908	1.783	1.582
		(0.175)	(0.258)
9	26,542	1.490	1.170
		(0.168)	(0.269)
10	32,228	1.942	1.397
		(0.198)	(0.310)
11	40,249	1.875	1.816
		(0.190)	(0.261)
12	48,685	1.617	1.740
		(0.158)	(0.255)
13	59,531	1.591	1.362
		(0.159)	(0.352)
14	73,964	1.834	1.520
		(0.170)	(0.309)
15	92,255	1.605	1.420
		(0.134)	(0.246)
16	125,416	1.669	1.531
		(0.180)	(0.451)
17	180,161	1.642	1.050
		(0.121)	(0.166)
18	267,337	1.805	1.910
		(0.136)	(0.248)
19	514,876	1.635	1.853
		(0.172)	(0.356)
20	2,349,991	1.504	2.344
		(0.145)	(0.351)

NOTE: These results are from 21 different regressions. One regression includes all commuting zones (CZs) but allows the multiplier to vary across 20 CZ size classes. The other 20 regressions are separate regressions for each of these 20 size classes. The size classes each have about the same number of CZs (i.e., about 34 or 35 of the 691 CZs). All regressions are weighted by 1998 employment and include year dummies. All regressions include current demand shocks to growth and five lags in demand shocks. The long-run multiplier, or the cumulative effect of a demand shock after five lags, is reported.

Table 7 Long-Run Multipliers by Employment-Size Class, Different Types of Geographic Units

States		BEA areas		Metro areas		Counties	
Group weighted mean	Long-run multiplier for group (standard error)	Group weighted mean	Long-run multiplier for group (standard error)	Group weighted mean	Long-run multiplier for group (standard error)	Group weighted mean	Long-run multiplier for group (standard error)
325,408	2.082	48,927	1.625	27,749	2.167	191	−0.551
—	(0.352)	—	(0.255)	—	(0.205)	—	(0.434)
811,508	2.371	83,291	1.514	36,923	1.980	502	0.665
—	(0.324)	—	(0.239)	—	(0.218)	—	(0.329)
1,561,033	2.193	116,719	2.164	43,510	2.007	766	1.128
—	(0.387)	—	(0.198)	—	(0.247)	—	(0.195)
2,589,496	2.114	163,727	1.952	48,745	1.680	1,044	1.203
—	(0.351)	—	(0.206)	—	(0.201)	—	(0.234)
6,052,407	2.099	206,525	1.837	54,182	1.764	1,331	1.108
—	(0.345)	—	(0.169)	—	(0.215)	—	(0.239)
—	—	288,999	2.226	60,209	1.791	1,603	0.889
		—	(0.202)	—	(0.189)	—	(0.145)
		433,523	1.896	71,052	1.399	1,905	0.786
		—	(0.175)	—	(0.232)	—	(0.150)
		625,596	1.892	82,906	1.927	2,242	0.976
		—	(0.266)	—	(0.206)	—	(0.191)
		1,036,468	1.784	96,159	1.920	2,628	1.508
		—	(0.205)	—	(0.200)	—	(0.155)
		3,162,657	1.755	116,269	1.715	3,035	1.047
			(0.192)	—	(0.194)	—	(0.150)
				135,495	1.900	3,530	1.271
				—	(0.169)	—	(0.319)
				154,766	1.616	4,053	1.212
				—	(0.176)	—	(0.149)
				185,392	1.805	4,625	1.278
				—	(0.234)	—	(0.176)
				228,253	1.871	5,223	1.416
				—	(0.182)	—	(0.122)
				271,031	1.991	5,952	1.298
				—	(0.192)	—	(0.146)
				396,388	1.746	6,804	1.504
				—	(0.140)	—	(0.154)
				514,129	1.629	7,865	0.997
				—	(0.231)	—	(0.124)

Table 7 (Continued)

States		BEA areas		Metro areas		Counties	
Group weighted mean	Long-run multiplier for group (standard error)	Group weighted mean	Long-run multiplier for group (standard error)	Group weighted mean	Long-run multiplier for group (standard error)	Group weighted mean	Long-run multiplier for group (standard error)
				628,113	1.815	8,929	1.171
				—	(0.171)	—	(0.173)
				859,313	1.697	10,160	1.250
				—	(0.169)	—	(0.168)
				1,946,965	1.694	11,746	0.942
				—	(0.161)	—	(0.139)
						13,403	1.286
						—	(0.179)
						15,839	1.430
						—	(0.120)
						18,872	0.974
						—	(0.129)
						23,307	1.527
						—	(0.134)
						29,174	1.460
						—	(0.116)
						38,393	1.338
						—	(0.112)
						50,849	1.499
						—	(0.117)
						79,260	1.337
						—	(0.109)
						152,480	1.217
						—	(0.122)
						533,496	1.218
						—	(0.107)

NOTE: These estimates are from four different regressions, one for each type of geographic area. Each estimate includes year dummies and a current demand shock variable and five lags in that demand shock variable. The estimation allows the multiplier to vary across different size classes of the geographic unit. Size classes are based upon 1998 employment. Each geographic unit type is sorted by 1998 size and arranged into groups with the same (as nearly as possible) number of geographic units in each size class. States are divided into five groups, BEA areas into 10 groups, Metro areas into 20 groups, and counties into 30 groups. The long-run multiplier after five lags of the demand shock is reported. Standard errors are adjusted for clustering of the disturbance term by geographic unit. The weighted mean 1998 employment for each size-class is also reported.

Table 8 Multipliers for CZs with Greater than 50,000 in Employment in 1998, Unweighted versus Weighted

Lag length for cumulative effects to the right (years)	Unweighted multiplier (standard error)	Weighted multiplier (standard error)
0	1.302	1.540
	(0.069)	(0.113)
1	1.673	1.928
	(0.093)	(0.117)
2	1.773	2.125
	(0.101)	(0.129)
3	1.662	2.040
	(0.106)	(0.138)
4	1.504	1.909
	(0.119)	(0.151)
5	1.375	1.630
	(0.110)	(0.130)
Mean of dependent variables	0.0051	0.0058
Standard deviation of dependent variable	0.0292	0.0275
Standard deviation of dependent variable, after residualizing from year means	0.0215	0.0177
Mean of 5th lag of growth	0.0023	0.0038
Standard deviation of 5th lag of growth	0.0233	0.0236
Standard deviation of 5th lag of growth, after residualizing from year means	0.0076	0.0061

NOTE: Both regressions are on 284 CZs, all those with greater than 50,000 in employment in 1998. The number of years is 13, from 2003–2004 to 2015–2016, so total observations is 3,692. All regressions include year dummies. Standard errors are clustered by CZ. The weighted regressions use 1998 employment as weights. The descriptive statistics also differ in being unweighted and weighted, respectively. The residualized statistics are the remaining standard deviation, unweighted or weighted, after residualizing from a regression of the relevant variable on year dummies, with that regression being unweighted or weighted.

Table 9 How Immediate and Long-Run Multipliers Vary with CZ Size, Unweighted Pooled Regression

Group mean employment, 1998	Immediate multiplier (standard error)	Long-run multiplier (standard error)
52,838	1.189	1.107
	(0.127)	(0.233)
57,830	1.176	1.253
	(0.098)	(0.208)
63,288	1.362	1.475
	(0.112)	(0.249)
69,893	1.237	1.319
	(0.088)	(0.216)
75,339	1.538	1.795
	(0.117)	(0.233)
81,846	1.139	1.346
	(0.135)	(0.210)
91,134	1.275	1.589
	(0.096)	(0.148)
99,436	1.163	1.221
	(0.088)	(0.204)
111,409	1.270	1.451
	(0.116)	(0.247)
132,717	1.283	1.409
	(0.142)	(0.237)
154,543	1.361	1.448
	(0.082)	(0.117)
178,356	1.405	1.455
	(0.107)	(0.120)
204,840	1.393	1.474
	(0.113)	(0.211)
243,864	1.379	1.346
	(0.142)	(0.193)
288,733	1.430	1.651
	(0.095)	(0.184)
379,839	1.387	1.322
	(0.100)	(0.146)
521,798	1.373	1.279
	(0.125)	(0.249)
686,754	1.341	1.358
	(0.110)	(0.184)
1,093,916	1.442	1.222
	(0.094)	(0.175)
2,485,581	1.334	1.198
	(0.101)	(0.173)

NOTE: These estimates come from one regression. The sample is all 284 CZs with 1998 employment of 50,000 or above. For each CZ, we have data on the dependent variable, the change in ln of employment, for 13 years, from 2003–2004 to 2015–2016. The demand shock data is for the current demand shock and up to five lags, from 1998–1999 to 2015–2016. All regressions include year dummies, and standard errors allow for clustering by CZ. The 284 CZs are ordered by 1998 employment and then grouped into 14 or 15 CZs by size. The multipliers shown are the immediate multiplier, after zero lags and the long-run multiplier after five lags.

Table 10 Multiplier with Interaction between Demand Shocks and ln(Employment in 1998), Unweighted and Weighted Regression Results

	Panel A (unweighted results)		Panel B (weighted results)	
Lag length for cumulative effects to the right (years)	Multiplier at means (standard error)	Interaction with ln(emp98) (standard error)	Multiplier at means (standard error)	Interaction with ln(emp98) (standard error)
0	1.327	0.046	1.573	0.026
	(0.069)	(0.018)	(0.083)	(0.012)
1	1.692	0.050	1.938	0.034
	(0.093)	(0.021)	(0.105)	(0.015)
2	1.789	0.027	2.116	0.009
	(0.101)	(0.024)	(0.122)	(0.021)
3	1.689	0.009	2.043	−0.011
	(0.105)	(0.028)	(0.142)	(0.018)
4	1.535	−0.004	1.927	−0.031
	(0.117)	(0.031)	(0.154)	(0.024)
5	1.383	−0.033	1.626	−0.065
	(0.111)	(0.037)	(0.141)	(0.028)
Mean of ln(emp98)		12.101		13.527
Standard deviation of ln(emp98) in sample		1.022		1.240

NOTE: These results come from two regressions, both with the sample of 284 CZs with employment exceeding 50,000 in 1998. One regression is unweighted, and the other uses employment in 1998 as a weight. Each regression's log growth dependent variable runs from 2003–2004 to 2015–2016. Both regressions include year dummies. All standard errors allow the error term to be clustered by CZ. Each regression looks at demand shocks to growth, with current demand shock and five lags in demand shock included. Demand shocks are interacted with deviation of ln(employment in 1998) from unweighted or weighted sample mean of ln(employment in 1998).

Table 11 Long-Run Multiplier Variation with ln(Employment in 1998), Quadratic Interaction

Long-run multiplier at means (standard error)	Interaction with ln(emp98) (standard error)	Interaction with ln(emp98) squared (standard error)
1.443	0.016	−0.057
(0.110)	(0.054)	(0.028)

NOTE: This is from a single unweighted regression with all CZs with more than 50,000 in employment in 1998. The demand shocks are interacted with deviations of ln(employment in 1998) from the unweighted mean and with those deviations squared.

Table 12 Long-Run Multiplier and CZ Size, Group Dummy Interactions versus Quadratic Interaction

Exp of mean ln(emp98)	Mean ln(emp98)	Unweighted multiplier with group dummies	Unweighted multiplier with quadratic interaction
52,818	10.875	1.107	1.337
57,815	10.965	1.253	1.351
63,257	11.055	1.475	1.363
69,867	11.154	1.319	1.376
75,324	11.230	1.795	1.385
81,802	11.312	1.346	1.394
91,083	11.420	1.589	1.405
99,389	11.507	1.221	1.413
111,348	11.620	1.451	1.422
132,515	11.794	1.409	1.432
154,372	11.947	1.448	1.439
178,237	12.091	1.455	1.443
204,704	12.229	1.474	1.444
243,003	12.401	1.346	1.443
288,589	12.573	1.651	1.438
376,582	12.839	1.322	1.424
520,419	13.162	1.279	1.396
682,119	13.433	1.358	1.363
1,076,376	13.889	1.222	1.289
2,269,432	14.635	1.198	1.117

NOTE: Estimated long-run multipliers are derived from Table 9 and Table 11.

Table 13 How the Short-Run (SR) and Long-Run (LR) Multiplier Varies with the Initial Prime-Age Employment Rate (ER)

		Multiplier estimates		
		Unweighted regression, interaction with both ln(emp98) and ln(prime-age ER)	Weighted regression, interaction with ln(emp98) and ln(prime-age ER)	Weighted regression, double interaction with ln(emp98) and ln(prime-age ER)
Multipliers at mean of ln(emp98) and ln(prime-age ER)	SR multiplier	1.338	1.570	1.600
	Standard error	(0.068)	(0.082)	(0.090)
	LR multiplier	1.479	1.635	1.768
	Standard error	(0.116)	(0.140)	(0.153)
Interaction with deviation of ln(emp98) from sample mean	SR multiplier effect	0.050	0.023	0.027
	Standard error	(0.018)	(0.013)	(0.016)
	LR multiplier effect	−0.022	−0.067	−0.039
	Standard error	(0.038)	(0.030)	(0.035)
Interaction with deviation of ln(prime-age ER) from sample mean	SR multiplier effect	−0.460	−0.289	−0.227
	Standard error	(0.315)	(0.412)	(0.451)
	LR multiplier effect	−1.518	−0.014	0.410
	Standard error	(0.672)	(0.802)	(0.866)
Interaction with product of deviation of ln(emp98) and deviation of ln(prime-age ER) from sample mean	SR multiplier effect			0.209
	Standard error			(0.221)
	LR multiplier effect			1.141
	Standard error			(0.473)
	Mean of ln(emp98)	12.101	13.527	13.527
	Standard deviation of ln(emp98)	1.022	1.240	1.240
	Mean of ln(prime-age ER)	−0.252	−0.249	−0.249
	Standard deviation of ln(prime-age ER)	0.064	0.053	0.053

NOTE: These results are from three separate regressions, each with same data from all 284 CZs with greater than 50,000 in employment in 1998. For each CZ, there are 13 observations on logarithmic annual job growth from 2003–2004 to 2015–2016. Growth is explained by six demand shocks: the current demand shock and five lags in demand shocks. The current demand shock effect (SR) and the cumulative effect after five lags (LR) are reported. The demand shock is allowed to have different multiplier effects depending on 1998 employment size and the year 2000 “prime-age” employment rate. The prime-age employment rate is the ratio of CZ employment to population for 25- to 54-year-olds. The interactions are between the demand shocks and the deviation of both the ln(employment in 1998) and ln(prime-age ER in 2000). One specification is unweighted. A second specification is weighted by CZ employment in 1998. A third specification is weighted and allows for a double interaction with both ln(employment in 1998) and ln(prime-age ER in 2000) interacted with each other.

Table 14 How Long-Run (LR) Multiplier is Affected by Lower Prime-Age Employment Rate (ER), Two Different Specifications

Employment equivalent to ln(employment)	ln(employment by group)	Effect of lower prime-age ER on LR multiplier, weighted double-interaction specification	Effect of lower prime-age ER on multiplier, unweighted single-interaction specification
52,818	10.875	2.616	1.518
57,815	10.965	2.512	1.518
63,257	11.055	2.410	1.518
69,867	11.154	2.296	1.518
75,324	11.230	2.211	1.518
81,802	11.312	2.116	1.518
91,083	11.420	1.994	1.518
99,389	11.507	1.894	1.518
111,348	11.620	1.765	1.518
132,515	11.794	1.566	1.518
154,372	11.947	1.392	1.518
178,237	12.091	1.228	1.518
204,704	12.229	1.070	1.518
243,003	12.401	0.874	1.518
288,589	12.573	0.678	1.518
376,582	12.839	0.374	1.518
520,419	13.162	0.005	1.518
682,119	13.433	−0.303	1.518
1,076,376	13.889	−0.824	1.518
2,269,432	14.635	−1.675	1.518

NOTE: Based largely on Table 13. This uses the double-interaction specification to calculate the derivative of the LR multiplier with respect to a one-unit reduction in the variable ln(prime-age ER) at various values of ln(employment in 1998).

Table 15 Classifications of High Tech

NAICS code	Industry name	2016 employment	Sci/eng occ % of employment	BLS classification	This study's cut-offs
5415	Computer Systems Design and Related Services	1,811,088	62.7%	1	
5417	Scientific Research and Development Services	732,558	50.3%	1	
5413	Architectural, Engineering, and Related Services	1,431,673	45.2%	1	
5112	Software Publishers	546,197	43.3%	1	
3341	Computer and Peripheral Equipment Manufacturing	42,656	39.3%	1	
3344	Semiconductor and Other Electronic Component Manufacturing	262,925	36.3%	1	
3346	Manufacturing and Reproducing Magnetic and Optical Media	9,696	36.3%	2	
5182	Data Processing, Hosting, and Related Services	462,389	34.6%	1	
5191	Other Information Services	283,516	32.8%	0	
3342	Communications Equipment Manufacturing	87,213	31.5%	1	
3343	Audio and Video Equipment Manufacturing	9,752	31.5%	2	
3345	Navigational, Measuring, Electromedical, and Control Instruments Mfg	374,250	30.5%	1	
3364	Aerospace Product and Parts Manufacturing	400,860	29.9%	1	
3254	Pharmaceutical and Medicine Manufacturing	247,267	27.2%	1	Tech 1: greater than 27.1%
1132	Forest Nurseries and Gathering of Forest Products	1,353	25.5%	2	
1131	Timber Tract Operations	4,180	25.5%	2	
5172	Wireless Telecommunications Carriers (except Satellite)	244,739	25.0%	3	
5174	Satellite Telecommunications	8,226	25.0%	3	
5179	Other Telecommunications	64,589	25.0%	1	
2111	Oil and Gas Extraction	122,143	22.2%	2	
3336	Engine, Turbine, and Power Transmission Equipment Manufacturing	95,969	21.5%	3	
5171	Wired Telecommunications Carriers	753,247	21.2%	3	
3259	Other Chemical Product and Preparation Manufacturing	80,446	19.9%	3	
3251	Basic Chemical Manufacturing	157,409	19.9%	2	
3391	Medical Equipment and Supplies Manufacturing	281,329	18.1%	0	
3333	Commercial and Service Industry Machinery Manufacturing	72,627	17.8%	2	
2211	Electric Power Generation, Transmission and Distribution	507,380	16.9%	2	Tech 2: greater than 16.9%
3353	Electrical Equipment Manufacturing	116,418	16.2%	3	
3351	Electric Lighting Equipment Manufacturing	43,470	16.2%	0	
3359	Other Electrical Equipment and Component Manufacturing	135,470	16.2%	0	
3253	Pesticide, Fertilizer, and Other Agricultural Chemical Manufacturing	29,654	15.5%	3	
3241	Petroleum and Coal Products Manufacturing	104,714	15.1%	3	
5511	Management of Companies and Enterprises	3,380,427	14.5%	3	
3255	Paint, Coating, and Adhesive Manufacturing	60,063	14.4%	3	
3331	Agriculture, Construction, and Mining Machinery Manufacturing	188,565	14.0%	0	
3365	Railroad Rolling Stock Manufacturing	29,021	13.5%	0	

Table 15 (Continued)

NAICS code	Industry name	2016 employment	Sci/eng occ % of employment	BLS classification	This study's cut-offs
3334	Ventilation, Heating, Air-Conditioning, and Commercial Refrigeration Equipment Manufacturing	126,800	13.5%	0	
3339	Other General Purpose Machinery Manufacturing	294,520	13.5%	3	
3332	Industrial Machinery Manufacturing	105,116	13.5%	2	
4862	Pipeline Transportation of Natural Gas	30,713	13.5%	3	
4869	Other Pipeline Transportation	8,753	13.5%	3	
4861	Pipeline Transportation of Crude Oil	14,197	13.5%	3	Tech 3: greater than 13.46%
2122	Metal Ore Mining	37,868	13.4%	0	
2212	Natural Gas Distribution	88,374	13.2%	0	
3256	Soap, Cleaning Compound, and Toilet Preparation Manufacturing	96,680	12.9%	0	
5416	Management, Scientific, and Technical Consulting Services	1,224,958	12.9%	2	
3361	Motor Vehicle Manufacturing	197,931	12.7%	0	
3363	Motor Vehicle Parts Manufacturing	560,248	12.7%	0	
3362	Motor Vehicle Body and Trailer Manufacturing	143,668	12.7%	0	
3366	Ship and Boat Building	140,182	12.6%	0	
3352	Household Appliance Manufacturing	48,912	12.3%	0	
2131	Support Activities for Mining	290,976	11.8%	0	Tech 4: greater than 11.7%
5152	Cable and Other Subscription Programming	49,708	11.2%	0	
5151	Radio and Television Broadcasting	217,300	11.2%	0	
4541	Electronic Shopping and Mail-Order Houses	434,744	10.4%	0	
3335	Metalworking Machinery Manufacturing	147,138	10.2%	0	
2213	Water, Sewage and Other Systems	43,101	10.2%	0	
8112	Electronic and Precision Equipment Repair and Maintenance	103,344	10.1%	3	
3369	Other Transportation Equipment Manufacturing	32,146	9.9%	3	
4234	Professional and Commercial Equipment and Supplies Merchant Wholesalers	654,377	9.6%	2	
6113	Colleges, Universities, and Professional Schools	1,876,666	9.2%	0	
6112	Junior Colleges	74,680	9.2%	0	
5231	Securities and Commodity Contracts Intermediation and Brokerage	410,465	9.2%	0	
5232	Securities and Commodity Exchanges	6,250	9.2%	3	
5259	Other Investment Pools and Funds	7,455	9.2%	0	
5239	Other Financial Investment Activities	490,128	9.2%	0	
3122	Tobacco Manufacturing	13,265	8.8%	0	
3111	Animal Food Manufacturing	53,224	8.4%	0	
3112	Grain and Oilseed Milling	55,744	8.4%	0	
4431	Electronics and Appliance Stores	378,635	8.3%	0	
3322	Cutlery and Handtool Manufacturing	36,317	8.2%	0	
3314	Nonferrous Metal (except Aluminum) Production and Processing	59,267	8.0%	0	

Table 15 (Continued)

NAICS code	Industry name	2016 employment	Sci/eng occ % of employment	BLS classification	This study's cut-offs
3252	Resin, Synthetic Rubber, and Artificial Synthetic Fibers and Filaments Manufacturing	95,234	8.0%	2	
5223	Activities Related to Credit Intermediation	287,086	8.0%	0	
5222	Nondepository Credit Intermediation	597,334	8.0%	0	
5242	Agencies, Brokerages, and Other Insurance Related Activities	990,756	7.9%	0	
5241	Insurance Carriers	1,574,266	7.9%	0	
4247	Petroleum and Petroleum Products Merchant Wholesalers	99,677	7.8%	0	
4236	Household Appliances and Electrical and Electronic Goods Merchant Wholesalers	550,186	7.7%	0	
5211	Monetary Authorities-Central Bank	19,365	7.6%	3	

NOTE: This table is based on ordering all 4-digit NAICS by percentage of employment in science, engineering, and technician occupations, based on 2012–2016 ACS data. However, this table only reports the 4-digit NAICS codes down to 7.6% in such occupations, as this encompasses all industries that BLS classifies as high tech using 2002 data. The BLS classification reports whether that industry is considered to be in BLS's Level 1, Level 2, or Level 3 definitions of high tech. These definitions, using 2002 data, are whether the industry has employment in tech occupations of five times the all-industry average (greater than 24.7% based on 2002 data), between three and five times the average (greater than 14.8%, but not in Level 1), and between two and three times the average (greater than 9.8% but not in Level 1 or 2). As can be seen, there have been some changes in industry ranks from the 2002 Occupational Employment Statistics data to the 2012–2016 ACS data, as well as a general increase in tech occupation employment. The update BLS cut-offs today are approximated, in the sense of roughly capturing the same industries, by cutoffs of 27.1%, 16.9%, and 13.46%. The updated BLS basic high-tech cut-off of twice the national average would be 11.7%, which includes more industries than the older BLS cut-off of 9.8%.

Table 16 National Trends in Employment in Tech Industries, 1998 to 2016

PANEL A: TOTAL EMPLOYMENT BY INDUSTRY CATEGORY

	Employment (millions)					
Year	Tech Tier 1	Tech Tier 2	Tech Tier 3	Tech Tier 4	Non-tech	Total
1998	5.250	2.634	4.025	2.288	85.508	99.704
1999	5.425	2.658	4.072	2.325	87.675	102.155
2000	5.545	2.825	4.154	2.403	90.087	105.014
2001	5.810	2.948	4.131	2.369	90.434	105.693
2002	5.249	2.760	3.993	2.228	88.883	103.112
2003	5.384	2.714	3.896	2.299	89.749	104.043
2004	5.334	2.564	3.799	2.342	91.095	105.134
2005	5.391	2.474	3.824	2.402	91.674	105.765
2006	5.602	2.402	3.894	2.467	94.259	108.625
2007	5.715	2.502	4.104	2.454	94.429	109.203
2008	5.789	2.431	3.885	2.342	94.955	109.402
2009	5.630	2.376	3.763	2.103	89.774	103.645
2010	5.523	2.265	3.645	2.076	87.777	101.286
2011	5.558	2.271	3.752	2.185	88.668	102.433
2012	5.659	2.231	3.926	2.440	90.486	104.742
2013	5.884	2.242	4.003	2.512	92.046	106.686
2014	6.057	2.217	4.125	2.650	93.857	108.906
2015	6.187	2.200	4.218	2.750	95.708	111.063
2016	6.324	2.224	4.263	2.792	97.286	112.889

PANEL B: PERCENTAGE OF TOTAL EMPLOYMENT BY INDUSTRY CATEGORY

Year	Tech Tier 1	Tech Tier 2	Tech Tier 3	Tech Tier 4	Non-tech
1998	5.27	2.64	4.04	2.30	85.76
1999	5.31	2.60	3.99	2.28	85.83
2000	5.28	2.69	3.96	2.29	85.79
2001	5.50	2.79	3.91	2.24	85.56
2002	5.09	2.68	3.87	2.16	86.20
2003	5.18	2.61	3.75	2.21	86.26
2004	5.07	2.44	3.61	2.23	86.65
2005	5.10	2.34	3.62	2.27	86.68
2006	5.16	2.21	3.58	2.27	86.77
2007	5.23	2.29	3.76	2.25	86.47
2008	5.29	2.22	3.55	2.14	86.79
2009	5.43	2.29	3.63	2.03	86.62
2010	5.45	2.24	3.60	2.05	86.66
2011	5.43	2.22	3.66	2.13	86.56
2012	5.40	2.13	3.75	2.33	86.39
2013	5.51	2.10	3.75	2.35	86.28
2014	5.56	2.04	3.79	2.43	86.18
2015	5.57	1.98	3.80	2.48	86.17
2016	5.60	1.97	3.78	2.47	86.18

NOTE: Based on the Upjohn Institute's WholeData, which is derived from County Business Patterns using an algorithm developed by Isserman and Westerveldt (2006). High-tech tiers are defined based on percentage of employment in high-tech occupations in each industry, based on the 2012–2016 ACS. Tier 1 is 27.1% or greater in high-tech occupations, Tier 2 is between 16.9% and 27.1%, Tier 3 is between 13.46% and 16.9%, and Tier 4 is between 11.7% and 13.46%. The employment reported is total employment in each industry Tier, not the industry's employment in high-tech occupations.

Table 17 High-Tech versus Low-Tech Multipliers Under Four Definitions of High Tech

	Lag length for the multipliers to the right (years)	Multiplier (standard error), unweighted				Multiplier (standard error), weighted
		Tech 1	Tech 2	Tech 3	Tech 4	Tech 4
Low tech	0	1.315	1.315	1.331	1.273	1.531
		(0.067)	(0.072)	(0.072)	(0.098)	(0.1550
	1	1.699	1.710	1.738	1.708	2.020
		(0.092)	(0.093)	(0.093)	(0.117)	(0.147)
	2	1.766	1.774	1.782	1.761	2.188
		(0.107)	(0.110)	(0.113)	(0.141)	(0.174)
	3	1.687	1.701	1.716	1.649	2.155
		(0.111)	(0.112)	(0.117)	(0.142)	(0.196)
	4	1.513	1.498	1.485	1.360	1.914
		(0.125)	(0.121)	(0.124)	(0.152)	(0.199)
	5	1.402	1.387	1.353	1.161	1.535
		(0.113)	(0.104)	(0.109)	(0.119)	(0.178)
High tech	0	1.159	1.181	1.121	1.402	1.570
		(0.366)	(0.284)	(0.236)	(0.087)	(0.163)
	1	1.224	1.369	1.319	1.718	1.767
		(0.510)	(0.470)	(0.393)	(0.197)	(0.214)
	2	1.638	1.635	1.633	1.934	2.052
		(0.487)	(0.440)	(0.371)	(0.185)	(0.244)
	3	1.322	1.387	1.410	1.857	1.935
		(0.525)	(0.486)	(0.415)	(0.212)	(0.255)
	4	1.213	1.426	1.541	1.933	2.006
		(0.593)	(0.560)	(0.484)	(0.245)	(0.302)
	5	1.002	1.279	1.503	1.970	1.920
		(0.661)	(0.620)	(0.526)	(0.254)	(0.308)
t-stat of high-tech differential long-run multiplier		−0.56	−0.14	0.31	2.64	0.96
	Mean of high-tech shock, 5th lag	0.0001	−0.0002	−0.0005	−0.0008	−0.0008
	SD of high-tech shock, 5th lag	0.0024	0.0030	0.0037	0.0054	0.0054
	SD of high-tech shock, 5th lag, differential from year means	0.0020	0.0023	0.0026	0.0040	0.0040
	Mean of low-tech shock, 5th lag	0.0022	0.0025	0.0028	0.0032	0.0032
	SD of low-tech shock, 5th lag	0.0224	0.0220	0.0214	0.0200	0.0200
	SD of low-tech shock, 5th lag, differential from year means	0.0072	0.0069	0.0067	0.0058	0.0058

NOTE: The estimated multipliers and standard errors are from five different regressions, one for each column. All estimates use data from 284 CZs, with the dependent variable, the annual change in ln(employment) for the CZ, ranging from 2003–2004 to 2015–2016. Among the unweighted specifications, the Tech 4 specification minimizes the sum of squared residuals and the AIC. The *t*-stat for the differential high-tech multiplier is from a separate set of five regressions that include overall growth shocks and high-tech growth shocks, and it looks at the significance of the cumulative high-tech growth effect in those five regressions.

Table 18 Long-run (LR) Multipliers, High-Tech and Manufacturing, Unweighted Regressions

PANEL A: EFFECTS OF MANUFACTURING VS. NON-MANUFACTURING SHOCKS		
Non-manufacturing	LR multiplier	1.468
	Standard error	(0.206)
Durable manufacturing	LR multiplier	1.478
	Standard error	(0.200)
Non-durable manufacturing	LR multiplier	1.036
	Standard error	(0.211)
PANEL B: DIFFERENTIAL EFFECTS OF MANUFACTURING SHOCKS AND HIGH-TECH SHOCKS FROM OVERALL SHOCKS		
Durable manufacturing differential multiplier	LR differential	−0.302
	Standard error	(0.305)
Non-durable manufacturing differential multiplier	LR differential	−0.210
	Standard error	(0.292)
High-tech differential multiplier	LR differential	0.840
	Standard error	(0.342)
Overall demand shock	LR multiplier	1.261
	Standard error	(0.179)

NOTE: These results are from two different regressions. Panel A shows regressions in which the overall demand shock is divided into three components: non-manufacturing, durable manufacturing, and non-durable manufacturing. Panel B shows regressions that include the overall demand shock, but also include three partial demand shock variables for just a smaller group of industries: durables, non-durables, and high-tech. The demand shocks on these smaller groups of industries in Panel B reflect differential effects of these smaller groups of industries, holding both the overall demand shock and the other shocks constant. The overall demand shock shows effects of the overall demand shock holding the industry-group demand shocks constant (e.g., the overall demand shock shows effects of demand shocks to industries that are NOT in manufacturing or high tech). The panel shows LR multipliers from specifications that include five lags in all demand shock variables. The sample is 284 CZs with 1998 employment greater than 50,000, with 13 years of growth observed, from 2003–2004 to 2015–2016.

Table 19 Long-Run (LR) Multipliers, High-Tech and Manufacturing, Weighted Regressions

PANEL A: EFFECTS OF MANUFACTURING VS. NON-MANUFACTURING SHOCKS		
Non-manufacturing	LR multiplier	1.696
	Standard error	(0.201)
Durable manufacturing	LR multiplier	1.859
	Standard error	(0.331)
Non-durable manufacturing	LR multiplier	1.136
	Standard error	(0.304)
PANEL B: DIFFERENTIAL EFFECTS OF MANUFACTURING SHOCKS AND HIGH-TECH SHOCKS FROM OVERALL SHOCKS		
Durable manufacturing differential multiplier	LR differential	0.065
	Standard error	(0.403)
Non-durable manufacturing differential multiplier	LR differential	−0.558
	Standard error	(0.350)
High-tech differential multiplier	LR differential	0.162
	Standard error	(0.401)
Overall demand shock	LR multiplier	1.658
	Standard error	(0.232)

NOTE: These results are from two different regressions. Panel A shows regressions in which the overall demand shock is divided into three components: non-manufacturing, durable manufacturing, and non-durable manufacturing. Panel B shows regressions that include the overall demand shock, but also include three partial demand shock variables for just a smaller group of industries: durables, non-durables, and high-tech. The demand shocks on these smaller groups of industries in Panel B reflect differential effects of these smaller groups of industries, holding both the overall demand shock and the other shocks constant. The overall demand shock shows effects of the overall demand shock holding the industry-group demand shocks constant (e.g., the overall demand shock shows effects of demand shocks to industries that are NOT in manufacturing or high tech). The panel shows LR multipliers from specifications that include five lags in all demand shock variables. The sample is 284 CZs with 1998 employment greater than 50,000, with 13 years of growth observed, from 2003–2004 to 2015–2016.

Table 20 High-Tech Share for Selected CZs, 1998

Central County of CZ	Description	Employment (1998)	Tech%	Rank (out of 284)
Santa Clara County, CA	San Jose/Salinas/Santa Cruz area (4 counties)	1,125,816	32.6%	1
Howard County, IN	Kokomo area (4 county area)	72,176	32.6%	2
Maury County, TN	Columbia/Lawrenceburg area (7 counties)	69,550	28.5%	3
Fairfax County, VA	DC area (17 county/city area)	1,904,878	21.0%	11
Wayne County, MI	Detroit-Oakland County area (7 counties)	1,928,965	20.1%	17
Dallas County, TX	Dallas area (10 counties)	1,661,730	19.9%	19
King County, WA	Seattle area (6 counties)	1,451,609	19.4%	22
Harris County, TX	Houston area (11 county area)	1,906,231	18.2%	27
Middlesex County, MA	Boston area (7 counties)	2,416,185	18.0%	31
St. Louis County, MO	St. Louis area (11 counties)	1,107,711	17.4%	37
Bergen County, NJ	Newark area (11 county area)	2,273,085	17.4%	38
Hennepin County, MN	Twin Cities area (12 counties)	1,523,900	17.3%	41
Denver County, CO	Denver/Boulder area (11 counties)	1,114,177	16.7%	50
Fairfield County, CT	Bridgeport/Hartford/New Haven area (8 counties)	1,491,784	16.6%	53
Alameda County, CA	San Francisco-Oakland-Napa-Sonoma (8 counties)	2,132,893	16.1%	60
Fulton County, GA	Atlanta area (17 county area)	1,780,671	16.0%	62
Cuyahoga County, OH	Cleveland/Akron area (7 counties)	1,238,794	15.6%	66
Cook County, IL	Chicago area (12 counties, up to SE WI, but not Indiana)	3,892,550	15.3%	70
Maricopa County, AZ	Phoenix area (3 counties)	1,287,519	14.8%	79
Philadelphia County, PA	Philadelphia area (6 counties)	1,834,084	14.3%	87
Los Angeles County, CA	Los Angeles area (6 counties)	5,953,232	13.3%	111
Allegheny County, PA	Pittsburgh area (7 counties)	1,021,541	13.0%	117
Kings County, NY	NYC + Long Island (7 counties)	4,097,947	9.6%	199
Miami-Dade County, FL	Miami/Ft. Lauderdale area (3 counties)	1,453,448	8.0%	235
Bowie County, TX	Texarkana area (5 counties)	56,515	4.3%	282
Lauderdale County, AL	Florence area (3 counties)	60,188	3.8%	283
Moore County, NC	Southern Pines/Rockingham area (3 counties)	51,164	3.5%	284

NOTE: This table reports selected statistics for CZs, out of 284 CZs in all states except Alaska and Hawaii that had greater than 50,000 in employment in 1998. These 284 CZs comprise 93% of total employment in these states as of 1998. The CZs included are those in top three and bottom three in ranking for percentage of employment in high-tech industries, and all CZs with greater than 1 million in employment in 1998. The unweighted average tech share is 12.4%; weighted average is 14.2%. High tech is defined using the Tech 4 definition (i.e., industries whose share of employment in science, engineering, and technician occupations is more than twice national average).

Table 21 Interaction of High-Tech and Low-Tech Multipliers with Local Size, Prime-Age Employment Rate, and Initial High-Tech Share, Unweighted Regression

		Multipliers at means	Interaction with ln(emp98)	Interaction with ln(prime-age ER)	Interaction with ln(high-tech share)
High tech	SR multiplier/interaction	1.253	0.170	−3.101	0.440
	Standard error	(0.156)	(0.091)	(1.739)	(0.191)
	LR multiplier/interaction	1.428	0.371	−9.169	1.963
	Standard error	(0.327)	(0.180)	(4.531)	(0.449)
Low tech	SR multiplier/interaction	1.291	0.060	0.307	−0.229
	Standard error	(0.098)	(0.026)	(0.442)	(0.071)
	LR multiplier/interaction	1.219	−0.004	−0.111	−0.532
	Standard error	(0.138)	(0.057)	(1.012)	(0.155)
Sample mean of interaction terms			12.101	−0.252	−2.158
Standard deviation of interaction term			1.022	0.064	0.388
Probability of *F*-test for interactions differing for high tech			0.232	0.230	0.000
Standard deviation times LR interaction for high tech			0.379	−0.586	0.762

NOTE: This table represents one regression. Demand shocks in high-tech and non-high-tech industries are interacted with differentials of 1998 size, 2000 prime-age employment rate (ER), and 1998 high-tech share from overall CZ average. The table reports short-run (SR, zero lag) multipliers and long-run (LR, cumulative after five lags) multipliers, and their interactions with these baseline CZ characteristics.

Table 22 Interaction of High-Tech and Low-Tech Multipliers with Local Size, Prime-Age Employment Rate, and Initial High-Tech Share, Weighted Regression

		Multipliers at means	Interaction with ln(emp98)	Interaction with ln(prime-age ER)	Interaction with ln(high-tech share)
High tech	SR multiplier/interaction	1.567	0.109	−3.352	0.495
	Standard error	(0.251)	(0.120)	(2.631)	(0.262)
	LR multiplier	1.513	0.285	−9.830	1.976
	Standard error	(0.525)	(0.240)	(5.619)	(0.592)
Low tech	SR multiplier/interaction	1.577	0.036	0.437	−0.236
	Standard error	(0.115)	(0.023)	(0.601)	(0.096)
	LR multiplier	1.542	−0.078	0.810	−0.304
	Standard error	(0.189)	(0.049)	(0.979)	(0.191)
Sample mean of interaction terms			13.527	−0.249	−1.998
Standard deviation of interaction term			1.240	0.053	0.323
Probability of F-test for interactions differing for high tech			0.282	0.178	0.000
Standard deviation times LR interaction for high-tech			0.353	-0.517	0.637

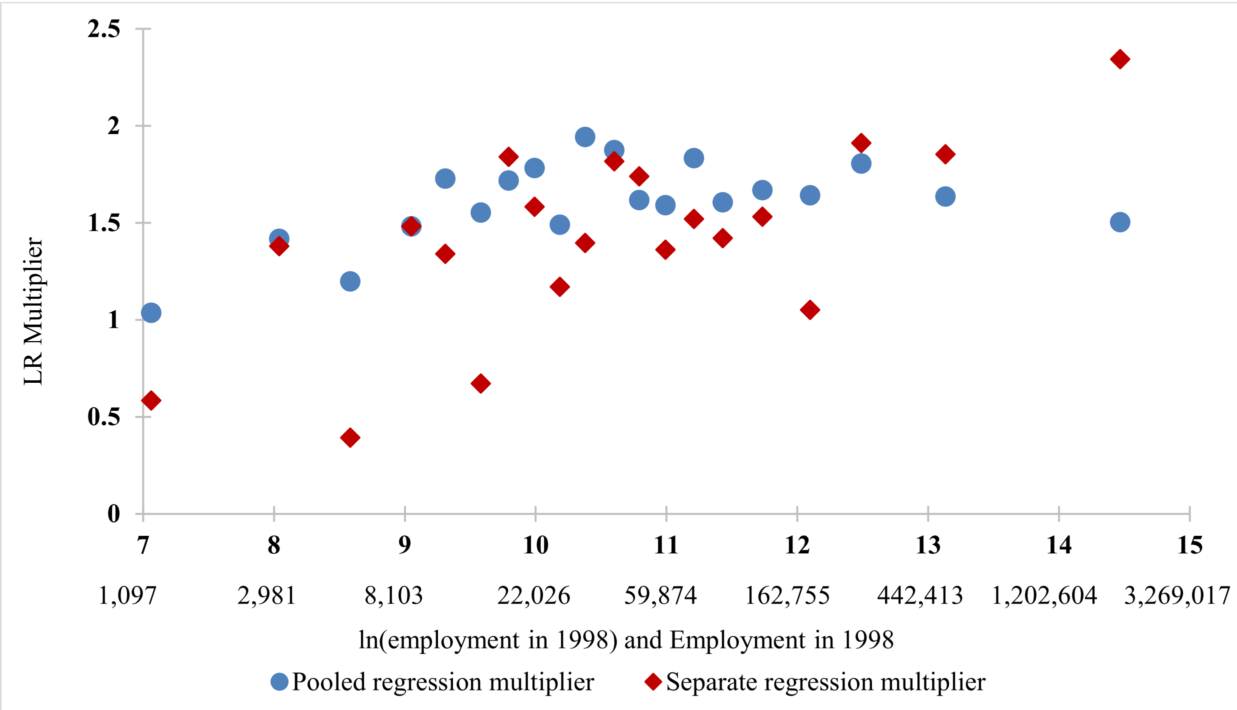
NOTE: This table represents one regression. Demand shocks in high-tech and non-high-tech industries are interacted with differentials of 1998 size, 2000 prime-age employment rate (ER), and 1998 high-tech share from overall CZ average. The table reports short-run (SR, zero lag) multipliers and long-run (LR, cumulative after five lags) multipliers, and their interactions with these baseline CZ characteristics.

Table 23 How Long-Run High-Tech and Low-Tech Multipliers Vary with Initial High-Tech Share, Various Functional Forms, Unweighted Regressions

1998	Linear functional form		Quadratic		20-group dummies		Linear functional form		Quadratic		20-group dummies	
High-tech share	High-tech multiplier	Standard error	High-tech multiplier	Standard error	High-tech multiplier	Standard error	Low-tech multiplier	Standard error	Low-tech multiplier	Standard error	Low-tech multiplier	Standard error
4.9%	−0.270	0.664	−1.957	0.887	−1.210	1.498	1.679	0.145	1.828	0.186	1.859	0.191
6.4%	0.257	0.552	−0.633	0.606	−2.888	2.014	1.536	0.129	1.587	0.135	1.777	0.271
7.4%	0.564	0.489	0.037	0.489	−1.357	1.731	1.453	0.125	1.464	0.126	1.455	0.160
8.0%	0.709	0.460	0.327	0.447	1.564	1.352	1.414	0.125	1.411	0.125	1.407	0.223
8.8%	0.889	0.424	0.665	0.404	2.457	0.868	1.365	0.126	1.348	0.127	1.068	0.340
9.4%	1.027	0.398	0.908	0.378	0.621	1.605	1.327	0.128	1.303	0.129	1.399	0.258
9.9%	1.117	0.381	1.057	0.365	2.509	1.606	1.303	0.129	1.275	0.131	1.049	0.304
10.4%	1.229	0.361	1.236	0.351	1.772	1.103	1.272	0.132	1.241	0.134	1.221	0.223
11.1%	1.341	0.341	1.403	0.339	1.144	1.046	1.242	0.135	1.210	0.137	1.100	0.315
11.5%	1.421	0.328	1.517	0.332	0.935	0.993	1.220	0.138	1.188	0.140	1.438	0.345
12.2%	1.528	0.311	1.661	0.324	3.368	1.803	1.192	0.142	1.160	0.143	1.168	0.209
12.8%	1.622	0.296	1.781	0.317	0.765	1.398	1.166	0.146	1.137	0.147	1.333	0.354
13.5%	1.739	0.280	1.920	0.309	1.571	0.630	1.134	0.151	1.110	0.152	0.921	0.240
14.2%	1.831	0.268	2.022	0.303	2.793	0.488	1.109	0.155	1.090	0.156	0.569	0.220
14.8%	1.919	0.258	2.113	0.297	2.671	0.612	1.086	0.159	1.072	0.161	1.215	0.258
15.8%	2.040	0.246	2.229	0.289	2.083	0.862	1.053	0.165	1.049	0.168	0.914	0.341
16.8%	2.158	0.236	2.331	0.280	3.402	1.522	1.021	0.171	1.028	0.177	0.903	0.307
17.6%	2.253	0.231	2.404	0.272	2.130	0.596	0.995	0.177	1.013	0.184	1.019	0.349
19.4%	2.440	0.226	2.529	0.255	2.453	0.611	0.944	0.187	0.986	0.202	1.046	0.288
23.8%	2.850	0.244	2.707	0.222	2.688	0.380	0.833	0.213	0.944	0.257	0.842	0.386

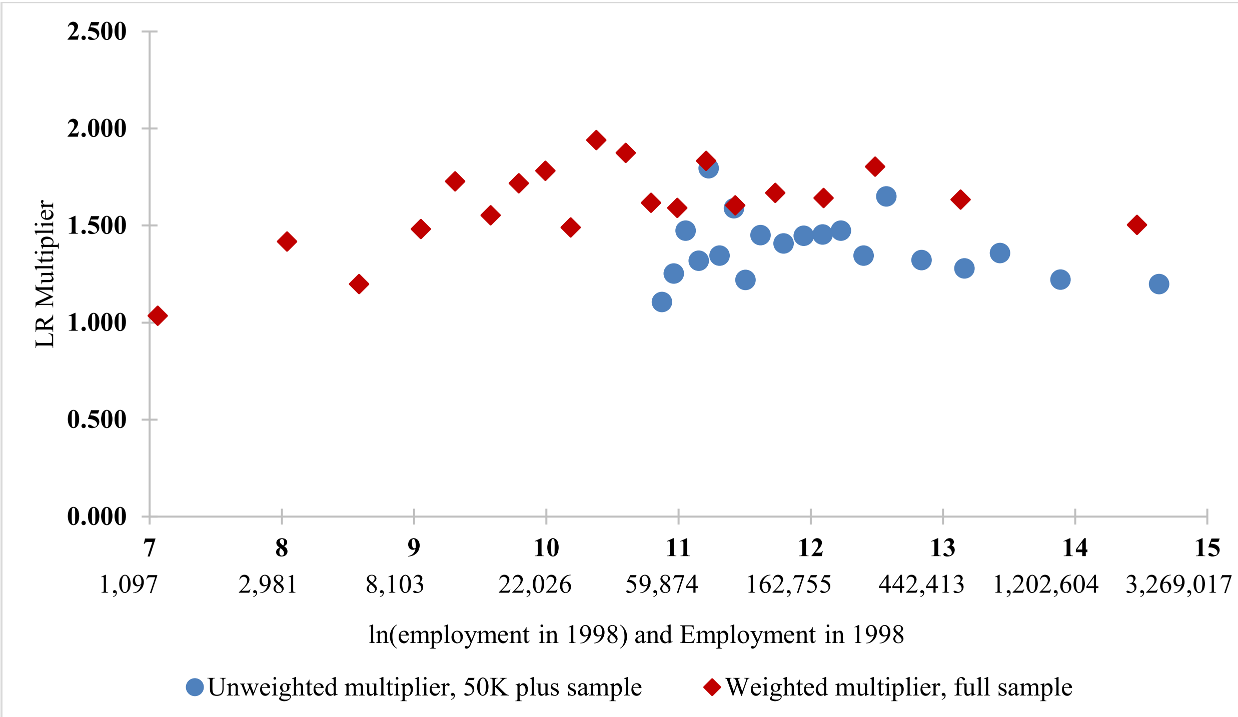
NOTE: These results come from three regressions. All regressions include year dummies and also interact the growth shocks with the CZ's employment size and prime-age employment rate. The three regressions differ in how they allow the effects of high-tech and low-tech growth shocks to vary with the 1998 CZ high-tech share. The linear specification interacts the growth shocks with ln(1998 high tech share). The quadratic adds in the square of this high-tech share as an interaction. The 20-group dummies add interactions with a dummy for each of the 20 groups, which sort the 284 CZs in the sample by the 1998 high-tech share and divide them into 20 groups of 14 or 15 CZs in each group. Effects are calculated at the ln(sample means) of each group.

Figure 1 Multipliers by Size Class of CZ, Pooled versus Separate Regressions



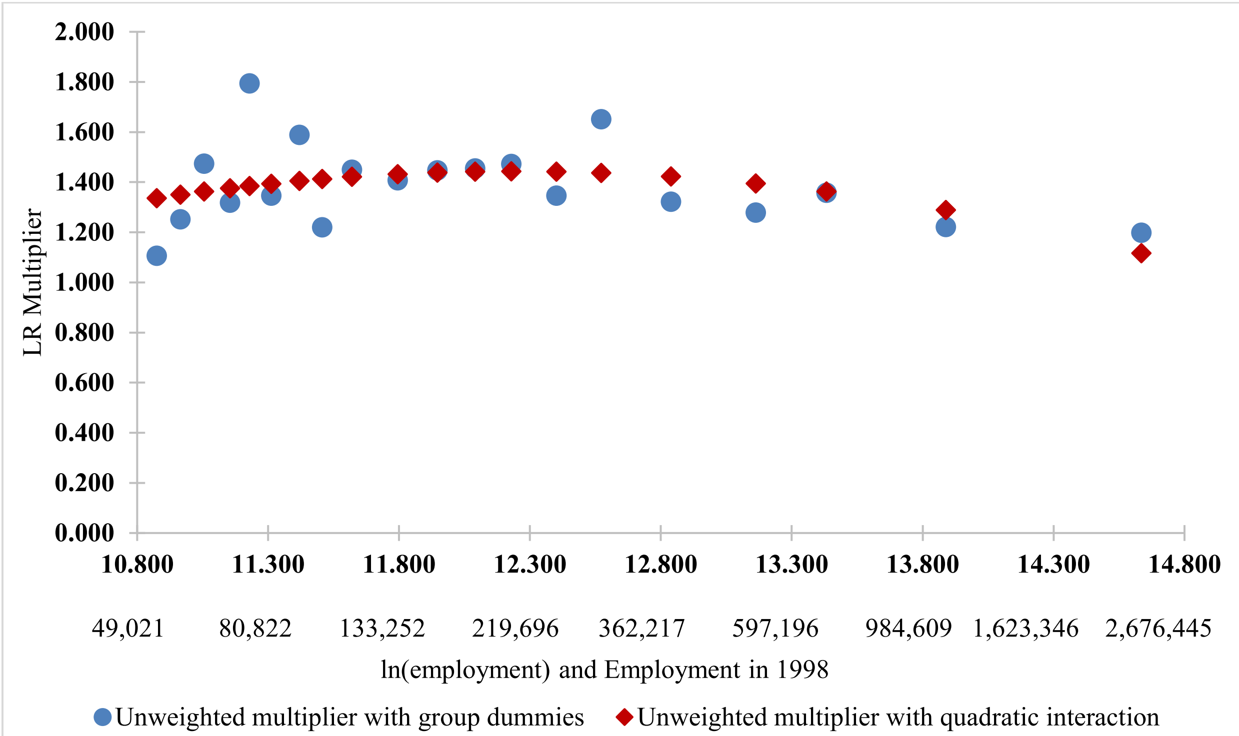
NOTE: Derived from estimates reported in Table 6. Estimates are shown based on logarithmic scale and are based on mean ln(employment in 1998) in CZ for each of 20 size classes. Horizontal axis also reports unlogged version of mean ln(employment).

Figure 2 Comparison of Long-Run (LR) Multiplier, Unweighted Regression with 50,000 or larger sample, Weighted Regression with Full Sample, by Size Group of CZs



NOTE: This shows the long-run multiplier when multiplier is allowed to vary by 20 size groups of CZs, in two different regressions: an unweighted regression with CZs 50,000 in employment and above in size, and a weighted regression with all CZs of whatever size.

Figure 3 Long-Run (LR) Multiplier and Commuting Zone Size, Group Dummy versus Quadratic Interaction



NOTE: Based on quadratic interaction reported in Table 11 and group dummy interaction reported in Table 9.

Figure 4 Effect of Prime-Age Employment Rate (ER) on Long-Run (LR) Multiplier, Variation with CZ Size

NOTE: This figure is derived from Table 13 and Table 14.

Figure 5 How High-Tech and Low-Tech Long-Run (LR) Multipliers Vary with Initial High-Tech Share, 20 Tech-Share Dummies Specification