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An empirical examination of shift‐share instruments

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Abstract

Bartik's (1991, 1993) approach to identifying shocks in demand to regional economies has been used extensively for nearly 30 years. We chronicle the development of Bartik‐type shift‐share instruments and examine the empirical performance of alternative versions that use different combinations of national shift and local share variables in their construction. We offer three main findings. First, instruments constructed from shares that omit employment in nontraded sectors empirically dominate versions that include total employment. Second, industrial sectors with high average shares and low variation across areas are more likely to be nontraded and endogenous. This suggests placing large weights on nontraded sector shares worsens both relevance and potential endogeneity. Finally, we demonstrate national shifters other than employment, such as prices and wages, can be used to construct instruments with unique and relevant explanatory power.

KEYWORDS

identification, measurement, regional dynamics, urban dynamics

JEL CLASSIFICATION

R10; R21; R23

1 | INTRODUCTION

Nearly 30 years have passed since the publication of *Who Benefits from State and Local Economic Development Policies?* by Timothy J. Bartik (1991), the book that introduced the "Bartik instrument" in the economics literature. Bartik's measure is a hypothetical projection of regional employment growth that is plausibly exogenous. The projection consists of the growth in employment that would have occurred had each industry in a region grown at its national rate of growth.

Bartik instruments are used in research designs when treatment occurs at an aggregate level. The projection of employment growth Z is calculated for each area j by taking the inner product of local employment shares s and national employment growth rates G by industry sector i,

$$Z_j = \sum_{i=1}^{I} s_{ij} G_i,$$

where the share is defined as the ratio of local sector employment to total local employment, with $\sum_{i=1}^{I} s_{ij} = 1 \ \forall\ j$ by construction. This measure is then used as a proxy for changes in local labor demand in equations of the form,

$$Y_j = \beta_0 + \beta_1 X_j + u_j, \tag{1}$$

where Y_j is the growth rate of a local outcome variable, and Z_j serves as an instrumental variable for X_j, the local employment growth rate, a treatment variable the researcher fears may be endogenous.

Bartik developed his namesake instrument to estimate the inverse elasticity of labor supply in cities, the effect on wages of an exogenous change in employment. Because observed employment and wages are endogenous, Bartik needed an instrument for local labor demand. However, this general technique of *combining aggregate shocks with measures of local shock exposure*, as popularized by Bartik (1991, 1993) and also Blanchard and Katz (1992), has had a remarkable influence in the economics literature—especially urban, regional, and real estate economics—that extends far beyond this specific application.[1] In addition to employment shocks, the research design has been used in hundreds of papers to estimate causal effects of various endogenous treatment variables that are aggregated at some regional level.[2,3]

In this paper, we chronicle the development of Bartik-type research designs in the literature and examine the empirical performance of alternative constructions. Over time, numerous modifications to the formulation of shift-share measures have been advanced and we add several of our own. The first issue concerns the choice of shifters. Several options are available. National employment is the most common in the literature, but wages and prices have also been used. There are also issues involving the choice of local shares. Should they be constructed using all employment or with local-service employment excluded. If using just employment in tradables, should shares be constructed based on location quotients or assumptions concerning the export-orientation of each sector? Finally, should shares be updated each period or fixed to a base year? Regression instruments are another variant, but how do they compare to standard instruments? These issues have not been addressed systematically in the literature and we fill this gap.

Although we discuss theoretical merits of candidate indexes, our primary means of comparison is empirical performance. We first construct an annual panel of 18 shift-share demand indicators at the county level covering the years 1990 through 2017. We then conduct a tournament to assess the relevance, exogeneity, and information content within each measure in models predicting short-run and intermediate-run changes in five locally endogenous variables: employment, wages, housing stock, population, and house prices. From the results of this tournament, we offer several main findings.

[1]The italicized text is from Borusyak, Hull, and Jaravel (2018, p. 1).

[2]Bartik (1991) has over 2,400 citations according to Google Scholar as of February, 2019. We do not attempt to determine how many of the citing papers employ a Bartik-type instrument, but additional searches on exact phrases such as “Bartik instrument” return hundreds of hits. For example, the approach has been leveraged influentially to examine the effects on labor markets of immigration (Altonji & Card, 1991), location of manufacturing (Holmes, 1998), incidence of crime (Gould, Weinberg, & Mustard, 2002), growth of disability rolls (Autor & Duggan, 2003), dynamics of housing supply in cities (Glaeser, Gyourko, & Saks, 2006), highway congestion (Duranton & Turner, 2011), effect of international trade on the U.S. employment (Autor, Dorn, & Hanson, 2013), supply of credit (Greenstone, Mas, & Nguyen, 2014), effect of food aid on civil conflict (Nunn & Qian, 2014), to name but a few.

[3]“Regional” in this context simply represents a level of geographic aggregation of economic activity. This commonly includes cities, counties, or states within the United States, or another large country, or countries within a larger currency area and trade bloc such as the European Union.

First, in terms of shifters, employment performs best among the national variables we test, but prices and wages are both useful in certain contexts. Second, in terms of shares, indicators calculated using local export employment contain more explanatory power than ones with all employment. Export shares calculated using location quotients are, in turn, more powerful than those based on a priori assumptions of export-orientation. Third, sectors with high employment shares and low variation across counties are more likely to be endogenous when included directly as instruments in two-stage least squares (2SLS) models. High share/low variation sectors are those typically thought to be serving a local area, whereas low share/high variation sectors are typically perceived to be export sectors. Combined with the findings related to power, it appears that placing large weights on nontradable sectors in instruments worsens both relevance and potential endogeneity. Fourth, context matters. Different instruments have different power when attempting to model different endogenous variables over different time frames and in different types of locales. For instance, instruments calculated using national prices have almost no power in samples of center-city MSA counties, but those calculated using national wages do; in more rural areas, the finding is reversed.

The organization of the paper is as follows. We first discuss the evolution of the Bartik instrument and its origins in the shift-share decomposition. This coverage includes recent papers on identification in Bartik-type designs and then moves on in Section 3 to various attempts at producing alternative instruments. In Sections 4 and 5, we describe our data and conduct an empirical tournament to systematically explore strengths and weaknesses of the various classes of instruments in common regional economics settings. We conclude in Section 6 with several recommendations for researchers to consider when modeling exogenous variation in locally endogenous variables.

2 | TRADITIONAL SHIFT-SHARE INSTRUMENTS

Researchers have developed various techniques to identify shocks in demand to regional economies. We place these approaches into three broad categories: (a) the shift-share literature, which takes national changes in employment (Bartik, 1991, 1993; Blanchard & Katz, 1992), prices (Hollar, 2011; Larson & Zhao, 2017; Pennington-Cross, 1997), or wages (Diamond, 2016; Guerrieri, Hartley, & Hurst, 2013; Partridge & Rickman, 1995) to instrument for local shocks; (b) the time-series literature, which makes assumptions about short-run elasticities (Blanchard & Katz, 1992), or operates within structural vector autoregression (Carlino & DeFina, 1999; Partridge & Rickman, 2003, 2006) or synthetic control frameworks (Abadie & Gardeazabal, 2003; Abadie, Diamond, & Hainmueller, 2010); and (c) the government spending literature, which uses variables that are locally exogenous by assumption, such as military or total federal spending (Owyang & Zubairy, 2013; Ramey, 2011).

Of the approaches listed above, the shift-share technique has been the most prevalent due to its simple calculation and ease of use in cross-section and panel regression frameworks. In this section, we chronicle the development in the economics literature of Bartik-type, or shift-share, research designs, as they have been called alternatively. Our coverage includes changes over time in strategies for both construction and identification of the instrument.

2.1 | Development

Bartik (1991) developed his instrument for demand shocks out of the shift-share approach to analyzing regional evolution. Shift-share analysis is a descriptive technique that attributes the growth in a local economic variable to industry, local, and national factors. Although the approach seems to have its origin in the World War II milieu (e.g., Creamer, 1943), the basis of the technique in modern use is an accounting identity due to Dunn (1960) that decomposes the regional growth rate, g, between time $t-1$ and t, of an economic variable, y (typically employment or output) by industry sector i,

$$\Delta y_{it} = y_{it-1}g_{it} \equiv y_{it-1}[G_t + (G_{it} - G_t) + (g_{it} - G_{it})], \tag{2}$$

where G_t is national average growth rate across all sectors and G_{it} is national average for a particular sector. Equation (2) attributes growth in y_i to three influences: (a) the performance of the national economy (G_t); (b) the national performance of industry i relative to the national economy ($G_{it} - G_t$); and (c) the regional performance of industry i relative to its national performance ($g_{it} - G_{it}$). In textbook shift-share nomenclature (e.g., McDonald & McMillen, 2010), these three influences are called the *national share*, *industry mix*, and *regional shift* effects, respectively. The third component ($g_{it} - G_t$) is also commonly dubbed the *competitiveness term* and given particular attention as an important driver of regional evolution.

In a personal communication (DAB, July 21, 2016), Bartik indicates he had the idea to use an industry mix summation as a demand shock measure while performing a shift-share analysis on Tennessee for a chapter on state economies, that is, Bartik (1988). Whereas typical shift-share analyses had focused on the regional shift effect in explaining economic growth, in this chapter Bartik emphasizes how having a favorable industry mix, consisting of above average shares of employment in industries with above average national growth rates, could substantially influence a region's performance. This effort to use shift-share analysis in different ways led Bartik to the realization that inter-area variation in an industry mix summation could constitute a good instrument for shocks in demand to a regional economy. The variation in such an instrument would be driven by changes in national demand for an area's export industries which, he would go on to argue, were unlikely correlated with local supply shifters (Bartik, 1991, p. 276).

To construct an exogenous demand indicator out of the shift-share decomposition, Bartik (1991) describes omitting the endogenous competitiveness term ($g_{it} - G_{it}$) leaving the remaining two plausibly exogenous components (G_t) and ($G_{it} - G_t$). These he aggregates across industries to form a projection for local employment growth due to national influences. If e_{ijt} represents employment in industrial sector i for location j at time t, and E_{it} represents concurrent national employment in the industry, that is, $E_{it} = \sum_{j=1}^{J} e_{ijt}$, then Bartik's original 1991 measure of projected employment growth is given by

$$\begin{aligned} B_{jt} &= \sum_{i=1}^{I} e_{ijt-1}G_t + \sum_{i=1}^{I} e_{ijt-1}(G_{it} - G_t) = \sum_{i=1}^{I} e_{ijt-1}G_{it} \\ &= \sum_{i=1}^{I} e_{ijt-1}\left(\frac{E_{it} - E_{it-1}}{E_{it-1}}\right). \end{aligned} \tag{3}$$

The original formulation calculates the absolute change in regional employment that would have occurred between two time periods had each one of a region's industrial sectors grown at its national rate of growth. In application, the original formulation requires a final step to convert this growth increment into log growth terms before implementing instrumental variables (IV) estimation, that is $Z_{jt} = \ln(e_{jt} + B_{jt}) - \ln e_{jt}$.

The standard version of the Bartik instrument that appears in the economics literature differs from the original version in two ways. First, beginning with Blanchard and Katz (1992) and Bartik (1993), the standard measure gives projected employment growth in relative rather than absolute terms. Second, beginning with Autor and Duggan (2003), the national industry growth rate terms typically exclude own-area employment. The construction is as follows:

$$Z_{jt} = \sum_{i=1}^{I} \frac{e_{ijt-1}}{e_{jt-1}}\left(\frac{E_{ijt} - E_{ijt-1}}{E_{ijt-1}}\right), \tag{4}$$

where $E_{ijt} = \left(\sum_{\tilde{j}=1}^{J} e_{i\tilde{j}t}\right) - e_{ijt}$. The standard formulation takes national employment shocks by industry, often called the shifts, and shares the effects out to regions based on the fraction of regional employment in those industries. Because the output Z_{jt} from (4) is a growth rate, it can be used directly as an instrument without additional steps.

Compared with (3), the "national" employment growth rate terms in (4) are indexed by a region subscript j because they vary across cross-sectional observations. This leave-one-out construction has become a common technique in the literature for diminishing concerns that production in certain industries may be concentrated in particular areas. The issue is that sectors with large shares are those most likely endogenous with respect to local economic activity.

Did Bartik invent the Bartik instrument? Beginning with Freeman (1975, 1980), there are papers in the literature on wage structure that use a fixed-coefficient "manpower requirements" index which is conceptually analogous to the measure that Bartik employs as an instrument in the book he published a decade later in 1991. Compared with Bartik's measure, the manpower requirements index predicts the exogenous change in labor demand for a demographic group, as opposed to a geographic region. However, the structure of the two measures is otherwise identical: both consist of the inner product of a vector of national growth rates and a vector of local shares. In the notes to his 1991 book, Bartik acknowledges that he is not the first researcher to use a measure based on industry mix to predict the economic growth of cities (see p. 282). Although he neglects to reference Freeman, he does cite a volume on urban decline by Bradbury, Downs, and Small (1982) as a prior instance. In this study, the authors include a four-sector shift-share measure directly as an explanatory variable in parametric city growth equations, not, we point out, in an instrumental variables design. In conclusion, while Bartik is not the original creator of what the field has come to call "the Bartik instrument," he clearly deserves significant credit for popularizing the measure and first using it as an instrument for shocks in demand to regional economies.[4]

2.2 | Identification

Identification in Bartik-type designs is complex, more so than in typical IV approaches. Not only is there the possibility of endogenous relations among local growth rates, on the one hand, and local shares and national growth rates on the other, these violations of identifying assumptions can be obscured by the inner-product structure of the instrument that may combine hundreds of pairs of industry growth rates and employment shares.

Traditionally, the view has been that Bartik-type designs require good-as-random national shocks for validity and heterogenous local shares for relevance. For example, Blanchard and Katz (1992) claim a shift-share instrument is exogeneous in their estimating equation by invoking the small market assumption. This assumption holds if innovations in labor supply at the regional level do not meaningfully affect industry employment at the national level. They contend the instrument is relevant by assuming there is sufficient variation in industry mix across regions to achieve a strong fit in the first stage.

Over time, identification has broadened beyond the focus on national industry growth rates and the small market assumption. To frame this discussion, note the symmetry inherent in the inner-product structure of shift-share instruments. While the traditional approach to identification has regarded the vector of national growth rates as the instruments and the vector of regional shares as weights, one could alternatively view the shares as the instruments and the shocks as weights. This is the perspective, for example, taken in the Baum-Snow and Ferreira (2015) chapter on inference in urban and regional economics. The authors argue if identifying variation in the instrument derives primarily from heterogeneity in industry mix across regions, then validity is most naturally described in terms of the industry employment shares.[5] Weak exogeneity of industry shares in this approach is typically justified based on a temporal assumption. This assumption holds if innovations in

[4]As has been the case with other eponymous laws, theorems, and techniques, there may be a tendency over time for researchers to over-credit Bartik's influence at the expense of earlier works. For example, consider three prominent papers that use shift-share instruments to predict labor demand for particular demographic groups. These works belong more properly in a chain tracing back to Freeman (1975, 1980) than to Bartik (1991). Katz and Murphy (1992) cite only Freeman; Bound and Holzer (2000) cite Freeman and Bartik; and Aizer (2010) cites only Bartik.

[5]A similar perspective motivates assumptions behind synthetic control models. In the synthetic control method, regional growth rates are the "shift" variable whereas the preintervention weights are the "shares."

regional labor supply are conditionally uncorrelated with past industry composition at the regional level directly and through unobservables.

Considering the structure of shift-share instruments also calls attention to the possibility of using either the vector of shares or shocks alone as instruments instead of their inner product sum. This approach takes inference from the just-identified case of the traditional scalar instrument to the over-identified setting with many and weak instruments that has been studied by Angrist, Imbens, and Krueger (1999), Anatolyev (2013), Kolesár, Chetty, Friedman, Glaeser, and Imbens (2015), and among others. Building on this econometric literature, three recent working papers have developed asymptotic distribution theory for shift-share estimators in IV designs that yields new results.

Goldsmith-Pinkham, Sorkin, and Swift (2018) examine three asymptotic cases in the setting of a single endogenous regressor and many instruments. If they let time go to infinity and fix the number of industries and locations, then identification requires random shocks conditional on shares. This case seems incompatible with typical regional economics applications in which researchers observe a large number of locations and industries, but a relatively small number of time periods. If instead the authors let the number of locations grow large and fix the number of industries and time periods, then identification requires random shares conditional on shocks, and inference, in turn, must assume the shares are independent across locations. In the case where they let locations *and* industries go to infinity and fix just time, they obtain the result from Kolesár et al. (2015) that shares may have direct effects on the outcome variable—that is the shares can fail the exclusion restriction (many invalid instruments)—because the misspecification averages out in the limit. This last case in particular illustrates how identification when using the entire vector of shares as instruments differs from identification when using a scalar instrument.

Based on the random-shares view of identification, Goldsmith-Pinkham et al. (2018) develop three results that should be of interest to practicioners. First, the authors show the finite sample equivalence between 2SLS estimator using the Bartik instrument and the generalized method of moments (GMM) estimator using the shares as instruments and shocks as weights. Next, they show the GMM estimator is a weighted sum of just-identified estimates based on a finite sample decomposition due to Rotemberg (1983). Finally, the sum of these "Rotemberg weights" is unity and they can be interpreted as misspecification elasticities: in other words, they indicate the amount of bias produced in the shift-share estimator of a one percentage point increase in bias in the estimator for a particular industry share. In short, the Rotemberg weights measure the amount of identifying variation contributed by each industry share.[6]

Borusyak et al. (2018) also examine identification in the setting of a single endogenous regressor and many shift-share instruments. In contrast to Goldsmith-Pinkham et al. (2018), their discussion of consistency proceeds from the more traditional random-shocks point-of-view. Based on the inner-product structure of the Bartik measure, they show that shift-share IV coefficients are numerically equivalent to a weighted industry-level regression. Based on this equivalency, they show that identification can come from random national shocks conditional on shares and for inference assume that shocks are independent across sectors.[7]

From their analysis of asymptotics, Borusyak et al. (2018) caution that the IV estimator may be biased when shocks are estimated using the same sample as the IV estimation, as is the common practice. Following Angrist and Krueger (1995) and Angrist et al. (1999), they show this potential inconsistency can be solved by split-sample estimation, of which leave-one-out is an extreme case. This provides a theoretical justification for the leave-one-out construction in Equation (4) beyond the concerns over violation of the small market assumption that have been raised in the literature for justification.[8]

[6]Code to estimate the Rotemberg weights in R and Stata is provided as an online appendix by Goldsmith-Pinkham et al. (2018) and available online at github.com/paulgp/bartik-weight.

[7]Borusyak et al. (2018) have developed a Stata package which creates industry-level aggregates based on their equivalence result available at github.com/borusyak/shift-share.

[8]Goldsmith-Pinkham et al. (2018) make an additional point that the excluded area in leave-one-out represents a finite sample correction factor for the bias otherwise induced by including mean growth rates in shift-share instruments.

Finally, Adao, Kolesár, and Morales (2018) build on the random-shocks approach to identification in Borusyak et al. (2018). They show that standard errors in shift-share designs are smaller than the true deviation of the ordinary least square (OLS) estimator. The problem arises because regression errors are likely correlated across regions that may not be spatially adjacent but have similar industry mixes. They derive methods that yield the appropriately wide confidence intervals.[9]

Ultimately, the applied researcher must consider the conditions necessary for the asymptotics summarized above to provide reliable approximations for the behavior of shift-share estimators in finite samples. Various specification tests and robustness measures for IV estimation put forward in the literature are suggested by the discussion in this section. Regarding relevance, standard diagnostics for first-stage goodness-of-fit such as adjusted and partial R^2's and associated F statistics should apply. Regarding exogeneity, when shift-share measures are implemented as demand indicators, researchers can test if the measures are uncorrelated with observed supply shocks in the hope that unobserved shocks are also orthogonal. For influential industries with large Rotemberg weights, researchers should make a strong case for exogeneity, and consider excluding those industries that might be problematic based on theory and sensitivity to misspecification. For shares with smaller weights, it is likely innocuous to assume possible misspecifications average out with a sufficiently large number of industries. Results in the recent literature point to similarities between shift-share instruments, GMM estimators, and maximum-likelihood estimators. This suggests the possibility of performing overidentification tests using various shift-share instruments and estimators.

When utilizing a Bartik instrument in a panel regression framework, researchers must also decide between fixing industry shares to an initial period or updating shares in successive time series. This is ultimately a trade-off between relevance and exogeneity, power and potential bias. Industry shares in one period are functions of growth rates in the previous. Thus, the potential exists for serial correlation in growth rates to violate the exclusion restriction. However, if the identifying variation in the instrument comes largely from the industry shares, then the strength of the relation between the instrument and the endogenous variables in the first stage will attenuate as the time interval from the base year increases.

3 | ALTERNATIVE SHIFT-SHARE INSTRUMENTS

We next discuss issues related to construction of shift-share instruments in the context of identifying regional demand shocks, specifically, choice of shocks and shock exposures, and restrictions inherent in inner-product structures. To analyze these issues empirically, we introduce a suite of alternative shift-share measures to which we can compare the traditional construction. Some of these measures have appeared elsewhere in the literature, and others are new contributions.

3.1 | Just-identified approaches

The first question concerns whether employment growth represents the most appropriate operationalization of the demand shock construct. Rickman (2010, p. 34) describes the issue succinctly when he contends that “employment and population [are] both outcome variables and not independent structural measures of labor demand and supply,” and highlights the difficulties of estimating dynamic responses to demand shocks when actual demand determinants are absent from the models being estimated.

It is possible to create shift-share instruments by combining employment shares with nonemployment measures of shocks that might have a more clear basis in economic theory. In addition to changes in employment, wages, and

[9]The code to implement the confidence intervals in R or Matlab is provided as an online appendix by Kolesár et al. (2015) and available online at github.com/kolesarm/BartikSE.

producer prices at the national level have also been used to index demand shocks in urban and regional applications. While wages have been used most often, but not exclusively, for specialized instances, such as studying housing price dynamics, producer prices have been put forward as a more general alternative to employment in index construction. It seems safe to say that operationalization remains an open question.

The next issue concerns whether the shares used to apportion shock exposure should be based on total employment or some subset. Arguments have been made in the literature for considering only employment that produces for export from the region. First, the number of workers that produce goods and services for local consumption may be endogenous with respect to the local economy. Second, the inclusion of nontradable sectors might simply add noise to the index, decreasing its power to isolate labor demand shocks. Bartik stated explicitly that he intended the instrument to serve as a proxy for demand for a local area's exports—see Bartik (1991, p. 274) and Bartik (1993, p. 300). However, in both versions of the instrument, presumably for reasons of expediency, all employment in all industries is included, with locally endogenous production weighted the same as production that is presumably exported.[10]

Whether shift-share instruments can be improved by incorporating information on export-orientation also remains an open question. Even if it is the case that nontraded sectors actually have larger multipliers than traded sectors, the lost predictive power from omitting them could be more than offset by high variability in industry shares and national growth rates in traded industries, as documented in Kilkenny and Partridge (2009).

We treat the choices of shocks and shock exposures as empirical questions, and we address the issues in the following sections by considering modified formulations of the shift-share instrument. The alterations include shocks based on change in wages or producer prices instead of employment, and shares that attempt to separate shocks into export and local service components.

3.1.1 | Distinguishing export employment

The literature on local multipliers contains various techniques for distinguishing export (*economic base* or *basic*) and local service (*nonbasic*) economic activity that could be used to construct alternative shift-share instruments. The two we use, the assumption method and location quotients, are chosen because they represent the easiest and the most common approaches, respectively, according to Thulin (2015).

The assumption method simply assigns industrial sectors into tradable and nontradable categories based on subjective criteria. To construct export employment measures using this approach, we follow Moretti and Thulin (2013) in assuming all employment in mining and manufacturing represents basic activity and assigning all remaining industries to the nonbasic category.[11] Basic employment is used to calculate export employment shares in the instrument, while all nonbasic employment is simply excluded.

An alternative to the assumption approach utilizes location quotients to distinguish export-oriented employment rather than exporting sectors. Location quotients are a common method of assessing industry concentration in a region. They are easy to calculate from the same data used to determine employment shares in shift-share instruments. An employment location quotient takes the ratio of a sector's share of area employment relative to its share of national employment,

$$LQ_{ijt} = \frac{e_{ijt}/e_{jt}}{E_{it}/E_t}. \tag{5}$$

[10]Bartik (1991) defends this approach, explaining how the instrument in a panel regression framework effectively downweights local service industries. Locally traded industries are likely to have low cross-region variation, so in a panel fixed effects model, locally endogenous employment shares will be close to national averages and thus their influence on parameter estimates should be relatively small.

[11]Moretti and Thulin (2013) consider Swedish industries which are recorded under a different classification system than North America. In later sections, we implement the assumption method by categorizing as basic any NAICS sector beginning with 1, 2, or 3.

A value greater than unity indicates that employment in industry i for region j exceeds the national average for that sector. To construct export employment measures using the location quotient approach, we follow Brown, Coulson, and Engle (1992) by assuming that employment in excess of the national average goes toward production of goods and services exported to other locations.[12] Basic or export employment (m_{ijt}) is given by this excess amount and calculated as follows,

$$m_{ijt} = \left(\frac{\mathrm{LQ}_{ijt} - 1}{\mathrm{LQ}_{ijt}}\right) e_{ijt}, \tag{6}$$

if $\mathrm{LQ}_{ijt} > 1$, otherwise $m_{ijt} = 0$. All other employment is considered nonbasic, or local service, and denoted l_{ijt}.

3.1.2 | Employment indexes

Our first two alternative shift-share measures are calculated by multiplying export or local service employment shares, respectively, by changes in national industry employment, excluding own region employment, for each industry sector in a region:

$$(\text{Exp Emp Index})_{jt} = \sum_{i=1}^{I} \frac{m_{ijt-1}}{m_{jt-1}} \left(\frac{E_{ijt} - E_{ijt-1}}{E_{ijt-1}}\right), \tag{7}$$

$$(\text{Loc Emp Index})_{jt} = \sum_{i=1}^{I} \frac{l_{ijt-1}}{l_{jt-1}} \left(\frac{E_{ijt} - E_{ijt-1}}{E_{ijt-1}}\right). \tag{8}$$

Export employment is classified as described above using either the assumption or location quotient methods, and local service employment is the remainder, that is $l_{ijt} = e_{ijt} - m_{ijt}$.

As with the standard Bartik instrument, the exclusion restriction for the export employment index (7) assumes lagging provides weak exogeneity for the local industry shares and the relatively small size of the region provides exogeneity for the national growth rates. By indexing shocks in nontraded industries, the local service measure (8) is constructed to be as endogenous as possible. We do not present it as a potentially valid shift-share instrument, of course, but rather for use in testing as a comparison device. Both indexes remain vulnerable to the criticisms raised above on shock choice that employment is a derived demand variable.

3.1.3 | Wage indexes

We also construct new export wage indexes based on average wage per worker. Beginning with Partridge and Rickman (1995), there are examples in the literature of instruments that index shocks in demand to local labor markets using wages instead of employment. The index presented in this section is motivated by Guerrieri et al. (2013), who use local employment-weighted changes in national average wages as instruments for changes in urban housing demand. We extend their measurement concept by constructing the index with a focus on export industries and based on higher frequencies and greater industrial granularity.

[12] Of course, $\mathrm{LQ}_{ijt} > 1$ can also result from heterogenous preferences among households combined with sorting by city, or complementarities between local amenities or production and employment in that sector that result in higher levels of local consumption.

Our export wage indexes are calculated by multiplying local export employment shares by changes in national average weekly wage for each industrial sector,

$$(\text{Exp Wage Index})_{jt} = \sum_{i=1}^{I} \frac{m_{ijt-1}}{m_{jt-1}} \left(\frac{W_{ijt} - W_{ijt-1}}{W_{ijt-1}} \right) \tag{9}$$

where $W_{ijt} - W_{ijt-1}$ is the change in national average weekly wage for industry i net of region j.

The new export wage indexes are similar to the export employment indexes in that they infer an increase in demand for a local area's product based on responses in inputs. Rather than input quantities (employment), however, the wage indexes measure input prices (wages). The wage indexes have the same issues as the employment indexes in terms of assumptions necessary for identification.

3.1.4 | Price indexes

To address the causal ambiguity of demand measures based on changes in national employment, Pennington-Cross (1997) presents an export price index for use as an instrument for regional demand shocks. This index is based on national price changes attributed to regions in proportion to share of export employment by industry.

The estimates of export employment by sector described previously are converted into an export price index, by multiplying the fraction of total export employment in each sector by the percentage increase in national product prices for that industry,

$$(\text{Exp Price Index})_{jt} = \sum_{i=1}^{I} \frac{m_{ijt-1}}{m_{jt-1}} \left(\frac{P_{it} - P_{it-1}}{P_{it-1}} \right), \tag{10}$$

where $P_{it} - P_{it-1}$ is the change in national average prices for the output of industry i.

In a panel regression framework, first stage estimates are based on deviations of the instrument from the mean. The variation in these deviations are equivalent to shifts in the terms of trade for a region. These changes in relative prices unambiguously represent positive external demand shocks through the profit maximization problem of firms in a given location.

Identification based on the small market assumption may be more defensible for price indexes than for employment indexes because prices of tradables are often set on national or world markets. As with all industry mix measures, these price indexes remain vulnerable to criticism for assuming sectoral independence.[13] Another shortcoming of the price index approach is technological change. If prices fall due to a rapid rise in productivity, the measure will fail to capture changing demand for an area's exports.

Use of the export price index in the empirical literature has been limited in spite of its appealing operationalization of underlying economic processes. One exception is Hollar (2011), who applied the export price index to compare employment growth in central cities and suburbs. The limited adoption of price indexes could stem from the additional complexity of working with producer price data in addition to employment data. Another explanation may be an incumbent effect, as familiarity with employment and wage approaches is more widespread in the economics discipline.

[13]In this way, the export price indexes relate to the vast literature on oil price shocks. It is widely asserted that oil price shocks have positive effects in some locations and negative effects in others (Larson & Zhao, 2017; Owyang, Piger, & Wall, 2005). The price indexes capture the positive demand effects of oil price shocks as they relate to employment in oil and gas industries, but not the substitution and income effects from the price change in a household's utility maximization problem, for example.

3.2 | Over-identified approaches

The view presented in the recent literature on shift-share instruments, in particular Borusyak et al. (2018) and Goldsmith-Pinkham et al. (2018), is that Bartik instruments constitute a solution to the dimensionality challenge involved in leveraging the sectoral structure of employment to identify demand shocks. A fully specified first stage equation could conceivably instrument for local employment growth using lagged local industry shares, concurrent national industry growth rates, and a full set of interaction terms to model spillover effects. However, it is difficult to imagine an application in which such an equation would have sufficient degrees of freedom to test for significance of parameter estimates. As the recent literature points out, the Bartik instrument reduces the dimensionality of this estimation problem to the just-identified case.

One drawback of the just-identified approach, assuming the instrument is valid, is a loss of power in the first stage. Some industries have larger local multipliers than others for a variety of reasons, including the wage structure of the sector and the local concentration of the sector's production linkages. In contrast, the Bartik approach assigns to each regional industry a multiplier proportional to its national rate of growth, precluding spillovers from induced (wage) and indirect (linkage) effects that could be estimated given a more flexible functional form. For example, if a location has the same employment shares in durable manufacturing and administrative services, and national employment is growing identically in both sectors, then the growth increment in the scalar Bartik measure contributed by each sector will be the same, even though the sectors likely have quite different multipliers in the local economy.

A compromise between the high-dimension, fully specified model and the scalar, just-identified Bartik instrument has been considered, but rarely implemented in the applied literature. Based on the random-shares view of identification, this approach involves using the full vector of industry shares as instruments and an IV estimator to determine weights. A regression-based approach is valid if innovations in regional labor supply are conditionally uncorrelated with past industry composition at the regional level. The issue discussed above regarding choice of shock exposures, that is employment vs export employment shares, would still apply. However, in the regression setting, we can frame the decision as the familiar trade-off between model goodness-of-fit and parsimony.

Detang-Dessendre, Partridge, and Piguet (2016) is the first instance of a regression-based instrument we find in the published urban and regional literature. The authors regress regional employment growth in France on local industry employment shares, and then use the predicted values from the regressions as instruments for employment growth. Because the instrument is created using OLS, it is by definition the best linear unbiased estimator of regional employment growth based on the local industry mix.[14]

The regression approach is conceptually similar to standard factor and synthetic control models found in the literature and straightforward to implement. First, the industry shares in the regression-based instrument are analogous to factor loadings, with the parameter estimates representing (nonorthogonal) factors. Second, the first-stage regressions, with shares lagged one period, can be seen as creating a weakly exogenous synthetic control. This is arguably preferable to common synthetic control construction methods that make a spatial exogeneity assumptions in constructing weights using neighboring areas. For these reasons, the regression-based instrument also represents a useful benchmark for comparing standard shift-share measures with some of the other popular methods of identifying exogenous demand shocks to regional economies that appear in the literature.

3.2.1 | Notation

In the results tables that follow, 18 shift-share measures are denoted using standardized nomenclature. The national shifter is listed first followed in parentheses by the local share variable and an indication whether the

[14]Practicioners should be aware that the over-identified 2SLS estimator is biased with many and weak instruments. A modified bias 2SLS estimator has appeared in the literature for use in the case, for details see Anatolyev (2013) and Kolesár et al. (2015).

share is fixed or updated. The national shift variables are "Employment," "Prices," "Regression," and "Wages." The local share variables are "Emp" for employment, "Exp" for export employment, and "Loc" for local service employment. The names of indexes that utilize export employment will include either "Exp-Asm" or "Exp-LQ," indicating that export employment was calculated using the assumption or location quotient methods, respectively. Finally, the names of indexes that utilize shares from a fixed base year have the abbreviation "Init" appended in parentheses, indicating the shares are from an initial year, and updated shares are denoted by "Upd." For example, the traditional Bartik estimator is denoted "Employment (Emp, Upd)" because the national growth rate is employment, the local shares are based on all employment ("Emp"), and the shares are updated each period ("Upd").

4 | DATA

In the following sections, we present the results of a tournament where we evaluate a number of plausibly exogenous shift-share instruments. The goal of this tournament is to evaluate which decisions involved in index construction are associated with the strongest empirical performance in terms of relevance and exogeneity. This section describes the data used in the tournament. This particular data set is designed to represent a common setting in which Bartik-type indicators are used in regional economics applications, that is an annual panel of county-level observations.[15]

The data set includes various local shares, national shifters, and locally endogenous variables. All data are available at the county level annually between 1990 and 2017. Sector shares and shifters are aggregated to the 3-digit sector level in the North American Industrial Classification System (NAICS).[16]

The Quarterly Census of Employment and Wages (QCEW) from the US Bureau of Labor Statistics is a major source of data for this study. The QCEW tabulates employment and wage data for workers covered by state and federal unemployment compensation programs, providing near-census coverage. We use these data to tabulate local employment shares and national growth rates for employment and earnings.

For national price shifters, we use producer price data from the Industry and Commodity series of the BLS's Producer Price Index (PPI). Because the QCEW data contain national industry employment, it is straightforward to link local employment shares to national price changes.[17]

House price index data are from the Federal Housing Finance Agency's county level data set as described in Bogin, Doerner, and Larson (2019). The authors use a weighted repeat-sales method and transaction data from Fannie Mae and Freddie Mac to construct annual constant-quality house price indexes at submetro geographies. Finally, data on population and housing units are from intercensal estimates produced by the US Census Bureau.

Table 1 presents summary statistics for the five locally endogenous variables and 18 demand instruments evaluated in this paper. The QCEW is the basis for balancing the panel, with year-on-year log differences spanning 1991 through 2017. After eliminating counties with populations less than 20,000 in 1990, 1,652 counties remain, of which 52% are within metropolitan statistical areas (38% center-city and 14% outlying), 33% are in micropolitan statistical areas, and 15% are classified as neither. Housing stock has lower time period coverage and house prices cover slightly fewer counties, giving fewer observations.

[15]We consider CBSA-level tests in Appendix B and obtain results are qualitatively similar. The shift-share approach has been used successfully with various levels of geographic aggregation. Counties are chosen as the unit of observation here rather than CBSAs or commuting zones because counties have stable boundaries, allow for within-metro variation, and provide a larger sample size, increasing the precision of our estimates. The possibility of local leakages and cross-hauling notwithstanding, we believe these benefits are compelling enough to focus on the county rather than these other common levels of aggregation.

[16]NAICS sectors go down to 6-digits in our data. However, values are suppressed in the publicly available files for sectors with small counts of employment. For this reason, we aggregate sectors to the 3-digit level, as this provides a good balance of granularity and coverage.

[17]For instance, NAICS code 236 is "Construction of Buildings" in the employment data, but no producer price exists at this level of aggregation. Instead, this is linked with NAICS 236211, "Nonresidential Building Construction" based on visual inspection. The price mapping file is available upon request.

TABLE 1 Summary statistics

	Counties	Mean (%)	*SD* (%)	CV	Source
Endogenous variables					
Employment	1,652	1.0	4.0	3.96	BLS–QCEW
Wages	1,652	2.9	3.5	1.18	BLS–QCEW
Housing stock	1,652	0.9	1.1	1.25	Census
Population	1,652	0.8	1.3	1.61	Census
House prices	1,646	2.6	4.8	1.83	FHFA (Bogin, Doerner, and Larson, 2019)
Potential instruments					
Employment (Emp, Upd)	1,652	1.2	2.2	1.74	Authors' Calculations (QCEW)
Employment (Exp-LQ, Upd)	1,652	0.9	2.6	2.83	Authors' Calculations (QCEW)
Employment (Exp-Asm, Upd)	1,652	0.2	4.2	21.21	Authors' Calculations (QCEW)
Employment (Exp-LQ, Init)	1,652	0.7	2.8	3.78	Authors' Calculations (QCEW)
Employment (Exp-Asm, Init)	1,652	0.1	4.4	35.52	Authors' Calculations (QCEW)
Employment (Loc-LQ, Upd)	1,652	1.5	2.0	1.35	Authors' Calculations (QCEW)
Prices (Exp-LQ, Upd)	1,652	1.8	2.0	1.11	Authors' Calculations (QCEW, PPI)
Prices (Exp-Asm, Upd)	1,652	2.3	2.5	1.09	Authors' Calculations (QCEW, PPI)
Prices (Exp-LQ, Init)	1,652	1.9	2.3	1.22	Authors' Calculations (QCEW, PPI)
Prices (Exp-Asm, Init)	1,652	2.3	2.7	1.18	Authors' Calculations (QCEW, PPI)
Wages (Exp-LQ, Upd)	1,652	3.0	1.3	0.43	Authors' Calculations (QCEW)
Wages (Exp-Asm, Upd)	1,652	3.1	1.4	0.44	Authors' Calculations (QCEW)
Wages (Exp-LQ, Init)	1,652	3.0	1.3	0.43	Authors' Calculations (QCEW)
Wages (Exp-Asm, Init)	1,652	3.1	1.4	0.46	Authors' Calculations (QCEW)
Regression (Exp-LQ, Upd)	1,652	0.9	2.3	2.58	Authors' Calculations (QCEW)
Regression (Emp, Upd)	1,652	0.9	2.2	2.37	Authors' Calculations (QCEW)
Regression (Exp-LQ, Init)	1,652	0.9	2.4	2.51	Authors' Calculations (QCEW)
Regression (Emp, Init)	1,652	1.0	2.2	2.30	Authors' Calculations (QCEW)

Note: The table presents summary statistics from a balanced panel of US counties, with the exception of house prices and housing stock, which are presented for all available periods. This sample consists of 28 years of annual data (1990 through 2017) over the 1,652 counties with a population of more than 20,000 in 1990 and a value for employment in every period. This gives a total of 44,604 growth rate observations (1991 through 2017), 42,993 observations for house prices, and 28,084 observations for housing stock (2001 through 2017).

The mean rates of change in employment, population, and housing stock are approximately 1% per year. Rates of change for the local nominal price variables, house prices and wages, are around 2.75%. As expected, the mean values for instruments calculated using the national series of these variables are quite similar. However, the local variable is more variable than the corresponding instrument. While house prices grow at rates and with variability that are comparable to growth in wages, housing stock and population grow more slowly and with less variation across counties.

We calculate sector shares for three categories of employment: gross employment, export employment based on location quotients, and export employment classified by assumption. Figure 1 shows the variation in shares present in an average location using each of these three methods. After sector shares are calculated for each county-year and averaged for each county across years, the shares are sorted and ranked from highest to lowest. Based on how broadly sector shares are spread across ranks, the figure shows that classic gross employment shares exhibit the lowest levels of concentration and the export share-assumption (Exp-Asm) method gives the most concentrated shares. The export share-location quotient (Exp-LQ) method occupies the middle.

The dispersion in shares is potentially relevant for instrument validity because according to Borusyak et al. (2018), shock exposure must be sufficiently spread across industries to be considered as good as randomly assigned. Because the indexes based on export employment use a smaller number of more volatile sectors, they are also prone to greater fluctuations than indexes based on overall employment shares, a characteristic confirmed in Table 1. While the traditional Bartik instrument has a coefficient of variation (CV) of 1.7, as expected based on the

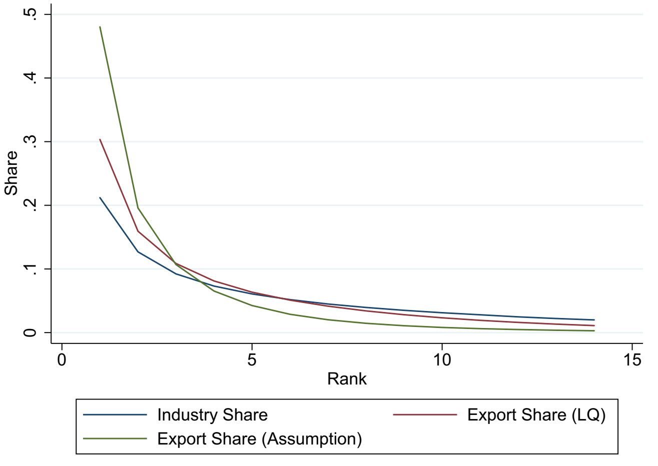

FIGURE 1 Weight share distributions. The rank-order county-average shares of the three weighting schemes used in index construction are shown. Industry shares are calculated using private county-level employment using 3-digit NAICS industry sectors. Export shares are calculated as described in the text, using the subset of employment used as inputs in the production of goods and services for export outside of the county [Color figure can be viewed at wileyonlinelibrary.com]

arguments above, the CV for the export employment versions is higher, ranging from 2.9 to 35.5, and value of 1.4 for the local service measure is lower.

As shown in Table 2, the demand instruments are all positively correlated, and the range is from 0.07 to 0.96. The size of the range suggests they are capturing different information, both across different shares for the same shift variable, and for different shift variables for common shares. For instance, comparing the traditional Bartik instrument in [1] with the Employment (Exp-LQ, Init) shown in [4], the correlation is 0.9, indicating these variables are highly similar. In contrast, comparing [4] with [9] and [13], the correlation between the same shares with different shifters is only about 0.25 for both pairwise combinations, and the correlation between [9] and [13] is just 0.09. The correlation among the four regression instruments is high, as expected. Across the measures, updating shares weakens correlations by approximately 0.12. Differences between demand indicators based on total employment versus export employment shares appear quite small. Overall, it seems the choice of shifters is responsible for a greater degree of variation across measures than the choice of shares.

5 | TOURNAMENT

We next compare the empirical performance of the standard Bartik measure and alternative shift-share instruments introduced in Section 3 that can be used to identify local demand shocks. This tournament consists of four contests: (a) individual instrument relevance, (b) model encompassing for relative explanatory power, (c) employment share endogeneity, and (d) point estimates of employment shocks.

Round one tests the correlation between 18 instruments and five endogenous variables over short-run and medium-run time periods. Passing this test indicates a measure likely satisfies the relevance requirement for instrument validity. Round two takes two of the more successful classes of instruments from round one and examines whether an instrument within each class contains unique and relevant explanatory power relative to the others. This round addresses two issues raised in the previous section: choice of shocks and choice of shock exposures. Round three performs overidentification tests to show which employment sectors are more likely to reject the null of exogeneity. This round examines implications of using the entire vector of employment shares as

TABLE 2 Instrument correlations, 1-year panel

Instrument	Name	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
[1]	Employment (Emp, Upd)	1.00	0.95	0.94	0.90	0.91	0.95	0.26	0.12	0.25
[2]	Employment (Exp-LQ, Upd)	0.95	1.00	0.91	0.92	0.87	0.82	0.27	0.14	0.26
[3]	Employment (Exp-Asm, Upd)	0.94	0.91	1.00	0.88	0.96	0.89	0.28	0.17	0.28
[4]	Employment (Exp-LQ, Init)	0.90	0.92	0.88	1.00	0.91	0.80	0.24	0.13	0.24
[5]	Employment (Exp-Asm, Init)	0.91	0.87	0.96	0.91	1.00	0.87	0.27	0.16	0.27
[6]	Employment (Loc-LQ, Upd)	0.95	0.82	0.89	0.80	0.87	1.00	0.22	0.07	0.22
[7]	Prices (Exp-LQ, Upd)	0.26	0.27	0.28	0.24	0.27	0.22	1.00	0.70	0.86
[8]	Prices (Exp-Asm, Upd)	0.12	0.14	0.17	0.13	0.16	0.07	0.70	1.00	0.65
[9]	Prices (Exp-LQ, Init)	0.25	0.26	0.28	0.24	0.27	0.22	0.86	0.65	1.00
[10]	Prices (Exp-Asm, Init)	0.12	0.15	0.17	0.15	0.18	0.08	0.64	0.92	0.69
[11]	Wages (Exp-LQ, Upd)	0.26	0.23	0.28	0.24	0.27	0.27	0.08	0.08	0.07
[12]	Wages (Exp-Asm, Upd)	0.30	0.28	0.35	0.27	0.33	0.31	0.22	0.21	0.21
[13]	Wages (Exp-LQ, Init)	0.27	0.25	0.29	0.25	0.29	0.28	0.08	0.07	0.09
[14]	Wages (Exp-Asm, Init)	0.30	0.27	0.34	0.27	0.34	0.30	0.22	0.20	0.23
[15]	Regression (Exp-LQ, Upd)	0.69	0.68	0.69	0.63	0.65	0.64	0.25	0.14	0.23
[16]	Regression (Emp, Upd)	0.71	0.67	0.69	0.63	0.66	0.68	0.25	0.13	0.23
[17]	Regression (Exp-LQ, Init)	0.67	0.66	0.67	0.63	0.65	0.64	0.25	0.13	0.24
[18]	Regression (Emp, Init)	0.70	0.66	0.68	0.64	0.66	0.67	0.25	0.13	0.24
Instrument	**Name**	**[10]**	**[11]**	**[12]**	**[13]**	**[14]**	**[15]**	**[16]**	**[17]**	**[18]**
[1]	Employment (Emp, Upd)	0.12	0.26	0.30	0.27	0.30	0.69	0.71	0.67	0.70
[2]	Employment (Exp-LQ, Upd)	0.15	0.23	0.28	0.25	0.27	0.68	0.67	0.66	0.66
[3]	Employment (Exp-Asm, Upd)	0.17	0.28	0.35	0.29	0.34	0.69	0.69	0.67	0.68
[4]	Employment (Exp-LQ, Init)	0.15	0.24	0.27	0.25	0.27	0.63	0.63	0.63	0.64
[5]	Employment (Exp-Asm, Init)	0.18	0.27	0.33	0.29	0.34	0.65	0.66	0.65	0.66
[6]	Employment (Loc-LQ, Upd)	0.08	0.27	0.31	0.28	0.30	0.64	0.68	0.64	0.67
[7]	Prices (Exp-LQ, Upd)	0.64	0.08	0.22	0.08	0.22	0.25	0.25	0.25	0.25
[8]	Prices (Exp-Asm, Upd)	0.92	0.08	0.21	0.07	0.20	0.14	0.13	0.13	0.13
[9]	Prices (Exp-LQ, Init)	0.69	0.07	0.21	0.09	0.23	0.23	0.23	0.24	0.24
[10]	Prices (Exp-Asm, Init)	1.00	0.08	0.20	0.08	0.22	0.13	0.12	0.13	0.12
[11]	Wages (Exp-LQ, Upd)	0.08	1.00	0.86	0.93	0.80	0.33	0.33	0.32	0.33
[12]	Wages (Exp-Asm, Upd)	0.20	0.86	1.00	0.84	0.93	0.36	0.37	0.36	0.37
[13]	Wages (Exp-LQ, Init)	0.08	0.93	0.84	1.00	0.87	0.33	0.33	0.33	0.34
[14]	Wages (Exp-Asm, Init)	0.22	0.80	0.93	0.87	1.00	0.35	0.36	0.35	0.36
[15]	Regression (Exp-LQ, Upd)	0.13	0.33	0.36	0.33	0.35	1.00	0.96	0.86	0.84
[16]	Regression (Emp, Upd)	0.12	0.33	0.37	0.33	0.36	0.96	1.00	0.86	0.87
[17]	Regression (Exp-LQ, Init)	0.13	0.32	0.36	0.33	0.35	0.86	0.86	1.00	0.97
[18]	Regression (Emp, Init)	0.12	0.33	0.37	0.34	0.36	0.84	0.87	0.97	1.00

Note: The table presents correlations from a balanced sample of shift-share instrument observations. This sample consists of 28 years of annual data (1990 through 2017) over the 1,652 counties with a population of more than 20,000 in 1990 and a value for employment in every period. This gives a total of 44,604 instrument observations (1991 through 2017).

instruments. Finally, round four estimates effects of employment changes on five endogenous variables using each instrument in sequence. This round culminates the tournament. In it, we close by examining parameter consistency by including in our estimation models variables potentially correlated with innovations in labor supply. Comparing scalar and regression approaches, the fourth round also addresses the final issue raised in the previous section concerning the restrictions inherent in the inner-product structure of shift-share instruments.

5.1 | Relevance

Our model for assessing instrument relevance includes two-way (county and year) fixed effects, α_j and α_t, respectively, and an individual shift-share demand indicator, Z_{jt},

$$\Delta y_{jt} = \alpha_j + \alpha_t + \beta Z_{jt} + C(L)\Delta y_{jt} + \epsilon_{jt}, \tag{11}$$

where Δy_{jt} is the log change in employment, wages, housing stock, population, or house prices, alternately, and the operator, $C(L)$, gives a sequence of lags and coefficients that are optimized for each of the locally endogenous variables we model.[18,19]

Each of the models explicitly equates the shock indexed by the plausibly exogenous measure with a shock in demand for the respective endogenous variable. The regressions with employment as both the endogenous variable and the shifter utilized in the instrument, for example, represent a hypothetical first-stage regression in a 2SLS model regressing employment growth on some outcome variable, Y, in the second stage. Under this interpretation, significance with $p < .01$ or lower is necessary to satisfy a test of instrument relevance. For the other dependent variables, tests examine whether the employment instrument will have relevant explanatory power when passed through a different treatment variable, though the power in the second stage will be weaker due to the projection onto a second variable.

Tables 3 and 4 show results from these approximate first-stage models. In the 1-year panel, the results with local employment and wages as dependent variables indicate all instruments are potentially relevant, with each estimate rejecting the null of no effect with p values all lower than .01. In the 5-year panel, results weaken for wage instruments, but all other instruments maintain significance for both local employment and wages.

In employment models, regression instruments (based on export employment shares) have the highest R^2 values and for wage models employment instruments perform best. Focusing on just the employment-based instruments, recall from Section 3 that our modifications to the standard Bartik measure allow us to separate broader shocks into export and local service components for comparison purposes. The estimates on the employment instruments in the employment and wage models show that export employment measures have more statistical power than the standard all-employment Bartik instrument—denoted "Employment (Emp, Upd)" in results tables—and the local service has less. Differences in power across indicators based on choice of employment shares appear small absolutely and relative to differences across shifters in our specification.

Housing stock and population present a challenge for these instruments in univariate specifications. In the 1-year panel, using employment instruments, shares require updating to achieve potential relevance. No wage instruments and only one price instrument survives (using export employment with updated location quotients). The regression instruments are all potentially relevant. In the 5-year panel, all instruments give the wrong sign in housing stock models and only the employment and regression instruments are relevant in population models.

[18]Lag order is based on a recursive procedure where sequence of models is estimated with varying lag lengths, n. The value for n is adjusted up or down until both the nth lag is significant in a model with n lags and the $n + 1$th lag is not significant in the model with $n + 1$ lags. For the 1-year panel, this results in 0 lags for wages, 1 lag for employment and housing stock, and 2 lags for population and house prices. For the 5-year panel, this results in 0 lags for employment, wages, and house prices, and 1 lag for housing stock and population.

[19]This specification potentially introduces Nickell (1981) bias. We consider models without fixed effects in Appendix A.

TABLE 3 Univariate models, 1-year panel

$\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \beta z_{jt} + u_{jt}$

Dependent variable	Employment			Wages			Housing stock		
National shift variable	β	SE	R^2	β	SE	R^2	β	SE	R^2
Employment (Emp, Upd)	.982***	(0.059)	.377	.602***	(0.055)	.111	.027***	(0.010)	.845
Employment (Exp-LQ, Upd)	.512***	(0.025)	.379	.331***	(0.024)	.114	.014***	(0.004)	.845
Employment (Exp-Asm, Upd)	.356***	(0.021)	.373	.242***	(0.020)	.112	.011***	(0.003)	.845
Employment (Exp-LQ, Init)	.424***	(0.024)	.372	.260***	(0.021)	.109	.006*	(0.003)	.844
Employment (Exp-Asm, Init)	.272***	(0.018)	.366	.177***	(0.016)	.107	.002	(0.003)	.844
Employment (Loc-LQ, Upd)	1.055***	(0.100)	.353	.508***	(0.085)	.099	.024	(0.017)	.844
Prices (Exp-LQ, Upd)	.137***	(0.019)	.351	.118***	(0.020)	.100	.011***	(0.003)	.845
Prices (Exp-Asm, Upd)	.147***	(0.020)	.351	.147***	(0.024)	.101	.002	(0.003)	.844
Prices (Exp-LQ, Init)	.087***	(0.017)	.350	.080***	(0.019)	.099	−.001	(0.002)	.844
Prices (Exp-Asm, Init)	.104***	(0.018)	.351	.108***	(0.021)	.100	−.003	(0.002)	.844
Wages (Exp-LQ, Upd)	.330***	(0.048)	.352	.582***	(0.046)	.109	−.006	(0.007)	.844
Wages (Exp-Asm, Upd)	.252***	(0.045)	.351	.393***	(0.044)	.105	−.015**	(0.007)	.844
Wages (Exp-LQ, Init)	.299***	(0.040)	.351	.488***	(0.038)	.106	−.012**	(0.006)	.844
Wages (Exp-Asm, Init)	.215***	(0.032)	.351	.315***	(0.033)	.103	−.007	(0.005)	.844
Regression (Exp-LQ, Upd)	.686***	(0.033)	.402	.245***	(0.046)	.106	.020***	(0.005)	.845
Regression (Emp, Upd)	.768***	(0.037)	.399	.249***	(0.048)	.104	.019***	(0.005)	.845
Regression (Exp-LQ, Init)	.679***	(0.027)	.401	.268***	(0.030)	.108	.021***	(0.004)	.845
Regression (Emp, Init)	.747***	(0.032)	.394	.272***	(0.035)	.105	.015***	(0.004)	.845
Observations:	Obs: 42,952, Counties: 1,652			Obs: 44,604, Counties: 1,652			Obs:26,432, Counties: 1,652		

Dependent variable:	Population			House prices		
National shift variable:	β	SE	R^2	β	SE	R^2
Employment (Emp, Upd)	.063***	(0.011)	.756	.235***	(0.049)	.553
Employment (Exp-LQ, Upd)	.035***	(0.005)	.757	.128***	(0.027)	.553
Employment (Exp-Asm, Upd)	.021***	(0.004)	.756	.113***	(0.022)	.553
Employment (Exp-LQ, Init)	.028***	(0.004)	.756	.081***	(0.024)	.552
Employment (Exp-Asm, Init)	.015***	(0.003)	.756	.041**	(0.020)	.552
Employment (Loc-LQ, Upd)	.119***	(0.018)	.756	.326***	(0.099)	.552
Prices (Exp-LQ, Upd)	−.001	(0.003)	.755	.090***	(0.024)	.553
Prices (Exp-Asm, Upd)	−.005	(0.004)	.755	.048*	(0.026)	.552
Prices (Exp-LQ, Init)	−.006*	(0.003)	.755	.036*	(0.019)	.552
Prices (Exp-Asm, Init)	−.004	(0.003)	.755	.011	(0.02)	.552
Wages (Exp-LQ, Upd)	−.003	(0.008)	.755	.139***	(0.052)	.552
Wages (Exp-Asm, Upd)	−.022***	(0.007)	.755	.001	(0.044)	.552
Wages (Exp-LQ, Init)	−.010	(0.007)	.755	.093**	(0.044)	.552
Wages (Exp-Asm, Init)	−.018***	(0.006)	.755	.052	(0.035)	.552
Regression (Exp-LQ, Upd)	.049***	(0.005)	.758	.212***	(0.025)	.555
Regression (Emp, Upd)	.051***	(0.006)	.757	.206***	(0.028)	.554

(Continues)

TABLE 3 (Continued)

Dependent variable:	Population			House prices		
National shift variable:	β	SE	R^2	β	SE	R^2
Regression (Exp-LQ, Init)	.052***	(0.005)	.758	.188***	(0.025)	.555
Regression (Emp, Init)	.051***	(0.005)	.757	.138***	(0.03)	.553
Observations:	Obs: 41,300, Counties: 1,652			Obs: 39,693, Counties: 1,646		

Note: The table presents three estimated parameters from a sequence of models: the $\hat{\beta}$ relating the instrument in the row to the change in the locally endogenous variable in the column; the standard error of the $\hat{\beta}$, and the fit of the model R^2. The operator $C(L)$ gives a sequence of lags and coefficients that are optimized for each locally endogenous variable: Earnings, 0 lags; Employment and Housing Stock, 1 lag; Population and House Prices, 2 lags. Samples are identical in each model for each dependent variable. Robust standard errors (clustered by State × Year) in brackets.

*$p < .1$.

**$p < .05$.

***$p < .01$.

House prices have more instruments that strongly predict variation than population and the housing stock, but fewer than employment and wages. Export employment-location quotient instruments seem to perform best. Wage index models, in the words of Guerrieri et al. (2013), act as tests of "local income shocks [as] local housing demand shocks" (p. 55), though the effects are mixed here: wage indexes are positively related to year-on-year house price changes but not significant for 5-year models, and either negatively or not significantly related in housing stock models.

Overall, we observe several main findings across these models of instrument relevance. First, employment and regression instruments have strong conditional correlations with every endogenous variable considered, indicating first-stage relevance seems likely in most applications. Second, while price and wage instruments may be strong in terms of modeling employment and wages, they do not perform well in predicting other important locally endogenous variables observed by the researchers, that is population, housing prices, and the housing stock. The strongest wage and price instruments involve export employment shares calculated using the location-quotient approach that are updated every period. Finally, nearly all instruments tend to have more power in 1-year panels versus 5-year panels.

5.2 | Encompassing

The results in Section 5.1 show that several alternative shift-share demand indicators are conditionally correlated with five locally endogenous variables, meeting the relevance requirement of a valid instrument. We next see if our empirical analysis can answer two questions posed in Section 3 regarding alternative constructions: (a) Does employment growth represent the most appropriate operationalization of the demand shock construct? and (b) Can performance of shift-share instruments be improved by incorporating information on the export-orientation of industries?

To address these issues, we compare two groups of instruments that perform well in the univariate relevance testing and that vary along the dimensions of shock and shock exposure. Instead of individual instrument relevance, we now assess relative explanatory power in a multivariate framework. Our approach involves a simple modification to Equation (11) that replaces the single instrument, Z_{jt}, with a vector of instruments, $\boldsymbol{Z_{jt}}$

$$\Delta y_{jt} = \alpha_j + \alpha_t + \boldsymbol{Z}'_{jt}\boldsymbol{\beta} + C(L)\Delta y_{jt} + \epsilon_{jt}, \tag{12}$$

These model encompassing tests allow us to determine if any candidate measure in the vector dominates the others, or if they each contain unique and relevant information.

TABLE 4 Univariate models, 5-year panel

$\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \beta z_{jt} + u_{jt}$

Dependent variable	Employment			Wages			Housing stock		
National shift variable	β	SE	R^2	β	SE	R^2	β	SE	R^2
Employment (Emp, Upd)	.943***	(0.078)	.585	.591***	(0.065)	.261	−.146***	(0.037)	.802
Employment (Exp-LQ, Upd)	.518***	(0.040)	.588	.332***	(0.033)	.267	−.077***	(0.018)	.803
Employment (Exp-Asm, Upd)	.342***	(0.033)	.578	.223***	(0.025)	.256	−.080***	(0.017)	.803
Employment (Exp-LQ, Init)	.477***	(0.041)	.582	.344***	(0.032)	.267	−.061**	(0.024)	.801
Employment (Exp-Asm, Init)	.292***	(0.030)	.576	.198***	(0.022)	.254	−.041**	(0.020)	.800
Employment (Loc-LQ, Upd)	.723***	(0.146)	.562	.512***	(0.118)	.237	−.359***	(0.102)	.802
Prices (Exp-LQ, Upd)	.264***	(0.051)	.566	.178***	(0.047)	.241	−.152***	(0.041)	.807
Prices (Exp-Asm, Upd)	.218***	(0.049)	.565	.181***	(0.043)	.245	−.084**	(0.033)	.802
Prices (Exp-LQ, Init)	.173***	(0.050)	.563	.142***	(0.042)	.240	−.134***	(0.027)	.808
Prices (Exp-Asm, Init)	.165***	(0.046)	.564	.150***	(0.036)	.244	−.078***	(0.029)	.803
Wages (Exp-LQ, Upd)	.200**	(0.093)	.560	.542***	(0.080)	.252	−.220***	(0.061)	.802
Wages (Exp-Asm, Upd)	.065	(0.090)	.560	.352***	(0.069)	.242	−.186**	(0.079)	.801
Wages (Exp-LQ, Init)	.219**	(0.092)	.561	.442***	(0.073)	.247	−.263***	(0.059)	.805
Wages (Exp-Asm, Init)	.096	(0.083)	.560	.311***	(0.060)	.242	−.215***	(0.065)	.804
Regression (Exp-LQ, Upd)	.857***	(0.041)	.614	.411***	(0.038)	.268	−.092***	(0.029)	.802
Regression (Emp, Upd)	1.008***	(0.054)	.607	.462***	(0.052)	.261	−.066*	(0.038)	.799
Regression (Exp-LQ, Init)	.858***	(0.052)	.593	.466***	(0.051)	.261	−.072*	(0.037)	.799
Regression (Emp, Init)	1.032***	(0.064)	.594	.511***	(0.063)	.257	−.093**	(0.046)	.799
Observations:	Obs: 8,260, Counties: 1,652			Obs: 8,260, Counties: 1,652			Obs: 3,304, Counties: 1,652		

Dependent variable	Population			House prices		
National shift variable	β	SE	R^2	β	SE	R^2
Employment (Emp, Upd)	.065**	(0.031)	.801	.471***	(0.165)	.467
Employment (Exp-LQ, Upd)	.060***	(0.018)	.802	.313***	(0.094)	.469
Employment (Exp-Asm, Upd)	.057***	(0.011)	.803	.148*	(0.086)	.466
Employment (Exp-LQ, Init)	.056***	(0.018)	.802	.166*	(0.093)	.465
Employment (Exp-Asm, Init)	.049***	(0.011)	.802	.060	(0.081)	.464
Employment (Loc-LQ, Upd)	−.070	(0.059)	.801	−.073	(0.310)	.464
Prices (Exp-LQ, Upd)	−.002	(0.020)	.800	.178*	(0.102)	.465
Prices (Exp-Asm, Upd)	−.003	(0.019)	.800	.017	(0.103)	.464
Prices (Exp-LQ, Init)	−.032*	(0.018)	.801	.079	(0.084)	.464
Prices (Exp-Asm, Init)	−.007	(0.018)	.800	.002	(0.082)	.464
Wages (Exp-LQ, Upd)	.021	(0.033)	.800	.003	(0.213)	.464
Wages (Exp-Asm, Upd)	−.077**	(0.035)	.801	−.236	(0.196)	.465
Wages (Exp-LQ, Init)	−.020	(0.031)	.800	−.264	(0.198)	.465
Wages (Exp-Asm, Init)	−.066**	(0.029)	.801	−.194	(0.162)	.465
Regression (Exp-LQ, Upd)	.099***	(0.024)	.803	.665***	(0.122)	.481
Regression (Emp, Upd)	.067**	(0.028)	.801	.745***	(0.143)	.477

(Continues)

TABLE 4 (Continued)

Dependent variable	Population			House prices		
National shift variable	*β*	*SE*	R^2	*β*	*SE*	R^2
Regression (Exp-LQ, Init)	.133***	(0.032)	.803	.881***	(0.174)	.482
Regression (Emp, Init)	.103**	(0.043)	.801	1.013***	(0.207)	.480
Observations:	Obs: 6,608, Counties: 1,652			Obs: 7,731, Counties: 1,646		

Note: The table presents three estimated parameters from a sequence of models: the $\hat{\beta}$ relating the instrument in the row to the change in the locally endogenous variable in the column; the standard error of the $\hat{\beta}$, and the fit of the model R^2. The operator $C(L)$ gives a sequence of lags and coefficients that are optimized for each locally endogenous variable: Employment, Earnings, and House Prices, 0 lags; Housing Stock and Population, 1 lag. Samples are identical in each model for each dependent variable. Robust standard errors (clustered by State × Year) in brackets.
*$p < .1$.
**$p < .05$.
***$p < .01$.

We begin by addressing choice of shocks. Table 5 shows estimates from models that include three shift-share instruments based on changes in national employment, producer prices, and wages. All three instruments utilize export employment shares based on the location quotient method. The dependent variables are the same as in the univariate relevance testing: employment, wages, housing stock, population, and house prices, all in log changes. Panel A contains the results for 1-year changes in the demand instruments and endogenous variables, and Panel B contains results for 5-year changes. For year-on-year changes, the coefficient on the employment index is positive and significant in each model, prices in four, and wages in two. For 5-year changes, the coefficient on the employment index is positive and significant in four models, prices in three, and wages in just one (the own variable case). The employment instrument, although it does not dominate, seems the most powerful, and the wage index, although it is not dominated, the least. While there may be theoretical justification for using changes in producer prices or wages in creating shift-share instruments, we find no empirical evidence to suggest that either is a superior operationalization of the demand shock construct. Our main finding here is that instruments using changes in employment, wages, and producer prices as shocks all appear to contain orthogonal explanatory information across different models. Rather than a single "best" instrument, our results suggest the use of overidentification.

We next turn to the choice of shock exposures. In Table 6, we hold the growth series constant, using employment, and test the effects of instruments constructed using alternative local employment shares. The vector $\boldsymbol{Z}_{jt}$ in Equation (12) now includes six different formulations: (1) standard all-employment shares updated each time period (Emp, Upd); (2–3) export employment shares based on location quotients, updated (Exp-LQ, Upd) and calculated in an initial period (Exp-LQ, Init); (4–5) export employment shares based on the assumption method, updated (Exp-Asm, Upd) and initial period (Exp-Asm, Init); and (6) local service employment shares based on location quotients, updated (Loc-LQ, Upd). As before we present results for 1-year changes in Panel A and 5-year changes in Panel B.

At the outset, we show the standard Bartik instrument, "Employment (Emp, Upd)," with negative and significant effects across models, is empirically dominated by alternative constructions in both the short-run and intermediate-run. Because the effects of the standard measure are positive and significant in the previous univariate models in Tables 3 and 4, this result suggests it is multicollinear with and encompassed by the other indexes.

For 1-year changes, Model (1) for employment shows all but one of the export-weighted variables contributing positive and statistically significant explanatory power.[20] Updated shares and shares based on location quotients

[20]Robustness tests in Appendix A suggest the price instrument has low power in dense areas where presumably there is more service employment and less variation in producer prices. In this appendix, we also consider other specifications including the presence of fixed effects. Results are generally robust.

TABLE 5 Model encompassing tests, alternative shifters

(A) Dependent variable: 1-year log-difference of variable shown in column head (Δy_t)					
Equation	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**
Dependent variable	**Employment**	**Wages**	**Housing stock**	**Population**	**House prices**
$y_{j,t-1}$	0.117*** [0.0184]		0.693*** [0.0248]	0.439*** [0.0138]	0.420*** [0.0354]
$y_{j,t-2}$				0.135*** [0.0112]	0.0824** [0.0324]
Employment (Exp-LQ, Upd)	0.494*** [0.0238]	0.303*** [0.0204]	0.0133*** [0.00416]	0.0366*** [0.00512]	0.114*** [0.0272]
Prices (Exp-LQ, Upd)	0.0708*** [0.0160]	0.0342* [0.0179]	0.00858** [0.00358]	−0.003 [0.00374]	0.0693*** [0.0242]
Wages (Exp-LQ, Upd)	0.214*** [0.0387]	0.502*** [0.0373]	−0.0166** [0.00798]	−0.009 [0.00754]	0.0948* [0.0570]
Observations	43,140	44,813	26,594	41,521	39,859
R-squared	0.375	0.124	0.837	0.752	0.555
(B) Dependent variable: 5-year log-difference of variable shown in column head (Δy_t)					
Equation	**[6]**	**[7]**	**[8]**	**[9]**	**[10]**
Dependent variable	**Employment**	**Wages**	**Housing stock**	**Population**	**House prices**
$y_{j,t-1}$			0.214*** [0.0735]	0.188*** [0.0453]	
Employment (Exp-LQ, Upd)	0.489*** [0.0392]	0.296*** [0.0285]	−0.0598*** [0.0162]	0.0598*** [0.0172]	0.292*** [0.0949]
Prices (Exp-LQ, Upd)	0.142*** [0.0448]	0.064 [0.0399]	−0.106** [0.0518]	−0.014 [0.0201]	0.123 [0.102]
Wages (Exp-LQ, Upd)	0.022 [0.0883]	0.445*** [0.0692]	−0.062 [0.0820]	0.020 [0.0330]	−0.092 [0.221]
Observations	8,344	8,344	3,326	6,642	7,764
R-squared	0.596	0.283	0.809	0.801	0.47

Note: The model estimated is: $\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \mathbf{d_{jt}}'\gamma + \epsilon_{jt}$, where Δy is the log change in the variable listed in the column head, **d** is the vector instrumental variables in the rows, and the operator C(L) gives a sequence of lags and coefficients that are optimized for each locally endogenous variable. The parameter estimates for $\boldsymbol{\gamma}$ are presented; the others are omitted for brevity but available upon request. Robust standard errors (clustered by State × Year) in brackets.
*$p < 0.1$.
**$p < .05$.
***$p < .01$.

appear to have more unique and relative information than initial period shares and shares based on the assumption method. In Model (2) for wages, only the two instruments with updated export employment shares have positive and significant effects. Of the two instruments, the coefficient on the location quotient shares measure has a larger t statistic than the measure with assumption method shares.

In the models estimating housing stock, only the instrument with updated assumption method shares appears correlated. For population and house price models, the local service measure—which we present as a comparison device and not as a serious instrument—is unexpectedly predictive. We conjecture that population and house prices may have associations with endogenous amenity sectors (e.g., entertainment, leisure, and recreation) that may have national trends indicative of innovations to consumption patterns.

For 5-year changes, the updated export employment instrument based on location quotients clearly outperforms, with positive and significant coefficients in predicting four of the five locally endogenous variables.

TABLE 6 Model encompassing tests, alternative shares

(A) Dependent variable: 1-year log-difference of variable shown in column head (Δy_t)					
Equation	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**
Dependent variable	**Employment**	**Wages**	**Housing stock**	**Population**	**House prices**
$y_{j,t-1}$	0.127*** [0.0189]		0.707*** [0.0228]	0.450*** [0.0133]	0.418*** [0.0355]
$y_{j,t-2}$				0.128*** [0.0110]	0.0812** [0.0325]
Employment (Emp, Upd)	−0.140 [0.149]	−0.375** [0.156]	−0.00759 [0.0335]	−0.0996*** [0.0277]	−0.434** [0.193]
Employment (Exp-LQ, Upd)	0.374*** [0.0644]	0.375*** [0.0621]	0.013 [0.0120]	0.0657*** [0.0124]	0.182* [0.103]
Employment (Exp-LQ, Init)	0.0992*** [0.0342]	0.020 [0.0286]	0.001 [0.00503]	0.009 [0.00687]	0.054 [0.0410]
Employment (Exp-Asm, Upd)	0.127*** [0.0303]	0.131*** [0.0269]	0.0155*** [0.00378]	0.006 [0.00523]	0.200*** [0.0385]
Employment (Exp-Asm, Init)	−0.0085 [0.0248]	−0.011 [0.0218]	−0.0126*** [0.00332]	−0.00352 [0.00491]	−0.129*** [0.0331]
Employment (Loc-LQ, Upd)	0.207 [0.130]	0.0781 [0.127]	0.00118 [0.0281]	0.106*** [0.0262]	0.337** [0.140]
Observations	42,952	44,604	26,432	41,300	39,696
R-squared	0.381	0.116	0.845	0.757	0.554
(B) Dependent variable: 5-year log-difference of variable shown in column head (Δy_t)					
Equation	**[6]**	**[7]**	**[8]**	**[9]**	**[10]**
Dependent variable	**Employment**	**Wages**	**Housing stock**	**Population**	**House prices**
$y_{j,t-1}$			0.194*** [0.0737]	0.184*** [0.0457]	
Employment (Emp, Upd)	0.113 [0.202]	−0.179 [0.166]	0.313** [0.152]	−0.410*** [0.0958]	– 0.642 [0.481]
Employment (Exp-LQ, Upd)	0.343*** [0.108]	0.230*** [0.0854]	−0.142** [0.0716]	0.208*** [0.0471]	0.745*** [0.255]
Employment (Exp-LQ, Init)	0.082 [0.0658]	0.176*** [0.0537]	−0.009 [0.0274]	0.003 [0.0247]	−0.144 [0.122]
Employment (Exp-Asm, Upd)	0.048 [0.0524]	0.058 [0.0519]	−0.0927*** [0.0253]	0.0502*** [0.0187]	0.128 [0.103]
Employment (Exp-Asm, Init)	0.057 [0.0471]	0.00725 [0.0466]	0.0249 [0.0229]	0.0119 [0.0159]	−0.126 [0.0972]
Employment (Loc-LQ, Upd)	−0.268 [0.179]	0.0161 [0.154]	−0.427*** [0.126]	−0.024 [0.0698]	−0.417 [0.430]
Observations	8,260	8,260	3,304	6,608	7,731
R-squared	0.59	0.272	0.807	0.805	0.473

Note: The model estimated is: $\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \mathbf{d}'_{jt}\gamma + \epsilon_{jt}$, where Δy is the log change in the variable listed in the column head, **d** is the vector instrumental variables in the rows, and the operator $C(L)$ gives a sequence of lags and coefficients that are optimized for each locally endogenous variable. The parameter estimates for $\boldsymbol{\gamma}$ are presented; the others are omitted for brevity but available upon request. Robust standard errors (clustered by State × Year) in brackets.

*$p < .1$.

**$p < .05$.

***$p < .01$.

The only other constructions that appear to contain independent information are the standard Bartik measure in predicting housing stock and the assumption based export employment instrument in predicting population.

Our primary finding from this testing for relative explanatory power is that for instruments that index employment changes, those that base shock exposure on export employment consistently perform best in predicting local potentially endogenous variables. These results imply that whatever predictive power is lost from omitting nontraded industries in instrument construction is more than offset by increased variability in traded sectors. These estimates suggest that the traditional Bartik index, so common in the literature, is empirically dominated by an index with a weighting strategy based on export employment shares that is nearly costless to implement.

5.3 | Exogeneity

While the testing thus far has evaluated alternative instrument constructions in terms of explanatory power in simulated first stage regressions, we now turn to the second, and arguably more important, requirement of a valid instrument, that the measure be uncorrelated with the structural error term. Testing for exogeneity is a well known challenge. While some informal procedures exist—see Goldsmith-Pinkham et al. (2018) for various examples—the most common formal approach consists of tests of overidentification restrictions. Overidentification tests require the model to include more instruments than endogenous variables. However, as we show in the encompassing round, alternative shift-share instruments often contain similar information, making multiple instruments difficult to test directly. Rather than continuing to pit instruments head-to-head, in this round of the tournament we instead focus on the exogeneity of the local industry employment shares used in instrument construction. This approach is based on the random-shares view of identification discussed in Section 2.2 and involves two steps.

The first step entails estimating a sequence of 2SLS models of wage growth on employment change using different pairs of employment shares as instruments. We pick one presumably exogenous sector share to fix and then vary the second member of the pair across models. There are 90 3-digit NAICS sectors in our sample. As the fixed instrument, we choose NAICS sector 335, "Electrical Equipment, Appliance, and Component Manufacturing" due to consistent results suggesting exogeneity in trial pairwise tests. We use shares from 1990 to estimate models with samples beginning in 2000 to ensure the shares are not reflective of current or recent shocks.

Our estimating system is again a two-way fixed effects model with log employment change as the treatment variable and, new in this round of testing, a vector of control variables ($\boldsymbol{X_{jt}}$) that are reasonably associated with labor supply,

$$\begin{aligned} \Delta w_{jt} &= \qquad\quad \alpha_j + \alpha_t + \beta\Delta e_{jt} + C(L)\Delta w_{jt} + \boldsymbol{X}_{jt}'\delta + \epsilon_{jt}, \\ \Delta e_{jt} &= a_j + a_t + b_1 s_{335,j,1990} + b_2 s_{i,j,1990} + c(L)\Delta w_{jt} + \boldsymbol{X}_{jt}'\boldsymbol{d} + u_{jt}, \end{aligned} \tag{13}$$

where $s_{i,j,1990} \equiv e_{i,j,1990}/e_{j,1990}$ is the 1990 employment share for industry i in county j, Δw_{jt} is the annual log change in average weekly wages per employee covering the years 2000 through 2017. The vector $\boldsymbol{X_{jt}}$ is a measure of supply shocks, following Goldsmith-Pinkham et al. (2018). In X_{jt}, initial (1990) fraction of the county population that is male, white, nonadult, college-educated, and native-born to the United States are interacted with time-period fixed effects. These variables act as supply shock controls, correcting for the potential endogeneity bias in models that omit them. With this model, we estimate Hansen's (1982) J statistic for the models with the remaining 89 sectors included in addition to industry NAICS-335.

For the 15 largest and smallest sectors, Table 7 reports the mean and variance in share size as well as the p value for tests of overidentifying restrictions based on the J statistic from estimates of Equation (13). We see sectors with high means have low coefficients of variation and are those commonly thought of as catering to local consumption (e.g., food services, ambulatory care, and retail). On the other hand, sectors with low means have high coefficients of variation, meaning these sectors have employment distributed unevenly across counties. These sectors appear to represent export-oriented industries (e.g., manufacturing, airports, and specialty services). We

TABLE 7 Three-digit NAICS sector statistics

NAICS code	Sector name	Share mean (%)	Share *SD* (%)	Coefficient of variation	Over ID *p* value
Top 15 sector share means					
722	Food Services and Drinking Places	14.12	4.67	0.33	.01
621	Ambulatory Health Care Services	8.25	4.00	0.49	.01
561	Administrative and Support Services	7.46	4.40	0.59	.00
445	Food and Beverage Stores	6.87	3.76	0.55	.05
238	Specialty Trade Contractors	6.58	3.59	0.55	.22
623	Nursing and Residential Care Facilities	6.50	4.66	0.72	.02
452	General Merchandise Stores	6.29	3.24	0.51	.00
541	Professional, Scientific, and Technical Services	6.01	4.36	0.73	.48
522	Credit Intermediation and Related Activities	4.33	2.25	0.52	.78
332	Fabricated Metal Product Manufacturing	4.07	5.27	1.29	.00
624	Social Assistance	4.07	3.54	0.87	.01
441	Motor Vehicle and Parts Dealers	3.77	1.89	0.50	.70
484	Truck Transportation	3.71	3.50	0.94	.58
423	Merchant Wholesalers, Durable Goods	3.60	2.28	0.63	.62
424	Merchant Wholesalers, Nondurable Goods	3.58	3.37	0.94	.23
Bottom 15 sector share means					
312	Beverage and Tobacco Product Manufacturing	0.23	0.87	3.85	.97
518	Data Processing, Hosting, and Related Services	0.21	0.54	2.54	.73
481	Air Transportation	0.18	1.11	6.13	.92
512	Motion Picture and Sound Recording Industries	0.17	0.29	1.72	.62
324	Petroleum and Coal Products Manufacturing	0.09	0.72	8.02	.37
712	Museums, Historical Sites, and Similar Institutions	0.08	0.40	4.78	.01
519	Other Information Services	0.07	0.23	3.33	.62
486	Pipeline Transportation	0.07	0.36	5.41	.01
316	Leather and Allied Product Manufacturing	0.05	0.74	13.40	.77
483	Water Transportation	0.05	0.45	8.63	.77
487	Scenic and Sightseeing Transportation	0.04	0.51	12.80	.05
114	Fishing, Hunting and Trapping	0.03	0.21	8.53	.63
525	Funds, Trusts, and Other Financial Vehicles	0.02	0.11	5.02	.68
533	Lessors of Nonfinancial Intangible Assets	0.01	0.04	4.44	.32
482	Rail Transportation	<0.01	<0.01	18.88	.94

Note: Values presented are from 3-digit NAICS sector employment shares by county for 1990. *J* statistic *p* values are from a sequence of tests of overidentifying restrictions for individual sector shares versus a base sector (see text).

conjecture that sectors with large shares and/or low variation are more likely endogenous with respect to local economic activity, and therefore more at risk of including endogenous information in shift-share instruments.

In the second step of this round, we plot *J* statistics from Equation (13) against average sector employment shares. Recall that a *J* statistic exceeding a critical value indicates rejection of the null hypothesis of no endogeneity. In Figure 2 we see the county average sector employment share has a significant, positive association with potential instrument endogeneity using the *J* statistic as a proxy. Panel (a) presents statistics from models estimated without supply shock controls and panel (b) presents statistics from models estimated without supply shocks. The overidentification testing performed in this round shows that sectors with large shares are more likely to reject exogeneity than sectors with small shares, and that share size is correlated with coefficient of variation of shares across areas. Accordingly, we find that shares with the most variation across areas—which is precisely the variation location quotients measure—are also those least likely to fail endogeneity tests. While controlling for supply shocks helps, as can be seen in the decline of the slope between panels (a) and (b), it does not eliminate endogeneity. We view these results as providing empirical support, in addition to the relevance results previously presented, for constructing instruments using export employment shares in terms of exogeneity.

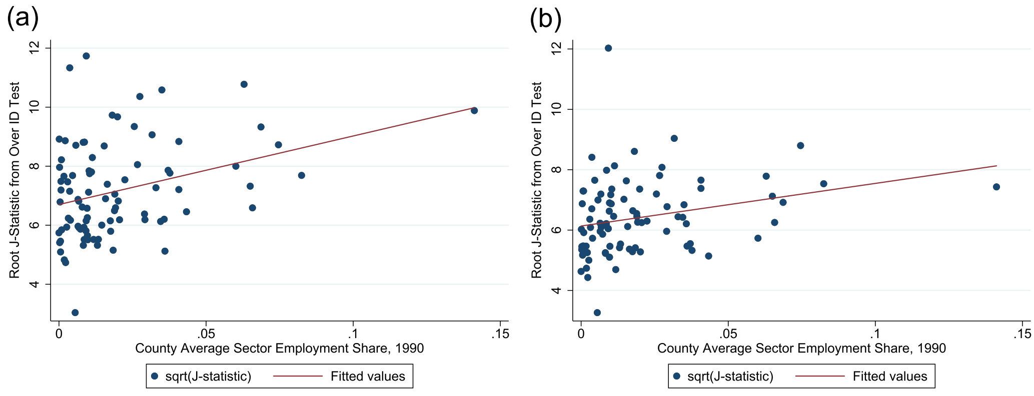

FIGURE 2 Failed exogeneity tests and sector employment shares (a) models with no supply shocks, (b) models with supply shocks. The figures represent scatterplots of two calculated series for 89 3-digit NAICS sectors, measured in 1990 using two different modeling frameworks. The vertical axis presents J statistics from a sequence of tests of overidentifying restrictions for individual sector shares versus a base sector (models in panel (a) do not include supply shocks; models in panel (b) include supply shocks; see text). Industry-specific J statistics, J_i, that are beyond a critical value indicate rejection of the null of no endogeneity. The horizontal axis presents the county-average sector employment share. Estimated equation for panel (a): $\sqrt{J_i} = 6.70(0.21) + 23.22(5.37)\bar{e}_i$, with robust standard errors in parentheses. The critical $\sqrt{J}$ statistic is 6.88 at the 95% level. Estimated equation for panel (b): $\sqrt{J_i} = 6.13(0.17) + 14.12(4.37)\bar{e}_i$ [Color figure can be viewed at wileyonlinelibrary.com]

5.4 | Second-stage estimates

The tests in the first two rounds of the tournament compare the relative power of the standard Bartik measure versus alternative shift-share constructions, with round three showing which employment sectors are more likely to fail tests of endogeneity. However, a more economically relevant question might be, how do the estimated effects of demand shocks on locally endogenous variables vary using different instruments? For the fourth empirical analysis of the tournament, we estimate 2SLS models of employment shock effects using alternative shift-share measures as instruments. An additional purpose of these estimates is to further analyze the class of regression-based industry-mix measures that address the issue of restricted multipliers described in Section 3.2.

Our estimating system is a two-way fixed effects model with log employment change as the treatment variable similar to Equation (13). However, the outcome variable, Δy_{jt}, is now the log change in the four remaining locally endogenous variables in our study: wages, housing stock, population, or house prices, alternately.

$$\begin{aligned} \Delta y_{jt} &= \alpha_j + \alpha_t + \beta \Delta e_{jt} + C(L)\Delta y_{jt} + \mathbf{X}'_{jt}\boldsymbol{\delta} + \epsilon_{jt}, \\ \Delta e_{jt} &= a_j + a_t + bZ_{ijt-1} + c(L)\Delta y_{jt} + \mathbf{X}'_{jt}\mathbf{d} + u_{jt}. \end{aligned} \tag{14}$$

Results from estimating Equation (14) are displayed in Table 8. For the second-stage equation, we report estimates of β and standard errors associated with these estimates, and standard errors for the regressions. The first row in each of the panels reports baseline OLS estimates that assume changes in employment are uncorrelated with the error term. Because this assumption is likely violated, the reported estimates of β are presumably biased. The remaining rows report results using the 18 shift-share measures developed in the paper as instruments for employment growth.

The wage equations replicate the classic use of Bartik-type designs in estimating the inverse elasticity of labor supply. We will ignore wage instruments in this application and compare employment, price, and regression measures. The point estimates of short-run labor supply elasticity vary considerably across these three types of instruments: approximately 0.3 for regression measures, 0.6 for employment, and 1.0 for prices. In our first-stage simulations above, we find that export-employment measures have more statistical power and are less likely to fail endogeneity tests than local service and all-employment instruments. In these follow-on second stage estimates, we see that weaker and less plausibly exogenous instruments tend to yield smaller second-stage coefficients.

In contrast, the point estimates on employment change in predicting growth of the other three variables (housing stock, population, and house prices) fall in a more narrow range. For example, the short-run estimated elasticity of house price with respect to employment shocks is approximately 0.3 across the groups of employment, price, and

TABLE 8 Estimated effects of employment shocks

$\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \beta\Delta e_{jt} + X_{jt}'\delta + u_{jt}$

Dependent variable	Wages			Housing stock		
National shift variable	β	SE	σ	β	SE	σ
OLS	.109***	(0.027)	0.033	.017***	(0.003)	0.004
2SLS: $\Delta e_{jt} = a_j + a_t + c(L)\Delta y_{jt} + bZ_{jt} + X'_{jt}d + v_{jt}$						
Employment (Emp, Upd)	.573***	(0.041)	0.037	.023***	(0.006)	0.004
Employment (Exp-LQ, Upd)	.618***	(0.032)	0.037	.023***	(0.005)	0.004
Employment (Exp-Asm, Upd)	.660***	(0.033)	0.038	.023***	(0.005)	0.004
Employment (Exp-LQ, Init)	.578***	(0.032)	0.037	.015***	(0.005)	0.004
Employment (Exp-Asm, Init)	.634***	(0.037)	0.038	.009	(0.006)	0.004
Employment (Loc-LQ, Upd)	.422***	(0.094)	0.035	.043***	(0.01)	0.004
Prices (Exp-LQ, Upd)	.849***	(0.141)	0.041	.035***	(0.013)	0.004
Prices (Exp-Asm, Upd)	1.075***	(0.162)	0.046	.003	(0.011)	0.004
Prices (Exp-LQ, Init)	.960***	(0.213)	0.044	−.037**	(0.018)	0.005
Prices (Exp-Asm, Init)	1.133***	(0.190)	0.048	−.028**	(0.014)	0.005
Wages (Exp-LQ, Upd)	1.557***	(0.152)	0.059	−.001	(0.01)	0.004
Wages (Exp-Asm, Upd)	1.703***	(0.237)	0.063	−.038***	(0.013)	0.005
Wages (Exp-LQ, Init)	1.390***	(0.134)	0.054	−.012	(0.009)	0.004
Wages (Exp-Asm, Init)	1.470***	(0.19)	0.056	−.028**	(0.013)	0.005
Regression (Exp-LQ, Upd)	.327***	(0.072)	0.034	.028***	(0.004)	0.004
Regression (Emp, Upd)	.296***	(0.067)	0.034	.025***	(0.005)	0.004
Regression (Exp-LQ, Init)	.372***	(0.046)	0.034	.029***	(0.004)	0.004
Regression (Emp, Init)	.344***	(0.047)	0.034	.021***	(0.004)	0.004
Observations Obs: 44,604, Counties: 1,652			Obs: 26,432, Counties: 1,652			

Dependent variable	Population			House prices		
National shift variable	β	SE	σ	β	SE	σ
OLS	.036***	(0.002)	0.007	.144***	(0.011)	0.031
2SLS: $\Delta e_{jt} = a_j + a_t + c(L)\Delta y_{jt} + bZ_{jt} + X_{jt}'d + v_{jt}$						
Employment (Emp, Upd)	.061***	(0.007)	0.007	.274***	(0.026)	0.031
Employment (Exp-LQ, Upd)	.066***	(0.006)	0.007	.308***	(0.025)	0.032
Employment (Exp-Asm, Upd)	.056***	(0.008)	0.007	.313***	(0.031)	0.032
Employment (Exp-LQ, Init)	.062***	(0.007)	0.007	.255***	(0.029)	0.031
Employment (Exp-Asm, Init)	.051***	(0.009)	0.007	.222***	(0.037)	0.031
Employment (Loc-LQ, Upd)	.094***	(0.016)	0.007	.445***	(0.073)	0.032
Prices (Exp-LQ, Upd)	.011	(0.021)	0.007	.464***	(0.103)	0.033
Prices (Exp-Asm, Upd)	−.049**	(0.024)	0.007	.414***	(0.116)	0.032
Prices (Exp-LQ, Init)	−.056*	(0.032)	0.007	.286*	(0.147)	0.031
Prices (Exp-Asm, Init)	−.057*	(0.030)	0.007	.199	(0.126)	0.031
Wages (Exp-LQ, Upd)	.001	(0.016)	0.007	.229***	(0.074)	0.031
Wages (Exp-Asm, Upd)	−.090***	(0.026)	0.008	−.133	(0.115)	0.032
Wages (Exp-LQ, Init)	−.020	(0.017)	0.007	.124	(0.080)	0.031
Wages (Exp-Asm, Init)	−.081***	(0.025)	0.008	−.018	(0.106)	0.032

(Continues)

TABLE 8 (Continued)

Dependent variable	Population			House prices		
National shift variable	β	SE	σ	β	SE	σ
Regression (Exp-LQ, Upd)	.067***	(0.005)	0.007	.364***	(0.021)	0.032
Regression (Emp, Upd)	.061***	(0.006)	0.007	.316***	(0.023)	0.032
Regression (Exp-LQ, Init)	.071***	(0.005)	0.007	.318***	(0.021)	0.032
Regression (Emp, Init)	.062***	(0.005)	0.007	.238***	(0.024)	0.031
Observations:	Obs: 41,300, Counties: 1,652			Obs: 39,696, Counties: 1,646		

Note: The table presents three estimated parameters from a sequence of models: the $\hat{\beta}$ relating the employment change (log-difference) to the change in the locally endogenous variable; the standard error of the $\hat{\beta}$, and the standard error of the regression σ. The operators $c(L)$ and $C(L)$ give a sequence of lags and coefficients that are optimized for each locally endogenous variable: Earnings, 0 lags; Housing Stock, 1 lag; Population and House Prices, 2 lags. Controls in X include the fraction male, fraction white, fraction of the population who are children, fraction with a college degree, and fraction who are native-born to the United States, all measured in 1990 and interacted with time-period fixed effects. Samples are identical in each model for each dependent variable. Robust standard errors (clustered by State × Year) in brackets for OLS regression. 2SLS estimates are estimated using the `xtivreg2` command in STATA (Baum, Schaffer, & Stillman, 2007) with robust (not clustered) standard errors in brackets. No instruments in any cell are "weak," defined as an F statistic of the first-stage equation less than the Stock and Yogo (2005) critical value of 16.38 under the null of a weak instrument.
*$p < 0.1$.
**$p < .05$.
***$p < .01$.

regression instruments (wage instruments are not significant). The success of these instruments is noteworthy because finding an instrument for the local price of housing is an acknowledged challenge in the literature.[21]

6 | CONCLUSION

In the quarter-plus century that has passed since the publication of *Who Benefits from State and Local Economic Development Policies?* by Timothy J. Bartik (1991), numerous suggestions for improving Bartik-type research designs have been advanced within the large collection of papers that implement this popular approach. What the literature lacks is a systematic comparison of the various classes of instruments that have been proposed. This paper is our attempt to fill that gap. We first synthesize the relevant literature on identification in Bartik-type designs. Then, we conduct an empirical tournament to explore strengths and weaknesses of various instrument constructions.

With at least five other papers currently in development, increased attention is certainly being paid to the topic of identification in Bartik-type research designs. These works include three recent working papers discussed in Section 2.2 that develop the formal econometric properties of shift-share estimators in IV designs (Adao et al., 2018; Borusyak et al., 2018; Goldsmith-Pinkham et al., 2018). By examining empirical performance in the particular setting of regional economics, however, our work may contribute more to the group of papers that comment on specific applications. These works include the discussion in Christian and Barrett (2017) on food aid and civil conflict in Nunn and Qian (2014), and the discussion in Jaeger, Ruist, and Stuhler (2018) on labor market effects of immigration in Altonji and Card (1991) and Card (2001).

[21]Davidoff (2016) argues that supply-side measures of natural and regulatory barriers to development, such as Saiz (2010) and Gyourko, Saiz, and Summers (2008), respectively, are problematic as they are correlated with demand factors. In addition, topographical features are static and thus only appropriate for cross-sectional usage, whereas regional approaches typically emphasize panel estimation. The same goes for new demand side instruments recently introduced in the literature, historical transportation plans (Duranton & Turner, 2012), and mineral deposits (Glaeser, Kerr, & Kerr, 2015).

The results of our empirical testing suggest three recommendations for modeling exogenous variation in locally endogenous variables. First, we suggest that researchers use export employment shares (not all-employment shares) in constructing shift-share instruments. This class of instruments offers superior first-stage performance and reduced threat of endogeneity. Second, when there exists a large number of cross-section units (i.e., locations) relative to the number of local industry sectors, an industry mix regression instrument may offer superior first-stage performance. Third, multiple versions of shift-share instruments can be calculated with publicly available data. We suggest that researchers implement overidentification approaches, as each instrument we test typically has independent explanatory power, and the power of any particular instrument varies with context.

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APPENDIX A: ROBUSTNESS—SAMPLE AND SPECIFICATION

This appendix examines the sensitivity of our findings regarding relative instrument performance to changes in sample and specification. This effort involves re-estimating Equation (12) using log-difference in employment as the dependent variable.

$$\Delta e_{jt} = \alpha_j + \alpha_t + \boldsymbol{Z}_{jt}'\boldsymbol{\beta} + C(L)\Delta e_{jt} + \epsilon_{jt}, \tag{15}$$

Recall how these model encompassing exercises allow us to determine if any measure in the vector of instrument candidates, $\boldsymbol{Z}_{jt}$ dominates the others, or if they each contain unique and relevant information.

The variations are presented in Table A1, where the "baseline" models are reproduced from Table 6, Models (1) and (6). Our first batch of models explores the robustness of the baseline estimates to splitting the sample into two groups: the first consists of counties that are defined by the Census Bureau as center-city counties within metro areas, and the second comprising all other counties (excluding those with populations less than 20,000 that are omitted from the initial sample). This criteria divides the observations into two sizeable subsamples, one with little density and a typically suburban or rural character, and the other with higher density. It is potentially useful to consider dense areas separately because different sectors typically occupy center-city versus rural or suburban locations. Density reflects high land prices and different skill mixes, creating a disincentive for land-intensive production and an incentive for human capital-based production. As well, agriculture and resource dominated rural counties with low populations can be highly export-oriented in production. Because there are substantially more nonmetro counties, there is the possibility that economic conditions in these counties are driving our results.

TABLE A1 Model and sample encompassing tests

(A) Dependent variable: 1-year log-difference of employment						
Description	**[1] Base**	**[2] MSA center-city**	**[3] Not MSA center-city**	**[4] County FEs**	**[5] Time FEs**	**[6] No FEs**
$y_{j,t-1}$	0.129***	0.0979*	0.138***	0.118***	0.238***	0.214***
	[0.0190]	[0.0541]	[0.0144]	[0.0169]	[0.0192]	[0.0175]
Employment (Emp, Upd)	0.107	0.309	−0.148	0.390***	0.201**	0.364***
	[0.108]	[0.210]	[0.130]	[0.0706]	[0.0909]	[0.0663]
Employment (Exp-LQ, Upd)	0.444***	0.337***	0.585***	0.248***	0.337***	0.219***
	[0.0476]	[0.0696]	[0.0659]	[0.0500]	[0.0400]	[0.0438]
Prices (Exp-LQ, Upd)	0.0561***	−0.0179	0.0901***	0.101***	0.0393***	0.0885***
	[0.0151]	[0.0218]	[0.0196]	[0.0174]	[0.0148]	[0.0174]
Wages (Exp-LQ, Upd)	0.213***	0.152***	0.307***	0.276***	0.243***	0.284***
	[0.0383]	[0.0402]	[0.0556]	[0.0343]	[0.0354]	[0.0342]
Time period FEs	Yes	Yes	Yes	No	Yes	No
County FEs	Yes	Yes	Yes	Yes	No	No
Observations	42,952	16,536	26,416	42,952	42,952	42,952
R-squared	0.381	0.459	0.347	0.317	0.311	0.244
(B) Dependent variable: 5-year log-difference of employment						
Description	**[7] Base**	**[8] MSA Center-City**	**[9] Not MSA Center-City**	**[10] County FEs**	**[11] Time FEs**	**[12] No FEs**
Employment (Emp, Upd)	0.00497	0.0755	−0.16	0.255***	0.380***	0.468***
	[0.148]	[0.238]	[0.218]	[0.0961]	[0.131]	[0.0996]
Employment (Exp-LQ, Upd)	0.484***	0.434***	0.582***	0.468***	0.233***	0.249***
	[0.0787]	[0.103]	[0.122]	[0.0716]	[0.0673]	[0.0657]
Prices (Exp-LQ, Upd)	0.142***	−0.00757	0.216***	0.0361	0.0715*	0.00077
	[0.0448]	[0.0500]	[0.0582]	[0.0509]	[0.0397]	[0.0423]
Wages (Exp-LQ, Upd)	0.0282	0.155*	−0.132	0.251***	0.149*	0.236***
	[0.0879]	[0.0847]	[0.131]	[0.0541]	[0.0803]	[0.0594]
Time period FEs	Yes	Yes	Yes	No	Yes	No
County FEs	Yes	Yes	Yes	Yes	No	No
Observations	8,260	3,180	5,080	8,260	8,260	8,260
R-squared	0.59	0.676	0.544	0.518	0.294	0.222

Note: Robust standard errors in brackets.
*$p < .1$.
**$p < .05$.
***$p < .01$.

Models (2) and (8) in Table A1 show estimates from the center-city samples, and Models (3) and (9) show estimates from the rural/suburban samples. There are two noteworthy departures from the baseline parameter estimates. First, the export price index performs poorly in center-cities where it has no ability to predict employment changes conditional on other potential instruments while estimates for export prices are maintained and strengthened in the rural/suburban sample. We conjecture this is due to the low variation in prices in service sectors or to leakages in our county level of aggregation that a metro-level measure might capture. Second, while relevance for the export price index goes away in center-cities, wage index estimates strengthen. Wages in cities are evidently more strongly associated with relevant national sector changes than is the association with rural or suburban areas, with wage instrument changes reflecting plausibly exogenous employment demand changes.

The next three models in each panel concern the use of fixed effects in our model specifications.[22] In all models using the 1-year panel, we find all instruments are potentially valid, with the standard Bartik instrument picking up some additional explanatory power. Similar results are found for the 5-year panel, except the export price explanatory power falls nearly to zero.

Across these robustness tests, it is clear that sample choice and the decision whether to include certain groups of fixed effects can affect instrument relevance. The employment index, calculated using location quotients with updated shares, is the only demand indicator that is robust to sample or model considered. Other measures are less robust.

APPENDIX B: ROBUSTNESS—GEOGRAPHY

It is possible that spatial leakages or measurement granularity issues attenuate county-based estimates. To determine if the choice of spatial aggregation to the county level affects the estimates from our simulated first-stage or second-stage equations, we estimate models replicating Tables 3 and 8 using data aggregated to the CBSA level. Table B1 reports the results from univariate models relating a particular instrument to a locally endogenous variable, and Table B2 shows 2SLS estimates of the effect of employment on four other locally endogenous variables, using each instrument we consider in sequence.

Comparing univariate model parameters estimated using CBSA-level data to the same models we estimate using county-level data, we see that virtually all multipliers are approximately the same. Any potentially significant differences are seemingly by chance, as there are no apparent patterns of differences across classes of shares or shifters.

In contrast, the 2SLS models do show some differences, though it is also difficult to establish clear patterns. For instance, while price instruments appear to give larger parameter estimates in the CBSA sample than the county sample, wage instruments give smaller parameter estimates. For models with house prices as the left-hand side endogenous variable, the CBSA data produces estimates that are almost uniformly larger.

In conclusion, while we may have expected some attenuation when aggregating at the county level rather than the CBSA level, the data do not provide any sort of broad evidence outside of house prices. We leave further analysis to future research.

[22] It is common in the literature to use both two-way fixed effects and lagged-dependent variables (e.g., see Blanchard & Katz, 1992, and Saks, 2008). This specification potentially introduces Nickell (1981) bias, so it is important to consider models either without lagged-dependent variables or without fixed effects. We have estimated models both with and without lagged-dependent variables, and effects on instrument parameters are small. Because believe the lagged-dependent variables control for important explanatory power we choose to include them alongside fixed effects, acknowledging the potential for low levels of endogenity bias in our particular models.

TABLE B1 Univariate models, 1-year panel, CBSA aggregation

Estimated equation: $\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \beta z_{jt} + u_{jt}$

Dependent variable	Employment			Wages			Housing stock		
National shift variable	β	SE	R^2	β	SE	R^2	β	SE	R^2
Employment (Emp, Upd)	.625***	(0.042)	.422	.323***	(0.043)	.097	.011	(0.009)	.822
Employment (Exp-LQ, Upd)	.533***	(0.024)	.450	.353***	(0.019)	.111	.015***	(0.005)	.821
Employment (Exp-Asm, Upd)	.405***	(0.021)	.444	.293***	(0.016)	.111	.013***	(0.005)	.821
Employment (Exp-LQ, Init)	.462***	(0.023)	.436	.297***	(0.019)	.104	.007	(0.005)	.82
Employment (Exp-Asm, Init)	.327***	(0.02)	.433	.226***	(0.015)	.104	.004	(0.004)	.82
Employment (Loc-LQ, Upd)	1.224***	(0.108)	.412	.347**	(0.143)	.093	.001	(0.018)	.82
Prices (Exp-LQ, Upd)	.146***	(0.019)	.404	.137***	(0.023)	.094	.005	(0.004)	.822
Prices (Exp-Asm, Upd)	.176***	(0.018)	.406	.154***	(0.026)	.095	−.003	(0.004)	.822
Prices (Exp-LQ, Init)	.092***	(0.016)	.403	.118***	(0.023)	.094	−.002	(0.003)	.822
Prices (Exp-Asm, Init)	.144***	(0.015)	.406	.151***	(0.019)	.096	−.008***	(0.003)	.822
Wages (Exp-LQ, Upd)	.396***	(0.041)	.412	.568***	(0.048)	.103	−.010	(0.008)	.82
Wages (Exp-Asm, Upd)	.274***	(0.038)	.409	.417***	(0.047)	.099	−.014*	(0.008)	.82
Wages (Exp-LQ, Init)	.358***	(0.036)	.411	.475***	(0.038)	.100	−.018***	(0.007)	.82
Wages (Exp-Asm, Init)	.244***	(0.033)	.409	.354***	(0.032)	.098	−.009	(0.007)	.82
Regression (Exp-LQ, Upd)	.522***	(0.019)	.484	.186***	(0.016)	.102	.021***	(0.003)	.822
Regression (Emp, Upd)	.444***	(0.019)	.479	.146***	(0.015)	.100	.015***	(0.003)	.822
Regression (Exp-LQ, Init)	.515***	(0.017)	.488	.181***	(0.016)	.102	.019***	(0.003)	.822
Regression (Emp, Init)	.478***	(0.018)	.49	.149***	(0.016)	.1	.015***	(0.003)	.822
Observations:	Obs: 21,162, CBSAs: 814			Obs: 21,976, CBSAs: 814			Obs: 13,022, CBSAs: 814		

Dependent variable	Population			House prices		
National shift variable	β	SE	R^2	β	SE	R^2
Employment (Emp, Upd)	.054***	(0.011)	.690	.262***	(0.04)	.354
Employment (Exp-LQ, Upd)	.045***	(0.007)	.692	.217***	(0.028)	.349
Employment (Exp-Asm, Upd)	.031***	(0.006)	.691	.174***	(0.026)	.349
Employment (Exp-LQ, Init)	.039***	(0.006)	.691	.18***	(0.033)	.348
Employment (Exp-Asm, Init)	.024***	(0.006)	.690	.102***	(0.028)	.347
Employment (Loc-LQ, Upd)	.079***	(0.024)	.689	.815***	(0.154)	.347
Prices (Exp-LQ, Upd)	.002	(0.004)	.689	.081***	(0.027)	.353
Prices (Exp-Asm, Upd)	−.009**	(0.004)	.689	.071**	(0.028)	.353
Prices (Exp-LQ, Init)	−.005	(0.004)	.689	.028	(0.023)	.352
Prices (Exp-Asm, Init)	−.008**	(0.004)	.689	.018	(0.022)	.352
Wages (Exp-LQ, Upd)	−.007	(0.009)	.689	.253***	(0.053)	.347
Wages (Exp-Asm, Upd)	−.023***	(0.008)	.689	.099*	(0.051)	.347
Wages (Exp-LQ, Init)	−.023***	(0.008)	.689	.198***	(0.052)	.347
Wages (Exp-Asm, Init)	−.024***	(0.007)	.689	.109**	(0.044)	.347
Regression (Exp-LQ, Upd)	.049***	(0.004)	.695	.296***	(0.020)	.355
Regression (Emp, Upd)	.04***	(0.004)	.694	.208***	(0.017)	.352

(Continues)

TABLE B1 (Continued)

Dependent variable National shift variable	Population β	SE	R^2	House prices β	SE	R^2
Regression (Exp-LQ, Init)	.047***	(0.004)	.695	.282***	(0.020)	.354
Regression (Emp, Init)	.041***	(0.004)	.695	.22***	(0.018)	.352
Observations:	Obs: 20,348, CBSAs: 814			Obs: 20,070, CBSAs: 814		

Note: The table presents three estimated parameters from a sequence of models: the $\hat{\beta}$ relating the instrument in the row to the change in the locally endogenous variable in the column; the standard error of the $\hat{\beta}$, and the fit of the model R^2. The operator $C(L)$ gives a sequence of lags and coefficients that are optimized for each locally endogenous variable: Earnings, 0 lags; Employment and Housing Stock, 1 lag; Population and House Prices, 2 lags. Samples are identical in each model for each dependent variable. Robust standard errors in brackets.

*$p < 0.1$.

**$p < 0.05$.

***$p < 0.01$.

TABLE B2 Estimated effects of employment shocks, CBSA aggregation

Estimated equation: $\Delta y_{jt} = \alpha_j + \alpha_t + C(L)\Delta y_{jt} + \beta \Delta e_{jt} + X_{jt}'\delta + u_{jt}$

Dependent variable Estimate	Wages β	SE	σ	Housing stock β	SE	σ
OLS	0.097**	(0.042)	0.034	0.026***	(0.004)	0.004
2SLS: $\Delta e_{jt} = a_j + a_t + c(L)\Delta y_{jt} + bZ_{jt} + X_{jt}'d + v_{jt}$						
Employment (Emp, Upd)	.478***	(0.08)	0.035	0.019*	(0.011)	0.004
Employment (Exp-LQ, Upd)	.607***	(0.034)	0.036	0.024***	(0.008)	0.004
Employment (Exp-Asm, Upd)	.668***	(0.04)	0.036	0.024***	(0.009)	0.004
Employment (Exp-LQ, Init)	.583***	(0.037)	0.035	0.015*	(0.008)	0.004
Employment (Exp-Asm, Init)	.634***	(0.044)	0.036	0.012	(0.011)	0.004
Employment (Loc-LQ, Upd)	.186	(0.149)	0.033	0.027**	(0.012)	0.004
Prices (Exp-LQ, Upd)	1.203***	(0.275)	0.046	−.001	(0.023)	0.004
Prices (Exp-Asm, Upd)	.965***	(0.201)	0.042	−.022	(0.018)	0.004
Prices (Exp-LQ, Init)	2.084***	(0.636)	0.065	−.072*	(0.043)	0.005
Prices (Exp-Asm, Init)	1.225***	(0.197)	0.046	−.064***	(0.022)	0.005
Wages (Exp-LQ, Upd)	1.402***	(0.179)	0.048	−.012	(0.015)	0.004
Wages (Exp-Asm, Upd)	1.814***	(0.355)	0.056	−.041*	(0.022)	0.004
Wages (Exp-LQ, Init)	1.333***	(0.16)	0.047	−.03**	(0.014)	0.004
Wages (Exp-Asm, Init)	1.671***	(0.283)	0.053	−.037	(0.023)	0.004
Regression (Exp-LQ, Upd)	.319***	(0.028)	0.034	.041***	(0.006)	0.004
Regression (Emp, Upd)	.294***	(0.03)	0.033	.037***	(0.008)	0.004
Regression (Exp-LQ, Init)	.317***	(0.03)	0.034	.038***	(0.006)	0.004
Regression (Emp, Init)	.28***	(0.032)	0.033	.034***	(0.007)	0.004
Observations:	Obs: 23,058, CBSAs 814			Obs: 13,664, CBSAs 814		
Dependent variable Estimate	**Population β**	**SE**	**σ**	**House prices β**	**SE**	**σ**
OLS	.055***	(0.003)	0.006	.277***	(0.014)	0.047
2SLS: $\Delta e_{jt} = a_j + a_t + c(L)\Delta y_{jt} + bZ_{jt} + X_{jt}'d + v_{jt}$						
Employment (Emp, Upd)	.082***	(0.013)	0.006	.41***	(0.05)	0.047
Employment (Exp-LQ, Upd)	.081***	(0.01)	0.006	.454***	(0.045)	0.047
Employment (Exp-Asm, Upd)	.07***	(0.011)	0.006	.407***	(0.056)	0.047

(Continues)

TABLE B2 (Continued)

Dependent variable	Population			House prices		
Estimate	β	SE	σ	β	SE	σ
Employment (Exp-LQ, Init)	.078***	(0.01)	0.006	.466***	(0.067)	0.047
Employment (Exp-Asm, Init)	.066***	(0.013)	0.006	.374***	(0.076)	0.047
Employment (Loc-LQ, Upd)	.058***	(0.019)	0.006	.706***	(0.131)	0.048
Prices (Exp-LQ, Upd)	−.016	(0.036)	0.007	.375*	(0.196)	0.047
Prices (Exp-Asm, Upd)	−.073**	(0.033)	0.007	.302*	(0.173)	0.047
Prices (Exp-LQ, Init)	−.103	(0.068)	0.008	.233	(0.334)	0.047
Prices (Exp-Asm, Init)	−.081**	(0.036)	0.007	.071	(0.179)	0.047
Wages (Exp-LQ, Upd)	.003	(0.022)	0.006	.466***	(0.138)	0.047
Wages (Exp-Asm, Upd)	−.083**	(0.037)	0.007	.139	(0.211)	0.047*
Wages (Exp-LQ, Init)	−.047	(0.025)	0.007	.343**	(0.153)	0.047
Wages (Exp-Asm, Init)	−.094**	(0.038)	0.007	.117	(0.197)	0.047
Regression (Exp-LQ, Upd)	.091***	(0.007)	0.006	.558***	(0.037)	0.048
Regression (Emp, Upd)	.087***	(0.008)	0.006	.465***	(0.037)	0.047
Regression (Exp-LQ, Init)	.088***	(0.007)	0.006	.553***	(0.039)	0.048
Regression (Emp, Init)	.082***	(0.007)	0.006	.47***	(0.036)	0.047
Observations:	Obs: 21,350, CBSAs 814			Obs: 20,030, CBSAs 814		

Note: The table presents three estimated parameters from a sequence of models: the $\hat{\beta}$ relating the employment change (log-difference) to the change in the locally endogenous variable; the standard error of the $\hat{\beta}$, and the standard error of the regression σ. The operators $c(L)$ and $C(L)$ give a sequence of lags and coefficients that are optimized for each locally endogenous variable: Earnings, 0 lags; Housing Stock, 1 lag; Population and House Prices, 2 lags. Controls in X include the fraction male, fraction white, fraction of the population who are children, fraction with a college degree, and fraction who are native-born to the United States, all measured in 1990 and interacted with time-period fixed effects. Samples are identical in each model for each dependent variable. Robust standard errors (clustered by State × Year) in brackets for OLS regression. Two-stage least square estimates are estimated using the `xtivreg2` command in STATA (Baum et al., 2007) with robust (not clustered) standard errors in brackets. No instruments in any cell are "weak," defined as an F statistic of the first-stage equation less than the Stock and Yogo (2005) critical value of 16.38 under the null of a weak instrument.

*$p < .1$.

**$p < .05$.

***$p < .01$.