Local Job Multipliers Revisited

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Abstract

There has been a recent surge in papers estimating local multiplier effects. However, existing studies rely on arbitrary periods of observation, limit samples to more populous regions, and commonly use relatively aggregated industrial categories. When we address these and other methodological issues, we find that, in the U.S., each new traded sector job adds half a nontraded job to a local economy, and that the addition of each high-tech job adds less than one job to the local nontraded sector. Furthermore, we find that the multiplier effect of the manufacturing sector is no higher than the multiplier effect of the average traded sector. We provide robust evidence that higher-paying traded sectors yield more non-traded jobs than lower paying sectors, and that multiplier effects are higher in larger cities. Furthermore, we generate IV estimates that remedy weak instrument problems in the existing multipliers literature. These findings offer needed clarity on the likely employment impacts of incentive policies aimed at attracting industries in the traded sector of the economy

Keywords: multipliers, cities, high-technology, local labor markets

JEL codes: F16, R15, R23

1. Introduction

Many local economic development policies aim to stimulate the growth of specific sectoral activities. Manufacturing firms have typically been the prized target of local governments, although high-tech activities have recently garnered a great deal of policymaker attention. While the use of tax incentives to attract specific industries has been widely criticized as inefficient and wasteful, among U.S. states the pecuniary value of such policies has more than tripled since 1990 (Bartik, 2017). Policymakers are willing to pay to host the next Amazon headquarters or BMW manufacturing plant, in part, based on the idea that local economies are interconnected systems, whereby an expansion in traded sector activities – those that primarily serve non-local markets – will raise local incomes, and thereby stimulate job growth in the local-serving, or nontraded, parts of the economy. The relationship between traded and nontraded jobs is measured by a multiplier effect, which estimates the number of nontraded jobs that are created in response to the addition of a job in the traded sector of the economy. This relationship is rooted in 'export-base theory,' a concept in urban economics with a long history (Haig, 1928; Andrews, 1953; Tiebout, 1956).

The methods and practice of export-base theory remain a cornerstone among economic development professionals, embodied in input-output and regional impact analyses. However, until recently, academic interest in the area had waned. The work of Moretti (2010) has revived research in this area. Applying a simple general-equilibrium framework, Moretti finds that the addition of a new manufacturing job in a U.S. city generates, on average, 1.6 new jobs in local nontraded activities. Meanwhile, jobs Moretti labels "high-technology" generate 4.9 additional jobs in the local services sector. Researchers, motivated by Moretti's framework, have produced a raft of estimates of multiplier effects, covering different time periods, sectors, and national contexts (Moretti and Thulin, 2013; Faggio and Overman, 2014; Moretti and Wilson, 2014; Van

Dijk 2017 and 2018; Goos et al. 2018, Lee and Clarke, 2019). Much of this work finds results that generally support Moretti's estimates. For instance, Goos et al., (2018) find that the addition of a new high-technology job in Europe adds 4.8 local nontraded jobs.

This paper finds that confidence in these estimates is limited by a series of empirical shortcomings. This paper aims to address these limitations to provide more robust estimates of multiplier effects in the United States. While each individual refinement made in this paper is modest, collectively they appear important. Our paper uses high quality data in which jobs are classified by place of work rather than residence. These data are available at finer industry granularity, enabling more effective delineation of the boundary between traded and nontraded activities. This allows a clearer identification of high-technology activities of the traded economy, relying on guidance from the BLS (Hecker, 2005). Unlike much of the existing work, we also estimate multipliers for nearly the entire population of urbanized areas in the U.S. Further, because we leverage a 28-year panel of cities, our estimates do not depend on arbitrary starting and ending years of analysis, which enables us to account better for the influence of the economic cycle. Like some related work, we shed light on the variation in multiplier effects across different sector of the economy. For example, similar to Goos et al., (2018), we estimate differences in multipliers across high-tech, manufacturing, and finance activities. Unlike these papers, we analyze these sectors in a 'horse-race' model that provides clearer identification of the relationships of interest. Testing theory, we also consider whether the size of multipliers varies according to wage differences among different traded sector activities. Finally, using a modified shift-share instrument, we generate IV estimates using techniques that remedy weak instruments problems that are widespread but unaddressed in the existing literature.

The findings in this paper suggest that many estimates of multiplier effects in the U.S. economy are inflated.[1] Our uninstrumented models suggest that each job added in the traded sector of the economy is associated with 0.51 new jobs in the nontraded sector for a given city. Our IV estimates generate weak-instrument-robust confidence sets with lower and upper bounds of 0.22 and 1.5 nontraded jobs for each new local traded job created, respectively. The midpoint of this range, 0.86 nontraded jobs added per traded job, is around half of Moretti's IV estimate of 1.6. Meanwhile, we find that each high-tech job added to a local economy is associated with 0.78 additional nontraded jobs, with IV confidence sets ranging between 0.46 and 2.65 (midpoint 1.55), which contrasts with Moretti's point estimate of 4.9. While we find a larger multiplier effect for the high-tech sector than for manufacturing industries, the effect is approximately one sixth of those estimated in Moretti (2010) and Goos et al. (2018). We additionally find that, in keeping with theory, traded jobs that pay higher wages generate larger multiplier effects, and that the largest multipliers are produced in the most populous regional economies.

The remainder of this paper is organized as follows: Section 2 reviews the literature on multipliers; Section 3 describes our data and empirical approach; Section 4 presents results; and Section 5 concludes.

2. Conceptual Framework and Literature Review

As in Moretti (2010), and most other recent studies (i.e. Moretti and Thulin, 2013; Faggio and Overman, 2014; Moretti and Wilson, 2014; Gerolimetto and Magrini, 2016; Van Dijk 2017 and 2018; Goos et al. 2018), we consider multiplier effects within a general-equilibrium framework. In this framework, it is assumed that there are two types of goods – traded and nontraded – and

[1] This is a theme consonant with a recent working paper by Bartik and Sotherland (2019), which uses a different approach.

that traded goods flow between regions within a national economy. In a competitive economy, the national market determines the price of traded goods, while the price of nontraded goods is determined locally. Labor is mobile across regions and sectors of the economy, and the local labor supply is elastic, as is the local supply of housing. Labor supply elasticity is a function of worker mobility between locations. Land use regulations and the particularities of local geography, such as the availability of developable land, determine the responsiveness of the local housing supply.

Assume that the number of traded jobs within a given location grow. Concretely, let us say there is an expansion of local biotechnology jobs. This could be accomplished by attracting a new plant, lab or office, or from expansion by existing employers in response to increasing national demand for biotech goods and services. We assume this leads to net job creation in the local biotech sector. In this paper we seek to estimate the relationship between the growth of jobs in this traded sector and employment in the local nontraded sector – the dry cleaners, restaurants, barbers, bookkeepers and other industries that serve the needs of biotech workers as well as the rest of the local population. Net employment growth in traded activities like biotech will result in an increase in regional incomes, raising demand for nontraded services. This generates a multiplier effect – the resulting increase in nontraded jobs. As Moretti and Thulin (2013) describe, the size of a multiplier is contingent upon several factors, such as the preferences of workers in traded activities as well as their wages. General equilibrium effects on prices and wages also matter. For instance, in locations with more constrained housing supply, growing demand for housing will lead to greater increases in the price of housing, thereby eroding effective incomes.[2]

[2] Employment growth in a particular traded activity can also affect employment growth in the rest of the traded sector in a given city. The increase in labor costs can reduce the competitiveness of other traded sectors, which could lower employment elsewhere in the local traded sector. Higher demand for some traded work can also increase demand for upstream suppliers depending on the spatial extent of the expanding sector's supply chain. And an increase in traded work could generate positive urbanization effects that could generate benefits for other parts of the traded sector. While some existing work examines such relationships (i.e. Moretti, 2010), this paper is narrowly focused on the links

For the most part, this framework has been applied to measuring multiplier effects across U.S. and European cities. Existing studies have mainly focused on the relationship between the traded and nontraded sectors of the economy, although some studies measure the multiplier effect of specific sectors within the traded economy, such as manufacturing and high-technology activities, as well as high and low-skilled traded jobs (Moretti, 2010; Moretti and Thulin, 2013; Moretti and Wilson, 2014; Fernandez, 2014; Gerolimetto and Magrini, 2016; Nguyen and Soh, 2017; Van Dijk 2017 and 2018; Goos et al. 2018; Lee and Clarke, 2019; Bartik and Sotherland, 2019). Related work has also considered the local effects of expansions in specific resource-intensive sectors, including coal (Black et al, 2005), and oil and gas (Marchand, 2012, Weinstein et al, 2018). Other studies have estimated the multiplier effect of public sector jobs (Faggio and Overman, 2014; Senftleben-König, 2014; Becker et al. 2018).

Since Moretti (2010), there is near-consensus around the methodology for calculating multiplier effects.[3] For a given time period, differences in the log of traded and nontraded employment are used to calculate the elasticity of nontraded employment with respect to traded employment (Moretti, 2010; Moretti and Thulin, 2013). In the U.S. context, estimated elasticities cluster around Moretti's (2010) estimates of between 0.33 and 0.55 (Van Dijk 2017; Gerolimetto and Magrini, 2016). In other words, when an exogenous shock leads to a 1% increase in the number of jobs in the traded sector of a given city, this is associated with between a 0.33% and 0.55% increase in the number of jobs in the nontraded sector of the economy. Similar multiplier effects have been found in Europe. Gerolimetto and Magrini (2014) estimate an elasticity of 0.5 for

between traded and nontraded employment. This study also ignores possible wage effects on nontradables – a topic analyzed in the U.S. in Kemeny and Osman (2018) and in Britain by Lee and Clarke (2019).

[3] This consensus has been challenged by a recent working paper by Bartik and Sotherland (2019), who follow a different approach, though they share with this paper a variety of other amendments, such as higher-frequency, detailed industrial data and a fuller range of regional economies. The present paper can be read against Bartik and Sotherland (2019) as indicating that radical changes to the estimation approach are not necessary to obtain substantially smaller multiplier estimates.

Spanish cities, while Moretti and Thulin (2013) estimate a range of 0.22 and 0.49 for Swedish cities. These elasticities are then typically translated into the number of nontraded jobs associated with the addition of one traded sector job, by multiplying the estimated elasticity by the ratio of nontraded to traded sector employment. For example, according to Moretti's (2010) work, there are 4.8 nontraded jobs for every traded sector job. If this figure is multiplied by the estimated elasticity of 0.33, this yields a figure of 1.59 nontraded jobs each time a traded sector job is added to a local economy.

Employing a slightly different approach from Moretti, Van Dijk's (2018) preferred estimates range from 0.17 to 0.88 nontraded jobs created for each traded sector job added to an economy. While this finding suggests, quite plausibly, that multipliers are considerably more diminutive than other studies have found, the estimated range is very wide - five times greater at the higher end than the lower end of the range, while the results may still be sensitive to idiosyncratic start and end years and other issues in common with Moretti (2010), such as overly aggregated industrial delineations.

Table 1 summarizes estimates from other multiplier studies of the number of nontraded jobs added in response to the addition of one traded job, as well as multiplier estimates for the manufacturing and high-tech sectors. For the traded sector of the economy, estimates range from a low of 0.17 (Van Dijk 2017, 2018) to a high of 1.6.[4] According to estimates in European economies, one traded job is associated with the addition of between 0.5 and 2.1 nontraded jobs.

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[4] Gerolimetto and Magrini (2016) represent an exception. Although the authors estimate an elasticity of nontraded to traded employment of 0.62, they conclude that the addition of one traded sector job generates only 0.53 nontraded jobs. Behind this unusually low multiplier appears to be an error: the authors calculate that there are 0.86 nontraded job for every traded job. This ratio sharply contrasts with widely accepted measures that there are many more local services jobs than there are traded industry jobs (Jensen and Kletzer, 2005; Moretti, 2010; Van Dijk, 2018; Kemeny and Osman, 2018). On this basis their multiplier (though not their elasticity) does not seem directly comparable.

[TABLE 1 AROUND HERE]

Meanwhile, existing estimates of multipliers for high-technology and skill-intensive sectors appear to be larger. This is logical, given that the creation of a more highly remunerated position in a local traded sector ought to raise demand for local services more than a less-well paid traded job. Moretti (2010) finds that the addition of one high-tech job creates 4.9 jobs in the local nontraded sector of the economy, while Goos et al. (2018) estimate a comparable figure of 4.8 for European cities. Moretti (2010) finds that traded jobs filled by workers with at least some college are associated with 2.5 times more nontraded jobs than employees without such qualifications. Taken at face value, these estimates for the high-tech sector, while widely cited, generate some cause for skepticism. To assume that five new local service sector employees are required to service demand each time Apple or Amazon adds a new employee, would be to assume that increased demand for local services can only be met with the addition of more employees, rather than increased hours for, or productivity increases from, existing employees, for example. Furthermore, if this estimate is accurate, we would expect, in the long-run, there to be a greater ratio of nontraded to traded workers in regions with high proportions of high-tech workers, compared to regions with relatively fewer tech workers. In fact, the opposite is true. Anecdotally, the ratio of nontraded to traded workers is lower in tech-dominated regions like San Jose (on average 1.5 nontraded to each traded job), Seattle (2.5 nontraded to 1 traded) and San Francisco (3.1 to 1) than in places like Jacksonville, North Carolina (32 to 1) and Flagstaff, Arizona (22 to 1). The reasons for this are complex – likely a combination of local services workers becoming priced out of high-tech and high growth regions, and greater efficiencies in nontraded sectors in regions with more tech jobs, perhaps through capital substitution or through other means.

As discussed at the outset, there are a number of ways that bias can be introduced into multiplier studies. The first relates to the choice of data. When studying the United States, researchers have largely relied upon public-use extracts of the Decennial Census and American Community Survey, which identify workers by place of residence rather than by place of work (Moretti, 2010: Van Dijk, 2017). These data are also known to suffer problems due to the incomplete identification of metropolitan areas, meaning that some proportion of a region's population lies in zones that cannot be neatly assigned to a given metropolitan area. Sampled individuals who lie in such zones, which lie on the outer edges of metropolitan areas, are unassigned to a given metropolitan area.[5] This issue is relatively widespread in the years analyzed in typical U.S.-focused studies (for instance, Moretti, 2010, 2014; Van Dijk, 2017; 2018).[6] Since it is reasonable to assume that unidentified residents, because of their particular geography, vary nonrandomly from those who are identified, their exclusion likely introduces bias into the estimates.

Second, most studies rely on a small sample of start- and end-years which are selected based on convenience (such as census years) rather than to be representative of trends in the economy. Such sampling is not well-suited to controlling for unobserved shocks to the economy. Using the standard estimation approach, Nguyen and Soh (2017) is the only known paper to have considered this explicitly. Armed with county-level annual data from County Business Patterns, they conclude that multipliers grow considerably larger during recession years, for example.[7]

[5] For a detailed discussion of incompletely identified areas in recent Census data, see https://usa.ipums.org/usa/volii/incompmetareas.shtml

[6] In the 1980 five percent sample, roughly one in three metropolitan areas are incompletely identified with an average of 20% residing in undesignated areas. Similarly, in the 1990 five percent sample, 108 metropolitan areas suffer from this issue, with a mean unidentified of 22%.

[7] Bartik and Sotherland (2019) also consider higher-frequency data, though they also use a different estimation strategy.

Third, multiplier estimates are sensitive to how industries are defined. In seeking to distinguish between traded and nontraded activities, Moretti (2010) and Gerolimetto and Magrini (2016) rely on 2-digit categories of the North American Industrial Classification System (NAICS), the most aggregated level of industrial classification. From this starting point, Moretti (2010) defines the traded sector to include manufacturing industries only (NAICS 31-33), and counts all other industries as part of the nontraded economy, except for agriculture, mining and government and military, which are dropped from his analysis. One outcome of this procedure is that a whole range of activities that we commonly regard as being traded are defined as nontraded. For example, NAICS code 51 – Information – is defined in Moretti (2010) as a nontraded activity, but this sector is comprised of activities like the motion picture industry, internet and software development, and data processing centers, all of which primarily serve non-local markets. This creates bias in several ways, not least because mislabeled sectors that should be on the right side of an estimating equation are not simply removed – they appear instead on the left side of the equation. This problem cannot be addressed merely by switching such two-digit categories to the traded side of the ledger, because at this level of granularity, categories combine both traded and untraded activities. For example, Finance and Insurance, 52, includes securities trading firms, which serve non-local markets, as well as local-serving retail banks.

Van Dijk (2018) takes a more defensible approach, using 3-digit NAICS industries and defining the traded and nontraded sectors using a locational Gini coefficient approach outlined by Krugman (1991) and Jenson and Kletzer (2005). While this represents an improvement, the use of 3-digit codes still provides a crude basis for distinguishing between traded and nontraded sectors. The 3-digit Finance and Insurance category, for example, contains a mix of traded and nontraded activities: 5221 Depository Credit Intermediation, which describes local banks and credit unions, and 5222 Nondepository Credit Intermediation, which issues credit cards and is likely to be a traded industry. While no level of detail short of the study of the markets of individual establishments will be perfect, further detail is available, and it is to be preferred to less.

These definitional challenges can be especially pronounced for the analysis of specific sectors of the economy. Moretti's oft-repeated claim that nearly 5 nontraded jobs are created for each high-tech job added to an economy relies on a definition of high-technology that consists of just two manufacturing sectors: Machinery and Computing Equipment, and Electrical Machinery and Professional Equipment. In addition to the potential problems described above, Moretti's (2010) inflated multiplier for the high-tech sector should be interpreted with caution since it greatly undercounts the extent of the high-tech sector – it includes no measures of high-tech services like software that have been major contributors to the U.S. economy (Galbraith and Hale, 2004), while privileging manufacturing activities that have been in decline as a source of employment in the U.S. for decades. Since Moretti relies on such a constrained definition of high-tech, this inflates the ratio between non-traded and high-tech jobs, and therefore biases his estimate upwards.

Fourth, multiplier estimates are vulnerable to location-specific omitted variables. One potential source of the latter arises in estimates of specific traded subsectors like tech, as studies like Moretti (2010) and Goos et al. (2018) do not control for changes that occur in the remainder of the traded sector. Interpretation in such cases is challenging since changes in these unobserved sectors may be correlated with changes in the traded sector of interest. To overcome another endogeneity concern, most studies include an instrumental variable, which seeks to isolate the exogenous component of city's a shift in the traded sector of the economy. Bartik's (1991) shift-share measure is the most used instrument, which predicts labor demand in a local traded sector on the basis of the national growth trajectory of the industry. A raft of recent papers highlight a variety of potential challenges to the incautious application of such instruments (Jaeger et al., 2018; Boxterman and Larson, 2018; Goldsmith-Pinkham et al., 2020; Borusyak et al., 2018), ranging from the need for substantial variability, to assumptions around linearity and the absence of interactions. All this suggests the need for care in attributing causal impacts.[8]

Fifth, because many studies limit analysis to only the largest cities in the national system, there may be underexplored issues of generalizability. Moretti (2010) and Van Dijk (2017, 2018) sample the largest 200 metropolitan regions in the U.S., while Gerolimetto and Magrini (2016) sample only the largest 123 cities. While the inclusion of city-population controls in Van Dijk (2017, 2018) helps to account for size differences among in-sample locations, it fails to capture the possibility that the relationship of interest in the excluded smaller regional economies might systematically differ from those obtained from larger metropolitan areas. While smaller regions might contain a relatively modest proportion of the national population, they remain objects of intellectual as well as policy interest.

3. Methods and Data

In this study we estimate multiplier effects using the following equation:

$$ln\left(e_{jt}^{NT}\right) = \beta_0 + \beta_1 ln(e_{jt}^{T}) + \mu_j + \eta_t + \nu_{jt} \tag{1}$$

[8] In Goldsmith-Pinkham et al. (2020) for instance, an identifying assumption is that lags of industry shares are uncorrelated with all unobserved local shocks. As Borusyak et al. (2018) argue, however, in many cases in which Bartik instruments are conceptualized as leveraging exogenous exposure shares, this may not be satisfied, rendering the instrument insufficient to accurately uncover causal estimates.

where *ln*(*e)* describes the log of employment in metropolitan area *j* and time *t*, and the superscripts *NT* and *T* denote the nontraded and traded sectors of the economy, respectively. μ_j is a city-specific fixed effect, to account for bias from unobserved but relatively constant features of each metropolitan region, while η_t is a year fixed effect, to capture time-varying but economy-wide shocks to the economy. The standard random error term is represented by ν_{jt}. The key parameter to be estimated is β_1, which measures the relationship between the traded and nontraded sector of the economy. On the assumption that our repeated observations on cities are not independent and identically-distributed, we cluster standard errors at the city level.

While similar in spirit, this approach differs in some respects from much of the existing empirical literature. With few exceptions, prior work has estimated the relationship between traded and nontraded employment over two relatively distant (often decadal) periods. Our approach, enabled by higher-frequency data, offers concrete advantages. First, our results are not as vulnerable to the arbitrariness of the start and end years. Second, higher frequency data means we should be better able to distinguish signal from noise in the relationship of interest. Third, it permits the inclusion of location-specific fixed effects. As indicated in Table 1, such fixed effects are missing from a number of existing studies because of the limitations imposed by the use of two cross-sections. Given the high-frequency approach taken in this study, results are best understood as capturing the short-run relationship between traded and nontraded jobs.

To address potential endogeneity concerns, we follow existing studies and employ a Bartik-style shift-share instrument for traded employment in two-stage least squares fixed effects estimates. While the shift-share measure has drawbacks, as discussed above, it provides advantages in the current study because it is a time-varying instrument, which are exceedingly hard to find. The purpose of the instrument is to capture the exogenous component of traded industry growth in a given region, by substituting observed industry growth in a given region with the national growth rate for a given industry. Our Bartik instrument, Z_{jt}^{T}, is constructed as follows:

$$Z_{jt}^{T} = \sum_{i=1}^{T} \frac{e_{ijt-1}}{e_{jt-1}} \left[\frac{\left(E_{it} - e_{ijt}\right) - \left(E_{it-1} - e_{ijt-1}\right)}{\left(E_{ijt-1} - e_{ijt-1}\right)} \right] \tag{2}$$

where e_{ijt} represents local employment in traded subsector i, in region j and E_{it} captures national employment levels for industry i. The first term of equation (2) represents a given industry's share or total employment at time *t-1.* The second term measures national industry growth absent the influence of growth in a given region. Each region therefore has a unique 'national' growth rate, which is purged of its own contribution to that growth rate. This 'leave-one-out' construction – absent in Moretti (2010) but present in papers like Faggio and Overman (2014) and Van Dijk (2018), and recommended by Goldsmith-Pinkham et al (2020) – aims to eliminate the lingering concern that national growth rates might be driven by local employment dynamics. This could be an issue in sectors that are highly spatially concentrated.

Formal testing has found the shift-share instrument to be weak in a range of multiplier applications, with some studies reporting first-stage *F*-statistics that are very close to, and at times beneath conventional cutoffs for weak instruments.[9] Given that estimates of coefficients and standard errors made with weak instruments are likely to be biased, for selected models below, we estimate the AR (Anderson-Rubin) confidence sets. In the presence of weak instruments in the just-identified case with a single endogenous regressor and non-i.i.d errors, an AR test generates

[9] For instance, Van Dijk (2018) reports *F*-statistics on excluded instruments in first stage models that vary considerably from one model to another, though a good number are below 15. It is not clear whether these are effective (i.e. Montiel-Pflueger), Kleibergen-Paap, or Cragg-Donald, the latter two of which are inappropriate in the non-homoscedastic case. Meanwhile, Moretti (2010) appears to report first-stage *F* statistics that would fail weak instrument tests of any kind. In short: it is likely that weak instruments are a problem hindering inference in at least some estimates.

unbiased confidence sets describing the range of parameter values of the relationship of interest that are consistent with the data (Moreira, 2009; Mikusheva, 2010). To our knowledge, such approaches have not been used in other multiplier studies.

The Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW) is the primary data source used in this study. Each quarter, private employers must submit a contributions report to the state in which they are located, which is the basis for the unemployment taxes they pay to fund the Federal-State Unemployment Insurance program. For each establishment, the report identifies the number of workers they employ and the wages they earn. The BLS verifies these data for accuracy and releases a quarterly aggregation by location. QCEW provides employment and establishment counts, as well as wages, by county and state. This study relies on annual data from 1990-2017, since the Bureau of Labor Statistics has coded these data using consistent industrial definitions (North American Industrial Classification System (NAICS))[10]. This avoids introducing noise into the data which would occur from converting data across different classification systems. QCEW are high-quality data that identify workers by place of work. Some QCEW data are restricted for reasons of confidentiality – to avoid the data being used to identify information about individual employers or workers. However, since the analysis in this paper is confined to metropolitan regions, which are home to high concentrations of employment, confidentiality does not greatly restrict data availability. We estimate that the data used in this study covers more than 90% of all employees for our regions of interest.

Multipliers are estimated at the scale of the Metropolitan Core-Based Statistical Areas (CBSAs).[11] Metropolitan CBSAs are defined by the Office of Management and Budget (OMB) as contiguous counties with a combined population of at least 50,000 people that have a high

[10] In prior data, industries are defined according to the Standard Industrial Classification (SIC) system
[11] In this paper, the terms metropolitan region and city are used interchangeably.

degree of economic and social interaction. These counties together form integrated and quasi-independent regional labor and housing markets. By using this scale as opposed to individual counties (c.f. Nguyen and Soh, 2017), we are better positioned to observe our relationship of interest at the scale at which it likely operates. In narrow statistical terms, it means we can reduce bias related to the modifiable areal unit problem (Fotheringham and Wong, 1991). These data also mean that, unlike approaches dependent upon public-use Census extracts, our estimates are not subject to bias from incompletely identified metropolitan areas.

To distinguish between traded and nontraded activities, we follow the approach of Kemeny and Osman (2018), who distinguish tradability by relying on Gini coefficients for 4-digit NAICS codes. Gini coefficients are calculated based on the distribution of employment among metropolitan regions in 2017. Industries for which employment is more concentrated than the spatial distribution of all employment are assumed to primarily serve non-local markets, and will have higher Gini coefficients. Those activities that confirm to the general dispersion of all employment are assumed to produce goods and services for local consumption, and will present lower Gini coefficients. At the tails of the distribution, industries are easily separated between our two primary categories: traded and nontraded. For the middle of the distribution, we use industry descriptions as a means to manually assign industries to one of these two categories. Since it consists of intermediate activities that cannot be clearly identified as traded or nontraded, we classified wholesale sectors into a third category which is removed from the analysis. Using this approach, we estimate that the average location contains 4.63 nontraded jobs for every traded sector job.

This paper also considers specific subsets of traded activity. Specifically, we estimate multipliers related to employment changes in the manufacturing, high-tech and the traded financial services sectors of the economy. To define the high-technology sector we follow the strictest definition in Hecker (2005), which is based on analysis from the Bureau of Labor Statistics. The BLS defines the high-tech sector according to four primary factors: first, the intensity of scientific, technical and engineering occupations that comprise an industry; second, the level of employment in Research & Development activities; third, an industry's output, such as whether it produces advanced-technology products; finally, the use of high-technology in the production process. The manufacturing sector is defined to include all 4-digit activities that fall within the NAICS 31-33 categories. Each subsector of the high-tech and manufacturing industries, as defined here, is considered to part of the wider set of traded activities, regardless of its Gini coefficient. Traded financial services are all 4-digit codes that begin with codes 521-523 that are deemed traded on the basis of their spatial concentration. For example, NAICS code 5232, 'Securities and Commodity Exchanges,' has a very high Gini coefficient of 0.41, and so is considered as part of the traded sector of the finance sector. By contrast, NAICS code 5223, 'Activities Related To Credit Intermediation,' has a Gini coefficient of 0.007, and so is not considered as part of the traded finance sector.

On the basis that higher paying jobs should generate greater multipliers, we also consider the impact of variation in wages among traded activities. To do this, we group 4-digit traded industries into terciles based on the national annual average wage a sector pays.[12] For each region, we then calculate total employment across each of these terciles.

A final modelling consideration concerns the use of controls. While controls are not universally applied in multiplier studies, there are notable exceptions (i.e., Faggio and Overman,

[12] To consider why we opted against using local wage distributions to create groups, consider the case of high-cost places like San Francisco and New York City. In these regions, relatively low-paying traded employment has been priced out, leaving nearly uniformly high wage activities. Quantiles of the local traded wage distribution will thus not be informative about the potential moderating effects of wages on multipliers.

2014; Van Dijk 2018; Lee and Clark, 2019). The key consideration should be the extent to which an observed relationship between the traded and nontraded sectors might be subject to omitted variable bias. When researchers have included controls in their models, these are most commonly measures of the unemployment rate, and of the share of residents in a region who hold a bachelor's degree (Faggio and Overman, 2014; Van Dijk 2018; Lee and Clark, 2019). The unemployment rate could affect local multipliers by reducing the stock of workers that would be willing to respond to rising demand for nontradables. However, assuming some degree of mobility among locations, it is likely to be the national unemployment rate that drives this effect, not local conditions. Though several papers include measures that capture endowments of college graduates, the mechanism linking such variation to multipliers remains largely unarticulated. The share of graduates should be highly correlated with the nature of the traded sector from one place to another, such as the extent to which a region is home to high-technology activities. In such cases, therefore, education should not have an effect independent of the local traded sector. In other words, for a wide range of traded activities – or at least those that have been the focus of much of the literature, the local traded sector should determine the share of workers who hold a bachelor's degree from one region to the next. In much of this extant work, estimates of the relationship between college graduates and nontraded employment have not been statistically significant (i.e. Van Dijk, 2018).[13]

As these controls would suggest, there are two primary factors that might shape the relationship between the traded and non-traded sector. The first is the health of the economy, which given the panel structure of our data, will be absorbed by the annual fixed effects in our estimation approach. The second is different consumption preferences among regions. Consumption

[13] Even if the rationale for including education were stronger, we are unable to capture annual variation in the local share of graduates for a considerable portion of our study period, which occurs prior to the introduction of the ACS. The only alternative would be interpolation, which given the weak support for this predictor seems unwarranted.

preferences might differ among regions due to cultural effects – maybe people are more inclined to save than spend a marginal dollar earned in one region compared to another – or perhaps differences in climate among regions induce different types of consumption. In either case, to the extent that such preferences are fairly stable, their influence will be captured in our CBSA fixed effects. A remaining vulnerability could arise to the extent that there are unobserved location-specific shocks that are correlated with traded employment growth and that also affect nontraded employment. For instance, a location experiencing employment growth in its traded sector could simultaneously pass legislation raising payroll taxes. If those payroll taxes reduced employment growth in nontraded work, the lack of inclusion of a variable capturing local payroll taxes would lead to biased estimates of the multiplier. In practice, such bias could operate in either direction. As described above, to the extent possible, we account for the possibility of such time-varying unit-specific unobserved heterogeneity with our instrumental variable strategy.

4. Results

Table 2 presents descriptive statistics for the variables in our analysis, with values presented for the year 2017 for 380 CBSA areas.[14] Our panel is modestly unbalanced as a result of the few locations that occasionally have no employment for specific activities – particularly finance.[15] The average CBSA hosts around 43,000 workers in traded activities, and 196,000 jobs in non-traded

[14] We also produced core results for 1990-vintage Commuting Zones, as defined in Tolbert and Sizer (1996). Results were closely comparable, and are available upon request.

[15] Ames, IA and Bay City, MI are examples of cities which, in certain years, contain zero employment counts in either finance or manufacturing that become missing given our log-log approach. This remains a small issue – we have complete data on 75 percent of locations, and mostly complete for the remainder (9781 observations out of a possible 10668). Moreover, estimates using samples of convenience for each model produce results that are reasonably similar to those reported in this paper though it somewhat reduces coefficients on manufacturing and finance. To further test the sensitivity of these choices, in some models not reported here we also replaced zero counts in finance and manufacturing with extremely small numbers, permitting estimation on a full, balanced panel of 381 CBSAs. Doing so did not make an important difference to the results.

sectors of the economy. The average CBSA is home to almost 17,000 manufacturing jobs, 16,000 high-tech jobs and 3,500 traded financial service sector jobs. Standard deviations are fairly high across these measures, capturing the diversity in sizes across the U.S. urban system.

[TABLE 2 ABOUT HERE]

4.2 Main Findings

Using the approach described in equation (1), we now report estimates of our primary relationships of interest. Throughout our analysis, to discount purely idiosyncratic changes, we use 3-year moving averages of each variable, centered on the current year.

In Table 3 we report how changes in traded employment are related to changes in nontraded employment. We examine overall traded employment as well as specific subsets: manufacturing; high-tech; and finance. In Model 5, we undertake a ‘horse race’ where we combine in a single model the variables from Models 2-4, as well as a variable capturing the balance of the traded sector. This approach should address the possibility that omitted sectors in Models 2-4 may be correlated with changes in the respective traded sectors of interest. to avoid double counting in several situations in which parts of the manufacturing sector are also part of the high-tech sector (for instance, the manufacture of aerospace products and parts), in Model 5 we define such activities as high-tech.

[TABLE 3 ABOUT HERE]

In Model 1 of Table 3, we estimate that a one percent increase in local traded employment is associated with a 0.11 percent increase in local nontraded employment. This relationship is significant at a 99 percent level of confidence. Our baseline model, therefore, reveals a much lower multiplier effect than has been estimated in other recent studies, where, as noted above, elasticity measures range between 0.33 and 0.55 (Moretti, 2010; Moretti and Thulin, 2013: Van Dijk 2017 and 2018; Gerolimetto and Magrini, 2016)[16].

Models 2, 3 and 4 estimate the relationship between the local manufacturing, high-tech and traded financial sectors and the nontraded sector of the economy, respectively. As is the case in Model 1, each of these models indicate a positive and statistically significant association between nontraded employment and each subset of traded activity. In Model 2, a one percent increase in local manufacturing sector employment is associated with a 0.045 percent increase in local nontraded employment, while in Model 3, a one percent increase in local high-tech sector employment is associated with a 0.073 percent increase in local nontraded employment. In other studies, estimates of the multiplier effect of the manufacturing sector ranges from 0.33 to 0.55 (Moretti, 2010), while the multiplier effect of high-tech ranges as high as 0.7 (Moretti, 2010; Van Dijk, 2018; Goos et al. 2018Lee and Clark, 2019). In Model 4, we find that a one percent increase in local traded financial services employment is associated with a 0.033 percent increase in local nontraded employment. Finally, in Model 5, we observe that when we include each sector of interest in a single model, the magnitude of the association between each sector and nontraded jobs is reduced. This suggests the need to control for potential correlation between changes in employment in a sector of interest and changes in other sectors of the traded economy.

[16] It is possible that our coefficients may differ from existing results because we study a different period of time. For example, Moretti (2010) estimates multipliers for the period 1980-2000, whereas we study the period 1990-2017. However, when we restrict our analysis to blocks of time within our period of analysis, we find broad consistency in our results. We discuss other possible reasons why our results might differ from extant work in Section 4.2.

As in Moretti (2010) and Van Dijk (2017), these elasticities can be used to estimate the actual number of new local nontraded jobs added in response to the addition of each new traded job. Multiplying the ratio of 4.63 nontraded jobs for each job in the traded sector of the economy by the estimated elasticity between local traded sector employment and the local non-traded sector in Model 1 (0.111), we calculate that each additional job in the local traded sector of the economy generates 0.51 new jobs in the nontraded sector, which in input-output terms would typically be referred to as a multiplier of 1.51. This multiplier, while considerably lower than has been found in much of the work in this area, does fall within the range of Van Dijk's (2018) various estimates.

We performed similar calculations for each subset of the traded sector, based on the elasticities estimated in the horse race model (Model 5). Results are displayed in the final column of Table 3. We find that each new non-high-tech manufacturing job is associated with the addition of 0.41 jobs in the nontraded sector of the economy. For each additional high-tech job, an average of 0.79 jobs are added in the nontraded sector of a local economy. And each new job in traded financial services is associated with 1.41 new, local nontraded jobs. These multiplier estimates offer contrasts with the findings of most existing work (c.f. Table 1, in particular Moretti, 2010; Moretti and Wilson, 2014; Van Dijk 2017; Goos et al. 2018). A theoretically-consistent interpretation of these differences is simply that better-paid traded jobs generate larger multipliers.[17] Prior work has proxied for such differences using worker skills, on the assumption that workers with higher "skills" will have higher earnings (Moretti, 2010). We prefer to focus directly on wage variation, considering how multipliers might vary according to differences in remuneration across the traded sector.

[17] An additional reason is mechanical: activities like finance occupy far small proportions of overall activity, hence produce far larger ratios of nontraded to traded-subset employment.

[TABLE 4 ABOUT HERE]

Table 4 presents findings where the trade sector is differentiated by wages, as described above. Models 1-3 predict multiplier effects for each wage tercile on its own. For each wage tercile of traded employment, we observe a positive and statistically significant association with nontraded employment. Our preferred model is Model 4 – another horse race – which includes each tercile from models 1-3. As the table reveals, the highest paying components of the traded sector have a multiplier effect that is nearly twice as large as that of the lowest paying components of the traded sector. While we saw that higher paying industries, like high-tech and finance, have higher multiplier effects, this model supports the intuition that higher paying traded sectors add more jobs to the nontraded sector.

4.2 Further Analysis and Discussion

Next, we consider some of the reasons for our more diminutive estimates in relation to other studies. In addition to differences in the data used, its granularity, and definitional differences, the estimates reported in this paper could also arise due to our estimation strategy and scale effects.

4.2.1 Short-run Versus Medium-Run

The contrast between our findings and prior work could be due, in part, to our focus on the short-run, as distinct from the bulk of existing studies that consider decadal changes in employment. Over this longer period, for example, studies could be capturing lagged effects of initial changes.[18] As we have argued, this approach is also vulnerable to bias from arbitrarily chosen start and end years, as well as from unobserved location-specific shocks and differences intrinsic to the underlying data. To perform an "apples to apples" comparison, we adjust our data for medium-term effects. First, we consider three distinct (nearly) decadal periods, 1990 to 1999; 2000 to 2009; and 2010 to 2017. Second, we alter our estimation approach to more closely mimic Moretti (2010), capturing the log of absolute changes in employment between the start and end period of each 'decade'. The estimating equation pools these periods together, such that each location is observed three times, with standard errors clustered for each CBSA, and local and period fixed effects are included. The first two columns of Table 5 contrast our baseline model for all traded employment (Model 1, reprised from Table 3) with results from a decadal differenced model (Model 2). The coefficient of interest in these two models remains closely comparable to our initial results, displayed in Table 3, with the point estimate on the decadal model in fact somewhat smaller. The choice to focus on short- rather than medium-run effects therefore appears to have little material impact on the estimated size of overall traded multipliers. Comparing Models 3 and 4 that present annual and decadal estimates where high-tech traded employment is the key regressor of interest, we also see consistency across annual and decadal models, with 95 percent confidence intervals that partially overlap.

[TABLE 5 ABOUT HERE]

[18] Though not shown here, we also considered the possibility of lagged effects. Exploring models with as many as 10 years of lags, we concluded that any lagged effects appear to dissipate after a maximum of 3 years, and that the cumulative effect of these multipliers was actually fairly well captured in non-lagged coefficients. These results are available upon request. But, together with the results in Table 5, they suggest the general adequacy of annual models in which the assumption is that multipliers arise quite rapidly.

4.2.2 Instrumental Variables Estimation

Citing concerns around unobserved shocks, Moretti (2010), Van Dijk (2018) and others privilege estimates generated using instrumental variables techniques. It is therefore possible that our initial estimates are comparatively smaller because we have thus far relied on non-IV estimates. Models 5-8 in Table 5 report IV results using the Bartik shift-share measure. As described in Section 3, the aim of this index is to remove the locally-endogenous component of changes in employment. Different shift-shares are calculated, depending on whether we are instrumenting for total traded, or more narrowly, high-tech traded employment, as well as whether models are annual or decadal.

Some caution about our IV estimates (as well as those in the extant literature) is worth heeding. Section 2 highlighted reasons for general concern with the widespread use of Bartik-style instruments. And, though we present annual IV models as well as decadal, an emerging literature suggests we should prefer models estimated over long periods. For instance, Jaeger et al, 2018 suggests such instruments may poorly capture short-run effects when – as is surely the case in the present context – the spatial distribution of sectoral activity is stable over time. Decadal results are not automatically free from the broader concerns, but bias may be less severe.

The two-stage least squares regressions yield some consistent findings. First, the decadal and annual 2SLS FE coefficients on traded employment are positive and significantly linked to nontraded employment. Second, point estimates for Models 5-8 in Table 5 are uniformly larger than their noninstrumented counterparts in Models 1-4. Third, in each case the instruments are weak, which we can establish by comparing the Montiel-Pfluegel 'Effective' *F*-statistic against a critical value (with tau equal to 10 percent) of 23.11.[19] This accords with extant studies that report first stage results (i.e. Moretti, 2010; Van Dijk, 2018 – see directly comparable estimates in their Table 2, though without clarity on which type of first-stage *F* is reported).

As Bound et al (1993) once described, as a 'cure' for endogeneity problems, weak instruments can be worse than the disease. More recent advances in econometrics, however, permit efficient estimation with non-homoscedastic errors. Motivated by Davidson and Mckinnon (2014) and Andrews et al (2019), for each IV model we generate an Anderson-Rubin Confidence Set, which describes an efficient confidence interval in the presence of weak instruments in the non-homoscedastic case. For all but one model these provide finite, bounded intervals that can be interpreted to suggest that multipliers could range from values lower than noninstrumented estimates to values considerably higher. Practically, this translates into a range between 0.22 and 1.5 nontraded jobs for each new local traded job created, and between 0.46 and 2.65 nontraded jobs added for each new high-tech traded job. Comparison between these results and standard instrumented coefficients reported in the literature can only be partially illuminating, in that prior work has not used methods that enable inference in weak instrument case. Nonetheless, while the top end of our confidence sets may overlap with some extant research, it remains centered below estimates from prior work – the midpoint of the range falls at 0.86. Therefore, the difference between our estimates and existing work holds, even when we compare the impact of instruments across studies.

[*TABLE 5 ABOUT HERE]*

[19] See Montiel Olea and Pfluegel (2013) for a detailed derivation of the Effective F, and its suitability in the just-identified case with non-i.i.d errors.

4.2.3 Scale Effects

Our comparatively diminutive multiplier estimates may also be the result of using a wider range of metropolitan areas in our analytical sample. In contrast to our near-population of 380 metropolitan CBSAs, prior studies have often analyzed only the largest metropolitan areas. In Table 6, we explore the potential moderating role of city size in two ways. First, in Model 1 we estimate a variant of Equation (1) that includes a linear interaction term between the log of traded employment and a categorical indicator variable capturing cities' ranks in the employment distribution. The reference category group 1, includes 80 out of 380 CBSAs which have the lowest average total employment over the 1990-2017 period (mean employment=20,052). Groups 2 to 4 describe increments of 100 cities in the employment ranks, such that Group 2 captures the next largest 100 cities (mean employment=36,286), Group 3 the next 100 (mean employment=86,793), and Group 4 the most populous 100 cities (mean employment=667,040). Given the strong positive skew in the distribution of city sizes in the U.S. urban system, Model 2 creates a continuous-by-continuous interaction term, the product of log traded employment and average annual employment over the entire study period. As average employment is time-invariant, it drops out in estimation, but the interaction itself is interpretable, suggesting how the multiplier effect may vary across differently-sized local labor markets.

[FIGURE 1 ABOUT HERE]

The models presented in Table 6 tell a coherent story. Model 1 suggests that there are significant differences between multipliers estimated for cities with the smallest local labor markets and those for cities with larger employment bases. The growing size of the coefficient hints at the possibility that multipliers rise with city size, though confidence intervals in larger groups are partly overlapping. The significant interaction term in Model 2 suggests that the size of the multiplier rises with local levels of employment. To aid interpretation, Figure 1 visualizes the relationship, with marginal effects plotted for cities at average employment thresholds ranging from 50,000 to two million. The figure suggests that there are positive multipliers for cities at each size 'class', while the slope of the relationship rises with city size. Cities with employment of more than two million appear to receive the largest multipliers from the same proportional change in traded employment, whereas those with employment of less than 100,000 – well over 50 percent of the cities in the sample, experience considerably more modest multipliers. This further qualifies some of the results of prior work. One simple interpretation of this pattern is that, particularly in recent decades, the most highly remunerated traded work is concentrated in a small group of 'superstar' cities, meaning that multipliers generated based on the analysis of larger cities will create greater multipliers than is the case for the entire population (Kemeny and Storper, 2020; Davis and Dingel, 2020).

5. Conclusion

The aim of this study is to revisit existing work estimating the responsiveness of the local nontraded sector to changes in local traded employment across US cities. We make a number of adjustments to modeling approaches employed in recent multiplier studies. These additions include the use of high-quality and high-frequency data, a more complete range of cities, a more fine-grained distinction between traded and nontraded sectors, and improved industry definitions. As a result of our approach, our findings are not biased by arbitrary start and end points, the sample of cities we employ or industrial categorization. Meanwhile, we can better control for temporal shocks to the economy than most other studies. And while do not fully resolve issues around the use of shift-share instruments, we provide IV estimates that are unbiased in the presence of weak instruments.

Together, these adjustments set our findings apart from most recent studies in the field. We find that the addition of each traded job is associated with around half a new job in the nontraded sector, with IV estimates yielding a confidence set ranging between 0.22 and 1.5, with a midpoint of 0.86 nontraded jobs. Findings focused on the high-tech sector contrast more starkly with those reported in extant work. While studies find that the addition of one job in high-tech sector creates as many as 5 new jobs in the non-traded sector, our noninstrumented estimates indicate a much more modest addition of 0.79 nontraded jobs, with weak-IV-robust confidence sets spanning a range between 0.46 and 2.65 nontraded jobs, with a midpoint of 1.55. The high-tech sector is an important creator of nontraded jobs, but not nearly as important as has been suggested. Further, consistent with the hypothesized mechanisms linking traded and nontraded employment, we find that higher paying components of the traded sector have a bigger impact on nontraded jobs than lower paying elements of the traded sector, and that the largest multipliers are concentrated in the most populous cities.

Every year, local governments across the nation undertake efforts to boost their economies. Oftentimes, these efforts include the use of incentives to attract particular industries to their localities, to generate a local multiplier effect. A significant premium is paid to lure high-tech and manufacturing firms to regions. The findings presented in this analysis reveal that, in terms of job creation in the local services sector of the economy, the manufacturing sector performs no better than the average traded sector job. The impact of the high-tech sector is more impressive, with the multiplier effect of each high-tech job added 50% greater than the effect of the average traded sector job. Yet the impact of the high-tech sector in this regard is much smaller than is widely held. The findings presented in this paper, we hope, will provide a more realistic basis for understanding the relationship between prized industries and the local services sector of the economy.

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Tables and Figures

Table 1. Summarizing estimates of the number of nontraded jobs added in response to changes in traded employment.

Author/Year	Country	All Traded Multiplier	Tech Multiplier	Manuf. Multiplier	Years	Scale	Units	Industry Detail	Approach
U.S.-focused studies									
Moretti 2010	U.S.	-	4.9	1.6	1980, 1990, 2000	MSAs	217	2-digit NAICS	IPUMS, decadal shift-share, no city FEs
Gerolimetto & Magrini, 2016	U.S.	-	-	0.53[a]	1980, 1990, 2000, 2010	MSAs	123	2-digit NAICS	IPUMS, Decadal shift-share, no city FEs
Van Dijk, 2017	U.S.	1.6	-	-	1980, 1990, 2000	MSAs	220	3-digit NAICS	IPUMS, decadal shift-share, no city FEs
Van Dijk, 2018	U.S.	0.17-0.88[b]	-	-	1990, 1998, 2006, 2014	MSAs	217	3-digit NAICS	QCEW, 8-year shift-share, city FEs.
Nguyen & Soh, 2017	U.S.	-	-	1.1[c]	1998-2015	Counties	3086	4-digit NAICS	CBP, annual panel, shift-share city FEs, lags
Bartik & Sotherland, 2019	U.S.	0.5-0.6	0.9-1.0	0.13-0.86	1998-2016	CZs	691	1-6 digit NAICS	Direct-shift share estimation with lags
Non-US-focused studies									
Blasio & Menon, 2011	Italy	-	-	No effect	2001, 2007	LLMs	686	2-digit	ASIA, 6-year shift-share, no city FEs
Moretti & Thulin, 2013	Sweden	0.5	1	0.77	1995, 2001, 2007	FA-regions	51	3-digit NACE	Statistics Sweden, 6-year shift-share, no city FEs
Bashford-Fernández, 2014	Spain	2.1	-	-	1995, 2001, 2007	Provinces	52	5 sectors	INE, 6-year shift-share, no city FEs
Malgouyres, 2013	France	1.2	-	-	1995,2001,2007	Zone d'emploi	348	NACE	DADS, 6-year shift-share, no city FEs
Wang & Chanda, 2018	China	-	-	3.4	2000, 2010	Prefectures	277	2-digit	Pop Census, decadal shift-share
Goos et al, 2018¯	EU	-	3.9-4.4	-	2000,2005, 2010	NUTS-2	227	NACE + occ	Eurostat, 5-year shift-share, no city FEs.
Lee & Clarke, 2019	UK	-	0.7	-	2009, 2015	TTWAs	182	SIC	BRES + APS, 7-year shift-share
Cerqua & Pelligrini, 2020	Italy	-	-	0.26–0.33	1995-2006	LLMs	324	2-digit	Mezzogiorno-focus, policy shock as instrument
Kazekami, 2017	Japan	-	-	0.09-0.41	1986, 1991, 1996, 2001, 2006	CZs	269	2-digit	5-year panel Bartik-shift share, distinct periods

Notes: This table includes only papers that investigate multipliers stemming from general traded work, manufacturing and/or high-tech traded activities. Columns with multiplier effects present elasticities, where reported, as well as in parentheses the number of nontraded jobs generated in response to changes in traded work. (a) The figure arrived at in this study is hard to reconcile with accepted conventions about the ratio of traded to nontraded work. The authors suggest there are 0.8 nontraded jobs for each traded job – a figure that is far lower than other estimates. Their multiplier flows not from a smaller estimated elasticity – in fact their elasticity is closely aligned existing estimates for the U.S. – but rather from an unusual ratio of traded to nontraded employment. (b) IV estimates using BLS data suffer from weak instruments issues, hence reported multipliers in this table are based on reduced form OLS estimates. (c) Authors here define tradability on the basis of an export value threshold; this results in traded sectors consisting of all manufacturing, oil and gas extraction and mining.

Table 2. Descriptive Statistics for 380 Metropolitan Core-Based Statistical Areas, 2017

	Mean	Median	Standard Deviation
Total Employment	250,872	67,155	624,527
Traded Employment	42,322	8,848	114,240
Nontraded Employment	196,011	54,169	479,703
Manufacturing	16,587	3,956	42,196
High-tech	15,681	1,798	47,421
Traded Financial Services	3,477	396	15,036
Highest Wage Traded	28,349	3,192	92,365
Medium Wage Traded	9,071	2,691	20,793
Lowest Wage Traded	28,349	3,192	92,365

Note: Values are estimated over 380 metropolitan CBSAs that constitute the analytical sample, using QCEW data for 2017.

Table 3. The relationship between traded and nontraded employment in U.S. metropolitan CBSAs, 1990-2017

Dependent variable: log nontraded employment

Sector	(1)	(2)	(3)	(4)	(5)	Nontraded Jobs Per Traded in Model 5 (6)
All Traded	0.111***					
	(0.022)					
All Manufacturing		0.045***				
		(0.011)				
Non-High-Tech Manufacturing					0.030***	0.41
					(0.009)	
High-Tech			0.073***		0.063***	0.79
			(0.012)		(0.011)	
Financial Services				0.033***	0.025***	1.41
				(0.006)	(0.006)	
Rest of Traded Sector					0.013**	0.32
					(0.005)	
Constant	3.611***	3.798***	3.829***	3.888***	3.822***	
	(0.051)	(0.016)	(0.009)	(0.010)	(0.015)	
Adjusted R-squared	0.798	0.786	0.793	0.782	0.807	-

Note: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Standard errors clustered at the CBSA level in parentheses. Each model estimated on 9,782 observations nested in 380 metropolitan areas. Year and CBSA-specific fixed effects included in each model. Estimates generated using xtreg and xtivreg, with corresponding R-Squared estimates that do not include fixed effects. The unit of observation is metropolitan CBSAs. Dependent variable in all models is the log on nontraded employment. All models estimated on

Table 4: The relationship between traded and nontraded employment in U.S. metropolitan CBSAs by terciles of traded wages, 1990-2017

Dependent variable: Log Nontraded Employment				
	(1)	(2)	(3)	(4)
Log Traded Employment				
Wage terciles				
Lowest	0. 025**			0. 020***
	(0.008)			(0.006)
Middle		0. 032***		0. 037***
		(0.005)		(0.005)
Highest			0. 035***	0.038**
			(0.006)	(0.006)
Constant	3. 716***	3. 739***	3. 780***	3. 757***
	(0.009)	(0.009)	(0.008)	(0.011)
Observations	9,781	9,781	9,781	9,781
Cities	380	380	380	380
Adjusted R-squared	0.794	0.780	0.776	0.803

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at CBSA level in all models in parentheses. City and year fixed effects included in each model. Terciles defined according to average wages by industry in relation to the national wage distribution.

Table 5. The relationship between traded and nontraded employment in U.S. metropolitan CBSAs: Annual and Decadal OLS estimates with corresponding IV estimates, 1990-2017.

	OLS				IV			
	All traded		High-Tech Traded		All traded		High-Tech Traded	
	Annual	Decadal	Annual	Decadal	Annual	Decadal	Annual	Decadal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log traded	0.111***	0.075***	0.073***	0.040***	0.245***	0.213***	0.221*	0.178***
	(0.022)	(0.019)	(0.012)	(0.009)	(0.037)	(0.051)	(0.107)	(0.042)
Constant	4.076***	0.527***	0.428***	0.688***		0.73***		0.095***
	(0.055)	(0.018)	(0.011)	(0.016)		(0.01)		(0.007)
Montiel-Pfluegel Effective F	-	-	-	-	20.07	19.41	8.14	13.60
AR Confidence Set	-	-	-	-	[0.174-0.329]	[0.049-0.291]	[0.04-…]	[0.042-0.255]
R-squared	0.80	0.69	0.79	0.69	-	-	-	-
Observations	9,781	1,138	9,781	1,138	9,781	1,138	9,781	1,138
Cities	380	380	380	380	380	380	380	380

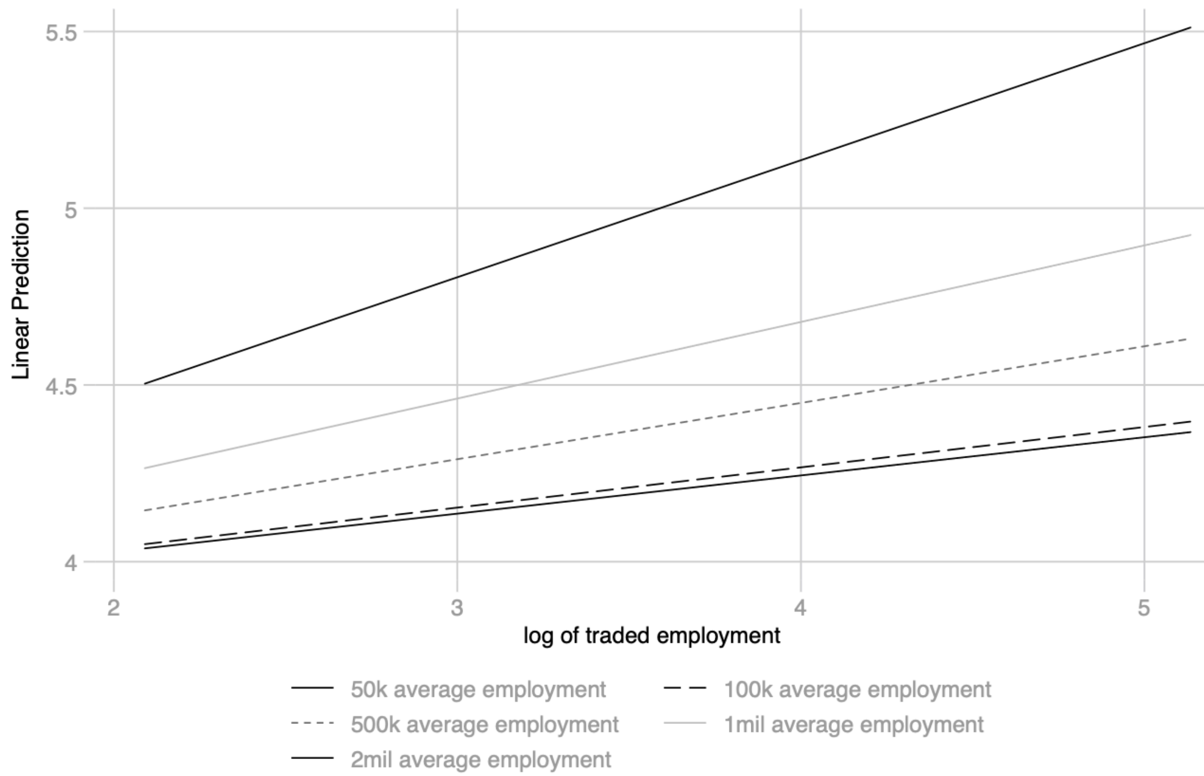
Note: * p<0.05, ** p<0.01, *** p<0.001. Standard errors clustered at CBSA level in all models. Year and CBSA-specific fixed effects included in annual models. Decadal differenced models (1990-1999; 2000-2009; 2010-2017) include CBSA and period fixed effects. Estimates generated using xtreg, fe and ivregress, with corresponding R-Squared estimates that do not include fixed effects. AR Confidence Set is the Anderson-Rubin Confidence Set. Critical value for Effective F at tau=10% is 23.11 for all four IV models. Instrument is Bartik shift-share (see Equation (2)).

Table 6. Estimating the moderating effect of overall city size in terms of employment on the relationship between traded and nontraded employment

Dependent variable: Log Nontraded Employment	(1) Categorical Employment Rank Interaction	(2) Continuous Employment Interaction
Log Traded Employment	0.037	0.103***
	(0.032)	(0.024)
Size Rank Group * Log Traded Employment		
Group 2	0.105**	
	(0.039)	
Group 3	0.126**	
	(0.039)	
Group 4	0.180***	
	(0.045)	
Average Employment * Log Traded Employment		0.0002***
		(0.00005)
Constant	3.45***	3.62***
	(0.049)	(0.058)
City Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	9781	9781
Cities	380	380
Adjusted R-squared	0.996	0.995

Note: Standard errors in parentheses, clustered by CBSA; * p<0.1, ** p<0.05, *** p<0.01. Rank categories in Model 1 defined as follows: 4=largest 100 CBSA (mean employment=667,040), 3=101-200 ranks (mean employment=86,793); 2=201-300 ranks (mean employment=36,286); 1=remaining (smallest) CBSAs (mean employment=20,052). Group 1 is the reference category in Model 1. R-squared includes fixed effects.

Figure 1. Marginal effects of the relationship between traded and nontraded employment by average CBSA employment, 1990-2017



Note: Marginal effects visualized from Model 2, Table 6, plotted for cities at specific levels of average (1990-2017) employment.