Estimating Who Benefits From Productivity Growth: Local and Distant Effects of City TFP Shocks on Wages, Rents, and Inequality

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Abstract

We estimate direct and indirect effects on US workers' earnings, housing costs, and purchasing power from city-level increases in revenue TFP (total factor productivity). Drawing on four alternative instrumental variables, we consistently find that when a city receives greater revenue TFP growth in the manufacturing sector, there are substantial local increases in employment and average earnings. For renters, these direct effects on earnings are largely offset by increased cost of living; for homeowners, the benefits are more substantial. Local revenue TFP growth leads to greater inflow of more-skilled workers, reflecting lower geographic mobility of less-skilled workers, which mutes the wage gains for more-skilled workers and *reduces* local inequality. We also estimate substantial indirect effects on other cities, due to the substantial migration responses of US workers to local revenue TFP growth: 38% of the combined increase in workers' purchasing power arises from indirect effects on other cities. These indirect benefits accrue especially to renters, and to more-skilled workers, such that neglecting these indirect effects would both understate the overall magnitude of benefits from revenue TFP growth and misstate the distributional consequences. Overall, the average US worker benefited substantially from revenue TFP growth in manufacturing from 1980 to 1990: summing direct and indirect effects, we find that purchasing power increased by 0.5-0.6% per year from 1980 to 2000. The direct effects and indirect effects vary substantially across US cities, however, such that these benefits depend substantially on where workers live.

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I Introduction

Economists have long considered differences in productivity across countries to be a key source of differences in standard of living. Indeed, increases in consumption and welfare depend centrally on productivity growth in many macroeconomic models (see, e.g., Solow, 1956). Within even the wealthiest countries, however, productivity varies tremendously across regions and cities. In the United States, for example, total factor productivity (TFP) in metropolitan areas at the top of the TFP distribution is three times larger than TFP in metropolitan areas at the bottom of the TFP distribution (Moretti, 2011). Despite the importance that many economic models attribute to productivity in driving workers' standard of living, there is little direct empirical evidence on how workers' standard of living is impacted by within-country productivity differences.[1]

In this paper, we examine geographical differences in total factor productivity growth across US metropolitan areas, and estimate who benefits from this productivity growth. The ultimate incidence of local productivity shocks is not clear ex-ante. On the one hand, a positive TFP shock to a city likely means more economic growth, higher labor demand, and therefore higher earnings for local workers. On the other hand, stronger economic growth in a city may induce substantial in-migration from other cities. This in-migration may limit wage gains for local workers. In addition, in-migration may also result in increased housing costs that mitigate economic gains for renters. This raises questions about how much productivity growth actually translates into higher *real* earnings and, therefore, increased workers' standard of living. If housing costs increase substantially in cities hit by TFP shocks, then incumbent landowners may capture most of the benefits and workers who rent their homes may see limited gains in real earnings. Because housing and other non-tradable goods account for the majority of worker consumption, changes in their costs have potential large consequences for workers' standard of living.[2]

Empirically, we measure city-level changes in revenue TFP using data from the United States' Census of Manufactures. We uncover striking geographical differences in revenue TFP

[1] At the national level, the link between productivity and real earnings may also be changing. In country-level data, the correlation between wage growth and productivity growth appears to have weakened (see, e.g., Jones and Klenow, 2016; Schwellnus, Kappeler and Pionnier, 2017; Stansbury and Summers, 2017). See Gennaioli et al. (2014) for an analysis of regional and national economic growth.

[2] This division of economic gains between labor and land is potentially more consequential for understanding the effects on standard of living than the split between labor and capital that has been the main focus of recent research. This issue revives the classical concern of Ricardo, in which land is in fixed supply and so landowners capture all gains from productivity growth. This issue has received substantially less attention than the parallel classical concern of Marx, in which capital owners might increasingly capture a greater share of output relative to labor. For recent work on the share of output that accrues to labor, see, for example, Blanchard, Nordhaus and Phelps (1997); Blanchard and Giavazzi (2003); Bentolila and Saint-Paul (2003); Jones (2005); Rodriguez and Jayadev (2010); Elsby, Hobijn and Şahin (2013); Pessoa, Van Reenen et al. (2013); Piketty (2014); Karabarbounis and Neiman (2014); Autor et al. (2017). See Rognlie (2015) for related analysis, which highlights the role of land scarcity. Suarez Serrato and Zidar (2016) analyze the incidence of local tax changes on labor, land, and capital.

growth across US cities. We use this cross-city heterogeneity in revenue TFP growth, from 1980 to 1990, to estimate its effects on city employment, earnings, and housing costs using US Census data from 1980 to 2010.

We focus on manufacturing revenue TFP growth for several reasons. First, in the period that we focus on, the manufacturing sector was at its peak and employed 20 million Americans (Charles, Hurst and Notowidigdo, 2018). Providing a third of private sector employment, and 41% of labor income, manufacturing was by far the most important 1-digit industry in the US economy. Second, manufacturing accounted for the vast majority of employment in the tradable sector, i.e., the sector that exports goods or services outside the city. If workers can move across sectors, productivity shocks in manufacturing can be expected to have effects not just in manufacturing, but also on other parts of the local labor market. Third, the manufacturing sector experienced large gains in revenue TFP in our sample period; by contrast, output per worker did not increase as substantially in other sectors like retail, wholesale, utilities, education, and health care. Fourth, there are considerations of empirical feasibility: while there are substantial challenges in measuring revenue TFP in the manufacturing sector, which we discuss below, there are even greater challenges in measuring revenue TFP in service sectors.[3]

An important aspect of our analysis is that we do not limit our focus to *direct* effects of a local revenue TFP shock, i.e., effects of a revenue TFP shock in a city on earnings and housing costs in that same city. We allow for the impact of a local revenue TFP shock to include indirect effects on other cities through worker mobility. Thus, in addition to estimating direct effects of local revenue TFP growth, we also quantify one channel of *indirect* effects on wages and housing costs in other cities. Empirically, these indirect effects will prove to be quantitatively important and will change the conclusions that would emerge from an analysis that focuses only on the direct effects.

In the first part of the paper, we focus on estimating <u>direct effects</u> of revenue TFP shocks on cities directly hit by those shocks. We acknowledge substantial identification challenges associated with directly estimating the impacts of revenue TFP shocks, given that spatial differences in productivity growth across the United States are inherently non-random. Our empirical approach uses four alternative instrumental variables, in isolation or in combination, to isolate different sources of variation in local revenue TFP growth that reflect national influences (rather than local influences, which would create spurious associations between revenue TFP and wages or housing costs). Our baseline instrumental variable reflects industry-specific changes in nationwide revenue TFP, but which have differential effects on cities due to differences in cities'

[3]Growth in revenue TFP reflects a variety of factors, including both technological improvement and increased output prices. The impacts of revenue TFP growth (TFPR) operate through increases in labor demand, similar to increases in quantity TFP growth (TFPQ), and so for our purposes the important distinction between TFPR and TFPQ is not central and the empirical analysis can draw on greater sources of variation in TFPR.

initial industry concentration.[4] We construct an alternative instrument based on technological innovation, as measured by patenting activity within technology classes, which affects cities differently due to initial differences across cities in the presence of each technology class. A third instrument is based on changes in exposure to export markets, since trade exposure affects firm output prices and has been associated with patenting and investment in R&D (Aghion et al., 2017; Autor et al., 2018). We also use a fourth instrument, based on changes in stock prices by industry, to isolate variation in revenue TFP that is unexpected.

The instruments have a similar structure, but the underlying identification assumptions are instrument-specific: given different sets of cities that are more or less exposed to different nationwide shocks, the identification assumptions are that more-exposed cities would otherwise have changed similarly to less-exposed cities.[5] Importantly, in practice, we show that the instruments use different sources of empirical variation and are in large part uncorrelated with each other. Put differently, the cities that are predicted to have larger TFP changes for one of our instruments are often different from the cities predicted to have larger TFP changes for other instruments. The alternative instruments yield similar estimates, however, and over-identification tests fail to reject that the estimates are statistically indistinguishable. We cannot rule out the existence of unobserved shocks to employment and wages that are correlated with predicted changes in TFP, but we see the empirical results as more credible when we see a similar pattern of results across the four instruments that use different empirical variation and are thereby based on different identification assumptions in practice. Further, we can control for shocks associated with exposure to particular industries, such as the Oil and Gas industry in the 1980s, or shocks generally associated with areas' manufacturing share or other broad industry shares.

We find that local revenue TFP growth increases the earnings of local workers. A 1% increase in city-level manufacturing revenue TFP from 1980 to 1990 is associated with an average long-run increase of 1.5% in annual earnings from 1980 to 2000. Local employment increases by 4%, driven largely by in-migration from other cities. As a consequence of in-migration, demand for housing increases. We find that a 1% increase in city-level manufacturing revenue TFP is associated with a 1.5% increase in housing rents and a 2.5% increase in home values.

Who benefits from local revenue TFP growth then depends in large part on residents' position in the housing market. For workers who rent their home, much of the increase in earnings is offset by increases in the local cost of living. We calculate impacts on worker

[4]Note that we calculate "leave-out" versions of this instrument, calculating nationwide changes in revenue TFP omitting establishments in that particular city.

[5]Sufficient identification assumptions are that the baseline city industry shares are exogenous (Goldsmith-Pinkham, Sorkin and Swift, 2019) or that the nationwide shocks are exogenous (Borusyak, Hull and Jaravel, 2019), but the necessary identification assumption concerns this interaction: that more-exposed observations would have otherwise changed similarly to less-exposed cities (as in all empirical analysis).

"purchasing power," which reflects earnings adjusted for cost of living, and find that a 1% increase in local revenue TFP increases renters' purchasing power by 0.6% in the long-run. For workers who bought their home prior to changes in local productivity, the gains are much larger: a 1% increase in local revenue TFP increases their purchasing power by 1.1% to 1.6%, depending on how we account for home equity gains.

Who benefits from local revenue TFP growth also depends substantially on workers' level of education. We estimate greater impacts on both nominal earnings and purchasing power for high school graduates than for college graduates. This finding is important because it means that local revenue TFP growth *compresses* local inequality. Correspondingly, we estimate that increases in nominal earnings and purchasing power are substantially greater for workers at the 10th percentile and 50th percentile of the income distribution than for workers at the 90th percentile of the income distribution.

At the same time, increases in local employment are substantially larger for skilled workers. A 1% increase in local revenue TFP is associated with a 5.8% long-run increase in the number of college graduate workers in a city and a 3.2% increase in the number of high school graduate workers. We interpret the larger earnings gains and smaller employment effects experienced by high school graduates as caused by their lower geographical mobility compared to college graduates. This interpretation is consistent with previous literature that has found less-skilled workers to be less mobile and more sensitive to local shocks (Bound and Holzer, 2000; Wozniak, 2010; Malamud and Wozniak, 2012; Diamond, 2016; Notowidigdo, 2019).[6] Our interpretation of these results is consistent with the general economic intuition about incidence: the gains from revenue TFP growth would generally accrue most to those factors that are supplied most inelastically, and so the incidence of local revenue TFP growth would then fall more on less-skilled labor if it is the less-mobile factor.

In the second part of the paper, we estimate <u>indirect effects</u> of local revenue TFP growth. To see why indirect effects might be important, consider an example in which Houston experiences a positive revenue TFP shock that raises wages and employment in Houston. Some workers may leave Dallas, attracted by higher wages in Houston, which affects the labor market and housing market in Dallas. Out-migration from Dallas will raise wages and lower housing costs in Dallas, everything else constant, given a downward sloping labor demand and upward sloping housing supply in Dallas. We expect workers to continue to move from Dallas to Houston until

[6] Bound and Holzer (2000) estimate that college graduates have a more elastic local labor supply than high school graduates due to higher geographical mobility. Wozniak (2010) estimates greater responsiveness to state labor demand shocks among college-educated workers, and Malamud and Wozniak (2012) estimate that college graduation increases geographic mobility. Notowidigdo (2019) finds that local labor demand shocks increase population of a city more than negative shocks reduce its population, and that this asymmetry is larger for low-skill workers. Notowidigdo attributes this difference in mobility to a lower incidence on low-skill workers from adverse labor demand shocks, rather than inherent differences in mobility costs. Our result is also consistent with estimates in Diamond (2016) of a lower propensity among less-skilled workers to move.

workers' purchasing power has increased sufficiently in Dallas for the marginal worker to be indifferent between Houston and Dallas.

One approach to estimate a full range of indirect effects would be to use a spatial general equilibrium model, quantifying the effects by drawing on the structure of the model. Instead, our paper seeks to quantify the general magnitude of some indirect effects by drawing on estimates from the first part of our paper and imposing fewer theoretical assumptions on the structure of the overall economy.[7] For each city hit by a TFP shock, we use our estimates of the direct employment effects and alternative assumptions on city-to-city migrant flows to estimate employment declines in other cities due to out-migration. We then use data on city-level elasticities of housing supply, along with an assumption on the elasticity of labor demand, to gauge the general magnitude of these indirect effects on housing costs and earnings in other cities. Given the quantitative importance of these indirect effects, which follow from our reduced-form estimates, we highlight the need for reduced-form relative comparisons to further consider these sorts of indirect cross-city effects.

We estimate indirect effects that are economically substantial, and have important implications for understanding who benefits from local revenue TFP growth. Revenue TFP growth in one city has only small indirect effects on earnings in each other city, on average, such that there is little bias in the initial estimation of direct effects. When summing these indirect effects across all other cities, however, the indirect effects are an important contributor to the combined effect on earnings. We estimate that 38% of the combined increase in purchasing power for the average worker occurs *outside* cities directly affected by local revenue TFP growth.

The indirect effects disproportionately benefit college-educated workers, due to their greater geographic mobility, which counterbalances the local decrease in inequality associated with local revenue TFP growth. While in-migration of high-skilled workers dampens direct wage gains for high-skilled workers, greater out-migration generates greater indirect increases in high-skilled wages in other cities.

In the final part of the paper, we sum the direct effects and indirect effects from local revenue TFP shocks. We find that the average US worker benefited substantially, in real terms, from revenue TFP growth in manufacturing. Our estimates indicate that local revenue TFP growth from 1980 to 1990 leads to economically large increases in purchasing power: on the order of 0.5-0.6% per year, between 1980 and 2000, for the average US full-time worker. Notably, these effects do not include general equilibrium impacts through decreases in goods prices, increased capital investment, or other such effects.

The summed direct effects and indirect effects are roughly similar for more-skilled workers and less-skilled workers, in percentage terms. Less-skilled workers benefit more from revenue

[7] See also Green, Morissette and Sand (2018) for the related question of how a localized labor demand shock spreads to nearby areas through commuting.

TFP growth in their city, but more-skilled workers benefit substantially more from revenue TFP growth in other cities. Thus, neglecting indirect effects would both understate the gains from local productivity growth and also yield a biased view of its distributional consequences.

How much workers benefit from productivity growth, including direct effects and indirect effects, also does not depend substantially on their exposure to the housing market. Homeowners benefit substantially more than renters from revenue TFP growth in their city, but renters benefit substantially more than homeowners from revenue TFP growth in other cities. The gains for homeowners in some cities come at the expense of homeowners in other cities, such that the impacts on land are largely a transfer from one location to another.

The gains from revenue TFP growth are very different, however, depending on workers' geographical location. This reflects both substantial geographic variation in revenue TFP growth across cities within the United States and also geographical heterogeneity in the indirect effects of local revenue TFP growth. Empirically, the magnitude of indirect effects in a location are not correlated with the magnitude of direct effects in a location. The benefits of productivity growth are economically very large in cities that benefit from strong direct and indirect effects (e.g., San Jose) and minimal in cities with weak direct and indirect effects (e.g., St. Louis). Thus, on net, the average US worker benefits substantially from productivity growth, but these gains depend in large part on where the worker lives. A high-level view of average country-level changes would mask substantial variation in experiences across areas and people.

From a methodological point of view, our results suggest caution for interpreting empirical results that focus on the local impacts of local shocks. Many studies in labor economics seek to estimate the effects of economic shocks, such as immigration (e.g., Card, 2001) or trade (e.g., Autor, Dorn and Hanson, 2013), by comparing areas that experience large shocks to areas that do not. Our findings indicate that when local shocks generate large migration responses, a substantial portion of the overall effects may be missed when focusing only on the direct effects. Including these indirect effects can yield qualitatively and quantitatively different conclusions than suggested by the direct effects, which are often the focus of research. Our proposed approach can potentially be used in other contexts to gauge the magnitude of indirect effects, in a reduced-form manner, and motivate the use of general equilibrium models to quantify additional indirect effects by drawing more strongly on the structure of a model.

Our estimates complement the large literature on skill-biased technological change and labor-saving technological change, which explores increases in inequality from productivity growth. In contrast to skill-biased technological change that favors more-skilled workers, or labor-saving technological change that potentially reduces labor demand, growth in revenue TFP growth is skill-neutral and raises labor demand. During the 1980s, the United States labor market experienced both skill-biased shocks and skill-neutral shocks, along with labor-saving technological change and a variety of other market changes. We focus on one component

of these overall changes – revenue TFP growth in manufacturing – which decreased local inequality but had more neutral effects on national inequality and substantially raised US workers' purchasing power.

The remainder of this paper is organized as follows. In Sections II and III, we discuss the data and the econometric specifications. We report the estimated direct effects in Section IV. We describe our approach for estimating indirect effects in Section V, and report our estimates in Section VI. Section VII concludes.

II Data

In our empirical analysis of the direct effects of TFP shocks, we relate changes in a city's average total factor productivity (TFP) to changes in that city's labor market outcomes and housing market outcomes. For 193 metropolitan statistical areas (MSAs), we combine data on local labor markets and housing markets from the Census of Population with data on total factor productivity of manufacturing establishments from the Census of Manufacturing.

Employment, Earnings, and Housing Costs. We measure labor market outcomes and housing market outcomes at the MSA level, aggregating individual-level data and household-level data from the 1980, 1990, and 2000 waves of the Census of Population and from five waves of the American Community Survey centered on 2010.[8] The main outcome variables are average annual earnings, average household gross rent (for renters), average household home value (for owners), and city employment. For all outcomes, we analyze city-level averages and separate city-level averages within education group (college, some college, high school or less). We also use a measure of city-level elasticity of housing supply (Saiz, 2010).

We use a sample of adult full-time workers. Following standard practice (see, e.g., Katz and Autor, 1999), we restrict the sample to: men and women between the ages of 19 and 65, who worked at least 40 weeks in the previous year, usually worked at least 35 hours per week, and worked for wages or salary in the private sector. Further, individuals' annual earnings must exceed one-half the minimum wage based on a 40-hour week and 40 weeks worked.[9]

We also estimate how TFP affects worker "purchasing power," defined as the increase in earnings minus the increase in local cost of living. Our measure of cost of living follows the BLS method for measuring the nationwide CPI, but adapted to vary at the city level. Estimated increases in housing costs are the most important component of increased local cost of living, and we discuss in more detail below how we define changes in purchasing power for renters and for homeowners.

Appendix Table 1 reports average characteristics of the 193 sample cities in 1980 and average changes over time.

[8] To obtain a larger sample size from the ACS, we use the five-year sample from 2008-2012 (pooling data from 2008 to 2012).

[9] Top-coded earnings and rents are multiplied by 1.5.

Total Factor Productivity. We measure average city-level total factor productivity (TFP) using confidential plant-level data from the Census of Manufacturers (CMF) in 1977, 1987, and 1997. The CMF contains plant-level data on all manufacturing plants' employment, capital stock, materials, and total value of shipments. In the rest of the paper, we refer to years 1980, 1990, and 2000 with the understanding that TFP data are measured three years prior.

To estimate average city-level TFP, in each decade, we adopt an econometric approach that is similar to that used in our previous work based on the same data from the Census of Manufacturers (Greenstone, Hornbeck and Moretti, 2010). We assume each plant uses a Cobb-Douglas technology and, in each year separately, we regress log output on log input expenditures and city fixed effects (weighting by plant output). The estimated 193 city fixed effects, in each decade, reflect average TFP in that city and decade. In Appendix A, we report details of the estimation procedure and its limitations. Here, we highlight two points.

First, our measure of TFP is a measure of "revenue productivity," as is typical in the literature, and therefore productivity growth has a broad meaning in our context that reflects increased *value* of plant output given plant input expenditures. This reflects not only physical productivity increases (more quantity produced for a given set of inputs), but also relative increases in output prices (for example, due to increased demand for firm output). This measure of revenue productivity is the correct measure, for the purposes of our paper, in capturing local changes in labor demand. Both sources of variation in TFP, from prices or physical productivity, have an equivalent effect on local labor markets and local housing markets because both sources of revenue productivity induce greater firm labor demand.

Second, estimated changes in TFP are likely to contain substantial measurement error. TFP is a residual, measured with error. Plant-level measurement error is lessened by analyzing city-level average TFP, though differencing these data to examine city-level changes then exacerbates measurement error. This problem motivates, in part, our use of instrumental variables discussed below.

For each city, Figure 1 shows average TFP in 1980 (panel A), in 1990 (panel B), and in changes from 1980 to 1990 (panel C). There is substantial variation in TFP growth across cities, within broader geographic regions, that we use in the empirical analysis.[10] Reassuringly, though, there is also persistence in TFP across areas, with higher productivity places in 1980 remaining higher productivity in 1990 (Appendix Figure 1).

A range of factors could contribute to differences in TFP growth across cities. Among the possible factors are: changes in local transportation infrastructure, changes in local economic policies and regulations (e.g., changes in environmental or labor regulations), technological improvements by local manufacturing plants, technological improvements at the broader industry

[10]The changes in TFP at different parts of the distribution are: -2.2% (10th percentile), 0.4% (25th percentile), 4.7% (50th percentile), 10.8% (75th percentile), and 13.7% (90th percentile).

level that benefit local plants in those industries, productivity spillovers from local agglomeration economies, or increases in output prices for local plants.

III Empirical Specifications and Identification of Direct Effects

To estimate the direct effects of TFP shocks, we relate changes in a city's average total factor productivity (TFP) to changes in that city's labor and housing market outcomes — employment, wages, and housing costs. In Appendix B, we present a simple spatial equilibrium model (Rosen-Roback) of the labor market and housing market that is useful for understanding how a shock to TFP in a city might affect employment, wages, and housing costs in that city. The model helps clarify the influences on who benefits from local TFP growth. Local TFP growth increases local labor demand, which results in higher nominal wages and also higher cost of housing. The local gains from TFP growth are then split between workers and landowners: the incidence depends on relative elasticities, and which of the two factors (labor or housing) is supplied more elastically. In addition, the model helps clarify how a local shock to one city might indirectly affect other cities through worker mobility.

In our empirical analysis, we regress the change in outcome Y_c in city c — employment, wages, and housing costs — on the change in the city's average total factor productivity A_c. We consider three variants. We define medium-run effects as those obtained when we estimate impacts on changes in outcomes between 1980 and 1990 from changes in TFP between 1980 and 1990:

$$\ln(Y_{c,1990}) - \ln(Y_{c,1980}) = \pi^M(\ln \hat{A}_{c,1990} - \ln \hat{A}_{c,1980}) + \alpha_r + \varepsilon_c, \tag{1}$$

where $\hat{A}_{ct}$ is our estimate of average TFP in city c in year t and α_r is a vector of region effects.[11]

We define long-run effects as those obtained when we estimate impacts on changes in outcomes between 1980 and 2000 from changes in TFP between 1980 and 1990:

$$\ln(Y_{c,2000}) - \ln(Y_{c,1980}) = \pi^L(\ln \hat{A}_{c,1990} - \ln \hat{A}_{c,1980}) + \alpha_r + \varepsilon_c. \tag{2}$$

We also consider longer-run effects as those obtained when we estimate impacts on changes in outcomes between 1980 and 2010 from changes in TFP between 1980 and 1990:

$$\ln(Y_{c,2010}) - \ln(Y_{c,1980}) = \pi^{LL}(\ln \hat{A}_{c,1990} - \ln \hat{A}_{c,1980}) + \alpha_r + \varepsilon_c. \tag{3}$$

These equations reflect reduced-form relationships between TFP and city-level outcomes. These relationships are motivated by the simple Rosen-Roback model in Appendix B, which suggests what factors influence these reduced-form relationships. In particular, the magnitude of the effect of TFP on wages, employment, and cost of housing depend on the relative elasticities of local labor supply and housing supply. Intuitively, employment increases more in a city

[11] The region fixed effects correspond to the four Census regions (Northeast, Midwest, South, West), and the estimates are similar omitting these fixed effects or including fixed effects for the 9 Census divisions.

when its elasticity of labor supply is higher and its elasticity of housing supply is higher. For a city, a more elastic labor supply means that workers are more mobile in response to differences in wages. A more elastic housing supply means that the city can add more housing units to accommodate in-migration with less increase in housing cost. Nominal wages increase more when the elasticity of labor supply is lower, and housing costs increase more when the elasticity of housing supply is lower. Changes in real wages, or purchasing power, depend on the relative elasticities of labor supply and housing supply: for a given elasticity of housing supply, a lower elasticity of labor supply implies greater increases in real wages; for a given elasticity of labor supply, a lower elasticity of housing supply implies that a larger share of the gains accrue to landowners. Intuitively, inelastically supplied factors should bear more incidence.

In the empirical analysis, we estimate several variants of equations (1), (2), and (3). In some specifications, motivated the model, we explore how the estimated impacts of TFP shocks vary with worker skill, or cities' elasticity of housing supply (Saiz, 2010). In other specifications, we add additional control variables. For example, we can control for shocks associated with cities' exposure to particular industries, such as the Oil and Gas industry in the 1980s, or shocks generally associated with areas' manufacturing share or other broad industry shares.

The long-run and longer-run specifications allow for potentially delayed responses in reaching the new spatial equilibrium. The long-run effects of a TFP shock on employment, wages, and housing costs will differ from the medium-run effects if it takes additional time for workers and firms to relocate and for the construction of new housing units.[12] In addition, in the presence of agglomeration spillovers, the effect of a positive local shock may grow stronger over time due to self-reinforcing dynamics.

Across all specifications, we report robust standard errors adjusted for heteroskedasticity. The regressions are weighted by each city's total manufacturing output in 1980. The empirical estimates are similar, however, from unweighted regressions or weighting by city population in 1980.[13]

One specific issue is that local TFP growth from 1980 to 1990 may be correlated with subsequent local TFP growth from 1990 to 2000. Estimating the long-run impacts of local TFP growth might then control for subsequent local TFP growth, though the estimation should not include this endogenous control variable to the extent that local TFP growth causes subsequent local TFP growth through agglomeration economies or other local impacts. Our preferred

[12] We do not know when the TFP shocks take place, within the 1980 to 1990 period, and the observed response from 1980 to 1990 may not reflect the full impact of a change in TFP. In practice, our measured changes in TFP come from Census of Manufacturers data in 1977 and 1987. One limitation is that some early changes in TFP might already be reflected in the 1980 data, though in practice we expect such attenuation to be small.

[13] Our measure of TFP reflects data grouped at the city level, where the size of that group reflects the value of manufacturing output among sample plants. In this case of grouped data, weighting the data by group size is expected to be efficient and provides an estimate of the average impact from increasing the productivity of a fixed segment of the economy.

estimates focus on the reduced-form long-run impact of local TFP growth from 1980 to 1990, though we also report estimates controlling for subsequent local TFP growth from 1990 to 2000.

A related issue is that local TFP growth in the manufacturing sector may induce local productivity growth in non-manufacturing sectors, such as through agglomeration spillovers of the type documented in Greenstone, Hornbeck and Moretti (2010). In this case, TFP growth in non-manufacturing sectors is endogenous and, as such, it should not be controlled for. Our estimates would then identify the net impact of manufacturing TFP growth, including impacts from any endogenous productivity growth through agglomeration economies. We do not measure TFP growth in non-manufacturing sectors, given data constraints on non-manufacturing activity. In one of our specifications, however, we estimate separate impacts on employment and earnings in the manufacturing sector and non-manufacturing sector and calculate the implied local multiplier effect of increased manufacturing employment on non-manufacturing employment.

Local TFP growth in a city may also reflect economic shocks to the regional economy, which impact local TFP growth and economic outcomes in nearby cities that indirectly affect economic outcomes in that city. Later empirical specifications also control for nearby local TFP growth, though not in our preferred specifications.

Figure 2 shows the correlations in local TFP growth over time and over space. Panel A shows that measured local TFP growth from 1980 to 1990 is negatively correlated with measured local TFP growth from 1990 to 2000, likely due to mean reversion. Panels B, C, and D show that local TFP growth from 1980 to 1990 is weakly positively correlated with local TFP growth in cities within 100 miles and not correlated with local TFP growth in cities within 250 miles and 500 miles.

III.A Instrumental Variables

OLS estimation of equations (1), (2), and (3) is likely to be biased for two categories of reasons. First, changes in city-level TFP might be correlated with changes in unobserved factors, such as productive amenities or consumption amenities, that affect employment, local earnings, or housing costs. These biases could be either positive or negative. For example, an improvement in local transportation infrastructure could both increase local TFP and the desirability of the area for workers, which would cause OLS estimates to understate the impact of TFP on wages and overstate the impact of TFP on rents. On the other hand, tighter air quality regulations may lower TFP, decrease nominal wages, and increase housing costs, causing OLS estimates to overstate the impact of TFP on wages and understate the impact of TFP on rents. Second, TFP is measured with error and our main empirical specifications are in first differences, which is known to exacerbate bias from measurement error.

We instrument for changes in TFP using four alternative instrumental variables, in isolation

or in combination. We use these instrumental variables to isolate changes in local revenue TFP that reflect national influences, rather than local changes in TFP that would create spurious associations between revenue TFP and wages or housing costs. Our baseline instrumental variable reflects industry-specific changes in nationwide revenue TFP, but which have differential effects on cities due to differences in cities' initial industry concentration. We use a "leave-out" approach, which calculates nationwide changes when omitting establishments in that particular city. We construct an alternative instrument based on technological innovation, as measured by patenting activity within technology classes, which affects cities differently due to initial differences across cities in the presence of each technology class. A third instrument is based on changes in exposure to export markets, since trade exposure affects firm output prices and has been associated with patenting and investment in R&D (Aghion et al., 2017; Autor et al., 2018). We also use a fourth instrument, based on changes in stock prices by industry, to isolate variation in revenue TFP that is unexpected.

The underlying identification assumptions – that more-exposed cities would otherwise have changed similarly to less-exposed cities – are instrument-specific, based on which cities are more or less exposed to different nationwide shocks. We show that the instruments use different sources of empirical variation, drawing on demand-side or supply-side influences on city-level TFP growth, and the instruments in practice are in large part uncorrelated with each other. That is, the cities that are predicted to have larger TFP changes by one of our instruments are often different from the cities that are predicted to have larger TFP changes by the other instruments. The alternative instruments yield similar estimates, however, and over-identification tests fail to reject that the estimates are statistically indistinguishable. We acknowledge substantial identification challenges associated with directly estimating the impacts of revenue TFP shocks, but we see the empirical results as more credible when different instruments, which draw on different identifying variation despite their similar structure, generate similar patterns of results.

(i) Baseline Instrument. Our baseline instrumental variable uses nationwide changes in TFP by industry to predict each city's change in TFP depending on each city's initial concentration of industries. Specifically, for each city, the instrument is defined by summing over all 3-digit SIC industries: the city's 1980 fraction of output in an industry ($\alpha_{i,c,1980}$), multiplied by the national change in TFP for that industry from 1980 to 1990 ($\gamma_{i,c,1980-1990}$), such that $IV_c^{\text{baseline}} = \sum_i \alpha_{i,c,1980} \times \gamma_{i,c,1980-1990}$. The national change in TFP for industry i is indexed by city c because, to avoid mechanical correlation between industry-level changes and city-specific shocks, we omit that particular city and estimate "leave-out" national changes in TFP by industry across all other cities. For each city, the predicted change in TFP from 1980 to 1990 then depends on that city's industries in 1980 and changes in TFP from 1980 to 1990 for

those industries in other parts of the country.[14]

The identification assumption is that changes in labor market and housing market outcomes in cities with manufacturing output initially concentrated in industries that experience stronger nationwide TFP gains would otherwise have been similar, on average, to changes in cities with manufacturing output initially concentrated in industries that experience weaker nationwide TFP gains. In practice, we can also control for cities' baseline concentration in particular industries, such as the oil and gas industry, that might create spurious correlation between predicted changes in TFP and changes in city-level outcomes. We can also control for cities' baseline manufacturing share, or share of employment in broad industry categories, to use only finer variation in cities' industry shares.

(ii) Technological Shocks Instrument. A second instrument is based on technology shocks, measured using patenting activity. Cities concentrated in particular technologies may experience greater TFP growth when there is greater patenting activity in those technologies nationwide. The patent data is organized by technology class, and different technology classes experienced different rates of patenting over our period of analysis. For example, from 1980 to 1990, the three technology classes with the greatest patent assignees were "Drug, Bio-Affecting and Body Treating Compositions," "Stock Material or Miscellaneous Articles," and "Measuring and Testing."

To construct the city-level instrumental variable, we maintain a similar approach as our baseline instrument but with different identifying variation and, therefore, a different underlying identification assumption. For each city, the instrument is defined by summing over all technology classes i: the number of patent assignees per manufacturing worker in 1980 ($\alpha^p_{i,c,1980}$, multiplied by the total number of patents filed nationwide between 1980 and 1990 ($\gamma^p_{i,c,1980-1990}$) excluding patents from an assignee located in that city, such that $IV_c^{\text{patent}} = \sum_i \alpha^p_{i,c,1980} \times \gamma^p_{i,c,1980-1990}$. We use patent data by technology class from the NBER Patent Data Project (Hall, Jaffe and Trajtenberg, 2001). We match assignee location names to cities using the geographical correspondence engine of the Missouri Census Data Center.

The identification assumption is that changes in labor market and housing market outcomes in cities with technology initially concentrated in technology classes that subsequently experience stronger nationwide patenting would otherwise have been similar, on average, to changes in cities with technology initially concentrated in technology classes that subsequently experience weaker nationwide patenting.

(iii) Export Shocks Instrument. A third instrument is based on increased industry exposure to export markets, which may increase TFP for two reasons. First, increased net imports has been found to reduce innovation of firms in the United States (Autor et al., 2018; Aghion et al.,

[14] We used the same confidential plant-level data.

2017); conversely, increased net exports may have a positive effect.[15] Second, increased export demand may translate into higher output prices and, therefore, higher revenue productivity in cities initially more concentrated in those industries.[16] Recall that measured revenue TFP reflects the *value* of output, conditional on input expenditures, and not only changes in physical productivity.

We aim to isolate exogenous trade shocks to United States industries by measuring increases in exports from high-income countries (excluding the United States). The instrument is calculated as the product of baseline city industry shares ($\alpha_{i,c,1980}$) times the change in exports by industry from 1980 to 1990 ($\gamma^e_{i,1980-1990}$), such that $IV_c^{\text{export}} = \sum_i \alpha_{i,c,1980} \times \gamma^e_{i,1980-1990}$. The instrument then reflects a city-specific index of export exposure, based on a weighted average of industry-specific growth in exports per worker. Export data are from the UN Comtrade database (United Nations, 2003), which include industry exports from 28 high-income countries (excluding the United States) to 94 countries of all income levels.[17]

The identification assumption is that changes in labor market and housing market outcomes in cities with manufacturing output initially concentrated in industries that internationally experience relatively increased export exposure would otherwise have been similar, on average, to changes in cities with manufacturing output initially concentrated in industries that internationally experience relatively decreased export exposure.

(iv) Stock Price Instrument. A fourth instrument is based on stock market returns. Increased stock market valuations capture a variety of factors, including improvements in production technologies and increased demand for firm output, which are associated with increased revenue productivity of particular industries and increased labor demand. The industry-level gains may benefit most those cities that were initially concentrated in those industries.

The main motivation for this instrument is that changes in stock prices between 1980 and 1990 are arguably unpredictable in 1980, as predictive information available in 1980 should be largely capitalized into stock market valuations in 1980. We use this instrument to isolate variation that is plausibly unexpected. This is in contrast to the baseline instrument and other instruments, which use variation that may be partially predicted at the beginning of the period. Thus, a comparison of estimates based on this instrument with estimates based on the other instruments is informative about how much the estimates based on the other instruments reflect unexpected changes.

[15] Bloom, Draca and Van Reenen (2016) find different results for Europe: that European firms have responded to higher levels of Chinese import competition by increasing patenting, raising their intensity of information technology, and increasing their overall level of TFP.

[16] In practice, the estimated first-stage may be attenuated if increased export demand allows for less-productive firms to stay open and this compositional shift lowers industry average TFP.

[17] We calculate the growth in industry exports per worker, using the total number of workers in that industry across all cities in the United States in 1980. The weights are industry employment shares in each city from the 1980 Census. In the trade data, industry definitions are based on SITC Rev. 1 4-digit industries.

The instrument is calculated using industry-specific stock market returns from 1980 to 1990 ($\gamma^s_{i,1980-1990}$), assigned to cities based on their industry output shares in 1980 ($\alpha_{i,c,1980}$). We calculate an index of stock market returns by industry from 1980 to 1990 using monthly CRSP data, weighting companies by market capitalization and restricting the sample to manufacturing companies. When assigning industry-specific growth rates to a city, we exclude companies headquartered in that city.[18]

The identification assumption is that changes in labor market and housing market outcomes in cities with manufacturing output initially concentrated in industries that experience larger increases in stock market valuation would otherwise have been similar, on average, to changes in cities with manufacturing output initially concentrated in industries that experience smaller increases in stock market valuation.

For each of the four instrumental variables, the instrumented effects of TFP reflect a particular Local Average Treatment Effect (LATE). Observed changes in TFP are a combination of permanent shocks and transitory shocks, such that changes in TFP are expected to be mean-reverting to some degree. By contrast, our instrumental variables isolate variation in TFP that is more likely to be permanent due to sectoral shifts in TFP that are largely long-lasting. Permanent changes in TFP are expected to have larger impacts than temporary changes in TFP, and so the IV estimates may more closely represent the impacts of a permanent change in TFP and be larger than the OLS estimates even in the absence of omitted variable bias or measurement error. Various sources of bias in the OLS estimates could go in either direction, however, making it difficult to interpret the relationship between the IV estimates and OLS estimates.

III.B Independent Variation Across Instrumental Variables

Empirically, the four instrumental variables are capturing different sources of variation. There is a substantial amount of independent variation in the four instrumental variables, based on different mixtures of industries experiencing differential growth through the 1980s.

Table 1 shows the sample cities with the largest and smallest predicted changes in TFP for each of the four instrumental variables. While there is some overlap in these lists, the cities predicted to experience the greatest TFP growth between 1980 and 1990 based on the baseline instrument are not the same set of cities predicted to experience the greatest TFP growth based on the patent instrument, export instrument, or stock market instrument. For example, the top three cities in predicted TFP growth are all different across the baseline instrument (Richmond, Atlantic City, Raleigh-Durham), patent instrument (Stamford, Washington, Wilmington), export instrument (Lexington, Fort Collins, Binghamton), and stock market instrument (Greenville, Charlotte, Greensboro). There is more overlap among cities predicted

[18] We assign company locations by matching cities to the zipcodes of Compustat addresses.

to experience the least TFP growth between 1980 and 1990, particularly among cities with substantial exposure to the oil and gas industry that experienced negative shocks in the 1980s, and so below we also report estimates when controlling for cities' baseline share in the oil and gas industry.

Figure 3 shows pairwise correlations for all pairs of the four instruments. Each dot represents a city. The different instruments are statistically correlated in three of the six cases, but reflect a great amount of independent variation, with pairwise regressions yielding R-squared values of 0.002, 0.358, 0.123, 0.014, 0.006, and 0.006.

Figure 4 shows four maps, one for each instrument, that illustrate the geographic variation in predicted changes in TFP. Each instrument is divided into deciles, with darker shades reflecting higher values of the instrument (which predict greater increases in TFP). The maps show that the instruments are not simply picking up local shocks common to each instrument, and that there is geographic variation within nearby areas for each instrument.

Overall, the instrumental variable estimates in our paper draw on alternative sources of variation in TFP that are distinct empirically. The identification assumptions are also then distinct, though the instruments are constructed in a similar manner. In the empirical analysis, over-identification tests will fail to reject that the 2SLS estimates are statistically the same. It lends some credibility to our estimates that the instruments yield similar estimates while drawing on different sources of variation.

III.C Contamination of Control Group

There is an additional source of bias due to indirect effects from local productivity growth, but the magnitude of the bias is small in our empirical setting. Our model predicts that productivity growth in "treated" cities would indirectly increase wages and decrease housing costs in other "control" cities, through city-to-city migration responses to local productivity growth. Our empirical specifications compare changes in cities with greater productivity growth, relative to cities with less productivity growth, and so this "contamination" of the control cities would downward-bias estimated impacts on wages and upward-bias estimated impacts on housing rents. This bias would be particularly severe in settings with a limited number of cities. Our empirical setting includes 193 cities, and each city is a small share of the total labor market. The indirect effects are thereby spread across many cities, and there is a negligible indirect impact on the *average* control city.

In summing these small indirect effects on each other city, however, the total indirect effect is important to consider and in Section VI, we will estimate the sum of these indirect effects.

IV Estimated Direct Effects of Local TFP Growth

We first estimate the direct local effects of local TFP growth. In later sections, we turn to estimating the indirect effects on other cities from local TFP growth (Section V) and then the

combined effect of local TFP growth (Section VI).

IV.A Direct Effects on Employment, Earnings, and Housing Costs

Table 2 reports our baseline estimates, which instrument for changes in TFP using the baseline instrumental variable. Each column reflects a different time horizon and each panel reports estimated impacts on a different city-level outcome. The estimated first-stage impact is reported at the bottom of the table, along with the F-statistic of the excluded instrument. Appendix Table 2 reports corresponding OLS estimates.[19]

City employment responds substantially to local TFP growth. A 1% increase in local TFP is estimated to increase city employment by 2.38% in the medium-run (Column 1), by 4.16% in the long-run (Column 2), and by 4.03% in the longer-run (Column 3). These estimates suggest that it takes additional years for worker migration to respond to increased real wages and for housing construction to respond to increased demand for housing, though this adjustment process was complete by 2000.

Panel B reports estimated impacts on annual earnings per worker. A 1% increase in local TFP is associated with a 0.91% increase in earnings in the medium-run (Column 1), a 1.45% increase in earnings in the long-run (Column 2), and a 1.46% increase in earnings in the longer-run (Column 3). These estimated magnitudes are reduced-form effects of TFP growth and, in particular, can be be greater than 1 when worker in-migration and increased economic activity generates agglomeration spillovers (as in Greenstone, Hornbeck and Moretti, 2010).

The estimated impacts on earnings are economically substantial. Given that real TFP increased by 5.3% between 1980 and 1990 in the average city (Appendix Table 1), the IV estimates suggest that TFP growth increased local earnings of full-time workers in the average sample city by 4.8% from 1980 to 1990, by 7.7% from 1980 to 2000, and by 7.7% from 1980 to 2010. In 2017 dollars, the long-run effect amounts to a total earnings increase of $615 million for incumbent full-time workers in the average city.

We expect increases in local housing costs to mitigate some portion of the estimated increases in local nominal earnings, given the estimated increases in employment that would create additional demand for housing. Indeed, Table 2 shows that increases in local TFP are associated with substantially higher housing costs. A 1% increase in local TFP leads to a 0.98% medium-run increase in rental costs, a 1.47% long-run increase in rental costs, and a 1.09% longer-run increase in rental costs (Panel B). The corresponding effects on home values are somewhat larger, 1.74% – 3.05% (Panel C), which suggests some expectation of future

[19]The IV estimates are larger than the corresponding OLS estimates, which is consistent with the instrument reducing attenuation bias from measurement error in TFP and downward bias from omitted variables (as discussed in Section III.A) though the OLS estimates themselves are difficult to interpret. We also report cross-sectional OLS estimates that are generally larger in magnitude than the OLS estimates for changes in TFP, which is also consistent with measurement error in TFP.

increases in rental costs.

We also explore whether the estimated impact on housing costs is larger in cities with a more inelastic housing supply, as a validation exercise of the TFP shocks suggested by the theory (Section 2) and previous empirical research (Glaeser and Gyourko, 2005; Glaeser, Gyourko and Saks, 2006; Gyourko, 2009; Saiz, 2010). We find that the estimated impact on local housing costs is indeed somewhat greater in cities with a more inelastic housing supply (Appendix Table 3). A 1% increase in local TFP leads to a 2.3% long-run increase in rents in cities with below-mean housing elasticity, and to a 1.2% increase in rents in cities with above-mean housing elasticity.[20]

IV.B Direct Effects on Purchasing Power

We have estimated that TFP gains in a city result in higher local earnings (Panel B) and also higher cost of housing (Panel C). An important question then is how increased TFP affects local "purchasing power," defined as increases in earnings net of increases in local cost of living. Our measure of cost of living follows the BLS method for measuring the nationwide CPI, but adapted to vary at the city level.

Renters. For renters, the effect of TFP growth in a city on local "purchasing power" is conceptually straightforward. In cities with TFP growth, renters have to pay increased housing rents, which reduces their purchasing power in proportion to the importance of housing as a share of total expenditures. Renters also pay increased costs of other non-tradable goods, which reduces their purchasing power in proportion to the importance of non-tradable goods as a share of total expenditures.

Thus, we define the effect on renter "purchasing power" as the percent increase in local earnings (panel B) minus the properly-weighted percent increase in local rent (panel C) and the properly-weighted percent increase in local prices of non-housing non-tradable goods. The proper weights correspond to the share of total expenditures that is spent on housing and non-housing non-tradable goods, respectively. We derive this expression in Appendix C, and address the important data limitation that there are not high-quality city-level data on the local price of non-housing non-tradable goods for most cities in our time period. We follow the approach of Moretti (2013) to impute changes in prices of non-tradables based on changes in rents.[21] In practice, this means that we estimate the impact on renters' purchasing power as

[20]The difference in coefficients is not statistically significant, as there is substantial noise in the estimates and measured housing elasticities. The sample size is also smaller for these regressions, as there are only housing elasticity data for 171 cities out of the original 193 cities (Saiz, 2010).

[21]Moretti (2013) infers how the prices of non-housing non-tradable goods increase in a city along with increases in the cost of housing, using a local CPI released by the BLS for a subset of 23 cities (U.S. Bureau of Labor Statistics, 2000). He estimates that a 1% increase in the local rental price of housing is associated with a 0.35% increase in the local prices of other goods from 1980 to 2000 in the 23 cities. He uses this estimate to predict changes in the prices of non-tradable goods, as a function of changes in housing costs, in cities for which the BLS does not report a local CPI. In practice, the housing share of total expenditures is 0.33 (U.S. Bureau of Labor

the estimated impact on log earnings minus 0.56 times the impact on log rent.

Panel E reports that a 1% increase in local TFP increases renters' purchasing power by 0.36% in the medium-run (Column 1) and by 0.62% in the long-run (Column 2). Purchasing power increases for renters because nominal earnings increase by more than the weighted increase in cost of living. Comparing the increase in purchasing power to the increase in nominal earnings, however, renters lose roughly two-thirds of their earnings increases to higher costs for housing and other local goods and services.

Homeowners. For homeowners, estimating changes in "purchasing power" is conceptually more complicated because it depends on how one accounts for the increase in home equity value. A large literature has examined the financial consequences of homeownership, but we are not aware of a widely accepted measure of the effect of housing prices changes on homeowners' purchasing power (see, e.g., Sinai and Souleles, 2005; Campbell and Cocco, 2007; Attanasio et al., 2009; Buiter, 2010; Mian, Rao and Sufi, 2013; Ströbel and Vavra, 2015; Berger et al., 2018). Thus, we provide two bounds for how TFP growth affects homeowners' purchasing power, which we discuss more formally in Appendix C. The two bounds are different in how they treat gains in home equity value.[22]

In one extreme case (Case A), we consider a homeowner whose purchasing power is effectively insulated from increases in local housing rents. This homeowner does not leave the city after the TFP shock and the homeowner passes the house on to heirs, who also do not leave that city. The homeowner does not pay higher out-of-pocket housing costs when local housing prices increase, but the homeowner does pay a higher user cost for living in the home that is equal to the increased annual rental return on the home. The homeowner also faces increased local prices for non-tradable goods, similar to renters. In this case, we measure the impact on homeowners' purchasing power as the estimated increase in earnings minus the properly-weighted increase in the cost of non-tradable goods (as calculated for renters, above).[23]

At the other extreme (Case B), we consider a homeowner who is able to consume the wealth created by increased home value, either by moving to another city or by leaving a bequest to heirs who will be living in another city whose housing prices have not increased. The homeowner still faces increased local prices for other non-housing goods. In this case, we measure the impact on homeowners' purchasing power as: the estimated increase in earnings, plus the properly-weighted increase in rental return on the home, minus the properly-weighted

Statistics, 2000). Thus, we calculate the estimated impact on renters' purchasing power to be the estimated impact on log earnings minus 0.56 times log rent, where $0.56 = 0.33 + 0.35 \times (1 - 0.33)$. Moretti (2013) validates this imputation using non-housing prices from the Accra dataset. See Appendix C for more details.

[22]Note that we are focused on the case of incumbent homeowners, who purchased their home before the TFP shock and the associated increase in housing prices. Thus, we are interested in how an ex-post change in housing prices affects homeowners' purchasing power.

[23]In practice, the change in purchasing power is measured as the estimated impact on log earnings minus 0.23 times the impact on log rent.

increase in the cost of non-tradable goods.[24]

Panel E reports that when homeowners are insulated from rising housing costs (Case A), a 1% increase in local TFP is associated with a 0.68% increase in purchasing power in the medium-run (Column 1) and a 1.11% increase in purchasing power in the long-run (Column 2). These increases in purchasing power are almost twice as large as the increases in purchasing power for renters, who face increased housing costs. The gains to homeowners are substantially larger when homeowners benefit from the increase in housing costs (Case B).

Comparing the changes in purchasing power of owners and renters, we see that local productivity growth benefits local workers in large part through the housing market rather than through the labor market.

IV.C Additional Instrumental Variables

Table 3 reports the estimated impacts of local TFP growth using the patent IV (Columns 2, 6, 10) and export IV (Columns 3, 7, 11), with the baseline IV estimates reported as a basis for comparison (Columns 1, 5, 9). The bottom row reports the estimated first-stage coefficients, along with the F-statistic on the excluded instruments. The estimated impacts fluctuate somewhat across specifications, and the alternative instruments have less power than our baseline instrument, but the pattern of results is generally consistent. Combining the use of all three instruments, Columns 4, 8, and 12 report similar estimates as our baseline IV specifications. We also then report over-identification tests, which fail to reject that the different instruments are yielding statistically different estimates. That is, despite drawing on identifying variation from different industries experiencing different shocks, the three instrumental variable estimates yield consistent estimates of how local TFP growth directly impacts local economies.

The above instrumental variables reflect changes in industry-level TFP that might be anticipated, to some extent. This would bias downward the estimated impact on housing costs if, for example, anticipated industry-level TFP growth is partly capitalized into local home prices by 1980. Our stock market instrument isolates variation in TFP growth that would be largely unanticipated. Comparing estimates from these instrumental variables can therefore be informative about the relative importance of anticipated and unanticipated TFP shocks in our data.

Table 4 reports the estimated impacts of local TFP growth using the stock market IV (Columns 2, 6, and 10). The estimated impacts are generally similar to the baseline IV estimates

[24] In practice, the change in purchasing power is measured as the estimated impact on log earnings plus 0.10 times the impact on log rent (where 0.10 = 0.33 - 0.23). This calculation assumes that homeowners can consume in perpetuity the annual return associated with increased housing rents in their city (i.e., the percent increase in housing rents multiplied by a 0.33 expenditure share on housing). We assume that homeowners can consume the increase in housing rents that would have been faced by renters of their home. Because homeowners' annual housing rents are unobserved, we assume homeowners and renters in the same city spend the same share of consumption on housing.

(Columns 1, 5, and 9), with somewhat greater estimated impacts on rental costs. Combining the stock market IV and the baseline IV (Columns 3, 7, and 11), or combining all four instruments (Columns 4, 8, and 12), the estimates are generally similar to the baseline estimates. Over-identification tests fail to reject that the different instruments are yielding statistically different estimates, which suggests the previous instrumental variable estimates were not skewed by the anticipation of TFP growth.

Given the similarity in the long-run estimates (1980 to 2000) and longer-run estimates (1980 to 2010) in Tables 2, 3, and 4, going forward we will focus on the long-run estimates and report medium-run estimates as a point of comparison.

IV.D Direct Effects, by Worker Skill

Table 5 reports our baseline IV estimates, separately by worker skill group (College, Some College, High School or Less).[25] We estimate larger impacts of local TFP growth on the employment of higher-skill workers, particularly in the long-run (Panel A). A 1% increase in local TFP leads to a 5.82% long-run increase in employment of higher-skill workers, which is statistically greater than the 3.23% increase in employment of lower-skill workers. These estimates are consistent with our expectation, drawing on previous research (Bound and Holzer, 2000; Wozniak, 2010; Malamud and Wozniak, 2012; Diamond, 2016; Notowidigdo, 2019), that higher-skill workers are more geographically mobile in response to local economic shocks.

We estimate larger impacts on earnings of lower-skill workers, by contrast, and increases in worker earnings within each skill group (Panel B). We estimate that a 1% increase in local TFP leads to a 1.12% medium-run increase in earnings of lower-skill workers, as compared to a 0.60% increase in earnings of higher-skill workers. The difference in these effects is statistically significant, and largely persists in the long-run but the difference is no longer statistically significant. Local higher-skill workers appear to benefit less from local TFP growth in percentage terms, as compared to local lower-skill workers (and local higher-skill workers would then suffer less from local TFP declines, as compared to local lower-skill workers).[26]

Panel E reports increases in the purchasing power of lower-skill renters in the medium-run and long-run, and somewhat smaller and statistically insignificant increases in the purchasing power of higher-skill renters. Local high-skill workers receive more substantial increases in purchasing power if they were homeowners prior to the TFP shock, however, and are thereby insulated from increased housing costs or otherwise benefit from increased home values.

[25] Appendix Table 6 reports the corresponding OLS estimates by worker skill group, though the OLS estimates are difficult to interpret for reasons discussed above.

[26] These differential effects on worker earnings are not undone by differential effects on housing costs by worker skill group. In principle, higher-skill and lower-skill workers could live in different parts of each city and exhibit different sensitivity of housing costs to local TFP shocks. There is some indication of lower impacts on rents and home values for higher-skill workers (Panels C and D), but the impact on renters' and homeowners' purchasing power is generally lower for higher-skill workers (Panel E).

These results suggest that productivity growth *reduces* inequality at the local level, both in nominal terms and adjusted for local cost of living. Higher-skill workers appear very responsive to TFP shocks, whereas lower-skill workers are less responsive, and the greater employment responses for higher-skill workers appear to dampen the local economic gains for higher-skill workers. This is consistent with Bound and Holzer (2000) and Notowidigdo (2019). In the context of our model, this would be the case if worker preferences for locations are relatively more important for lower-skill workers (s is higher).[27]

Figure 5 shows the differential responsiveness of workers by skill group, and its relationship with the local college earnings premium. Panel A shows a decreasing relationship between the change in local college earnings premium and predicted growth in local TFP (using our baseline instrument). Panel B shows an increasing relationship between the change in employment share of college workers and predicted growth in local TFP. This figure summarizes the intuition for how local TFP growth decreases local inequality due to spatial mobility: following an increase in labor demand from local TFP growth, a relative increase in the supply of higher-skilled workers contributes to a decline in the skill premium.[28]

Table 6 reports estimated impacts of local TFP growth on the distribution of local earnings, measured as the difference in log earnings at the 90th and 10th percentiles (panel A). We then separate impacts on overall inequality into impacts on inequality within the upper portion of the distribution (panel B) and within the lower portion of the distribution (panel C). Panel A, row 1, reports that increased local TFP is indeed associated with substantial declines in local earnings inequality. The estimated magnitude implies that a 1% increase in local TFP reduces the 90-10 earnings gap by 0.632%, or that earnings at the 10th percentile increase by 0.632% more than earnings at the 90th percentile. This impact on inequality occurs at the upper portion of the distribution (panel B), whereas there is little impact on earnings inequality at the lower portion of the distribution (panel C).[29] These effects are larger in the long-run, with a 1% increase in local TFP reducing the 90-10 earnings gap by 0.998% and reducing the 90-50 earnings gap by 0.930%.

IV.E Implications for Local Labor Supply

One way to interpret the economic magnitude of our estimated effects is to relate the estimated impacts of TFP to cities' elasticity of local labor supply. Local labor supply reflects how many workers are willing to live in a city for a given wage. Consider Appendix Figure 2, in which

[27]Note that this greater "preference" for locations among lower-skill workers could reflect a number of factors, including: greater reliance on local family networks, greater benefits from local safety nets, or fixed costs of moving.

[28]Much of the literature on technological change and wage inequality has focused on the degree of skill bias in technological change, but we emphasize that even skill-neutral changes in local TFP can differentially impact workers with different levels of education if they have different levels of geographic mobility.

[29]Appendix Table 7 reports the corresponding OLS estimates.

Point 1 represents the equilibrium wage (w_1) and equilibrium employment (N_1) in a city before an increase in TFP. An increase in TFP then shifts local labor demand out from $D(TFP_1)$ to $D(TFP_2)$ and Point 2 reflects the new equilibrium. By shifting labor demand, the TFP shock identifies the slope of the function. The inverse elasticity of local labor supply is given by the ratio of the percent increase in earnings over the percent increase in employment. When this ratio is smaller, the supply of labor to this city is more elastic and the supply curve is flatter. This reflects workers being more willing to move from other cities (without requiring much higher wages), as well as the housing stock being more able to adjust upward (without requiring much higher housing prices).

From Table 2, the estimated long-run impact on earnings (1.45), divided by the estimated long-run impact on employment (4.16), implies a long-run inverse elasticity of 0.35. This number reflects a relatively elastic local labor supply, indicating that in the long-run, the US labor force is fairly willing in this period to relocate to cities with better labor markets.[30]

The local labor supply of college graduates is much more elastic than the local labor supply of high school graduates. The estimates by skill group, from Table 5, imply an inverse elasticity of 0.15 for college graduates and 0.38 for high school graduates.

Note that we have been interpreting the estimated increases in employment as additional workers moving into the city, though increased employment could also reflect increased labor supply of existing city residents. Consistent with migration explaining most of the employment effect, we find that TFP increases the level of employment and level of population by similar amounts (Appendix Figure 3).[31] We also estimate similar percentage increases in employment and the number of households.

IV.F Alternative Specifications and Robustness

Serial Correlation and Spatial Correlation. Local TFP growth from 1980 to 1990 is partially correlated with local TFP growth from 1990 to 2000, which affects the interpretation of the long-run estimates as discussed above. Appendix Table 4 reports long-run estimates when controlling for changes in TFP from 1990 to 2000 (Column 1). Column 2 reports estimates when controlling for changes in TFP from 1990 to 2000 and instrumenting for this change in TFP with an analogous instrument for that later period.[32] In a related exercise, Column 3 reports estimates from a long difference specification, regressing outcome changes from 1980 to 2000 on TFP changes from 1980 to 2000, and instrumenting with the predicted change in TFP from 1980 to 2000. The long difference specification may not reflect long-run effects, however,

[30] This elasticity is higher than that estimated by Beaudry, Green and Sand (2014).

[31] Appendix Figure 3 shows that predicted local TFP growth is not systematically associated with the difference between local changes in working-age adult population and local changes in workers in the medium-run or long-run.

[32] The predicted change in TFP from 1990 to 2000 has a less robust first-stage, which precludes reliable further exploration of its independent effects.

as changes in TFP could occur any time between 1980 and 2000 and the estimated magnitudes are more similar to the medium-run estimates in Table 2.

Columns 4, 5, and 6 report similar estimates when controlling for local TFP growth in cities within 500 miles, 250 miles, and 100 miles, instrumenting using predicted local TFP growth in those cities based on their industry shares and industry-level TFP growth. These specifications also effectively control for regional industry concentration, exploiting variation in relative local industry concentration within that particular city.

Additional Controls. Local TFP growth from 1980 to 1990 may be correlated with other economic shocks, from 1980 to 1990 or from 1990 to 2000. For example, predicted growth in local TFP might be associated with the overall size of the manufacturing sector and differential local economic growth from 1980 to 2000. In Appendix Table 5, Columns 1 and 5, we report similar medium-run and long-run estimates after controlling for differential changes in local economic outcomes associated with the city's manufacturing employment share in 1980. In Columns 2 and 6, we control for cities' employment share in 1980 in broad industry categories, with moderately smaller estimated impacts on nominal wages and housing costs and similar estimated impacts on real earnings. These specifications focus the identifying variation on more-detailed variation in baseline manufacturing industry shares, interacted with nationwide changes in industry-level TFP. In Columns 3 and 7, we report similar estimates when controlling for cities' employment share in 1980 in the oil and gas industry, which experienced negative shocks in the 1980s and was associated with some of the larger relative declines in predicted TFP (from Table 1).

Given the estimated increases in local employment following local TFP growth, one natural question is whether changes in worker composition are driving the estimated increases in annual earnings. Columns 4 and 8, of Panel B in Appendix Table 5, report estimated impacts on annual earnings using individual-level data to condition on worker characteristics: age, age-squared, education (high school, some college, college), race, and gender. Panels C and D report similar estimated impacts on housing costs when using individual-level data to condition on physical characteristics of the home: the number of rooms and number of bedrooms (indicator variables for each number), whether the home is part of a multi-unit structure, and the presence of a kitchen or plumbing.[33] The medium-run estimates are similar, and the long-run estimates are somewhat smaller in magnitude. These specifications are not our preferred models, however, because the changes in worker composition are endogenous and conditioning on endogenous responses to local TFP growth would bias the estimates.

Multiplier Effect on the Non-Manufacturing Sector. Increases in manufacturing TFP directly impact the manufacturing sector, but would be expected to also impact the local non-

[33]Panel E then reports impacts on purchasing power for renters and homeowners, defined as above, including both sets of control variables.

manufacturing sector. Wage and employment growth in manufacturing increase the demand for local non-traded goods and services, and therefore employment in non-manufacturing sectors (Moretti, 2010). The extent to which non-manufacturing sectors are impacted is informative about how much policies directed at the manufacturing sector might influence the broader local economy. Indeed, policy efforts to support the growth of local manufacturing, or prevent the collapse of local manufacturing, are often justified by policymakers on these grounds.

Appendix Table 8 reports that employment responds similarly in the manufacturing sector and the non-manufacturing sector. These increases reflect a combination of in-migration and movement between sectors. We compute the implied "multiplier effect" of the manufacturing sector on the local non-manufacturing sector, defined as the number of additional non-manufacturing jobs created for each additional manufacturing job generated by TFP gains. From an increase in manufacturing TFP that creates one manufacturing job, panel B reports an implied increase of 1.62 non-manufacturing jobs. This estimate is consistent with estimates by Moretti (2010) based on a similar time horizon. A longer time horizon yields a larger multiplier, perhaps because it takes time for the effect of shocks in manufacturing to generate additional demand for local services. Over the long-run, there is an implied increase of 2.21 non-manufacturing jobs.[34]

V Estimating Indirect Effects of Local TFP Growth

Estimates from Section IV report how a local TFP shock affects employment, wages, and housing costs in the city where the shock occurs, relative to other cities. These estimated *direct* effects on local outcomes are only part of the overall impact from a local TFP shock, however, as the local TFP shock also has *indirect* effects outside that particular city. Each local TFP shock will have small indirect effects on other cities, on average, but of more importance may be the sum of these indirect effects across all other cities. In quantifying the full impact of local TFP shocks, it will prove important to include both the direct effects and indirect effects.

We propose a methodology for quantifying indirect effects generated through worker mobility. Quantifying these indirect effects relies on some assumptions regarding which cities workers would move between in response to local TFP shocks, and we explore the sensitivity of our estimates to alternative assumptions. We see our approach as providing a simple gauge as to the rough magnitude of these indirect effects, in contrast to other approaches that lay out a full general equilibrium model of the economy and quantify indirect effects by drawing on the structure of the model.[35] Our approach builds on the empirical estimates from Section IV and

[34]Earnings increase similarly in the medium-run across sectors, and somewhat more in the non-manufacturing sector in the long-run, which could reflect changes in the composition of workers or greater agglomeration productivity gains in the non-manufacturing sector.

[35]For recent examples on the general equilibrium effects of local productivity, see Hsieh and Moretti (2019) and Caliendo et al. (2018). See also recent complementary work on the general equilibrium effects of trade shocks (Adao, Arkolakis and Esposito, 2019; Caliendo, Dvorkin and Parro, 2019).

relies only on assumptions for the elasticity of labor demand and patterns of worker mobility.

The simple Rosen-Roback model in Appendix B precisely defines these indirect effects, which occur through cross-city worker flows, and clarifies how the magnitude of these indirect effects depends on the the elasticity of labor supply to a city and and the elasticity of housing supply in a city.

Intuitively, a local TFP shock in a city generates additional indirect impacts on labor markets and housing markets in other cities, as worker migration responses induce indirect effects. To see why, and to motivate our method for quantifying these effects, consider an example in which Houston experiences a positive TFP shock that raises local wages. Some workers may be attracted by higher wages in Houston and leave other cities, such as Dallas. This affects the labor market and housing market in Dallas, as the decline in workers changes equilibrium earnings and rents in Dallas. Given a downward-sloping labor demand and an upward-sloping housing supply in Dallas, out-migration from Dallas would raise wages and lower rents. Migration from Dallas to Houston might then continue until workers' purchasing power in Dallas has increased sufficiently for the marginal worker to be indifferent between the two cities. Dallas also experiences its own TFP shock, as do other cities, but this effect represents the indirect pressure on labor markets and housing markets in Dallas from TFP changes in Houston.

The magnitude of indirect effects depends on the magnitude of worker reallocation, and our estimates from Section IV found substantial direct employment effects in response to local TFP growth (particularly in the long-run). We therefore expect the indirect effects to be substantial. The magnitude of indirect effects would then be particularly large for workers who are particularly mobile, such as higher-skill workers. Thus, we anticipate that local TFP shocks will have different impacts on inequality at the aggregate level, as compared to the direct effects on inequality at the local level. While we estimated smaller increases in local earnings for higher-skill workers due to an inflow of mobile high-skill workers that dissipate increases in local earnings, this greater inflow of high-skill workers would disproportionately increase earnings of higher-skill workers in other locations. A purely local perspective might then be misleading for the overall effects on inequality from local TFP growth.

Our approach focuses only on indirect effects stemming from worker mobility. There may exist other types of indirect effects in general equilibrium, such as on the price of traded goods or the returns to capital, but quantifying these other potential general equilibrium effects is outside the scope of this paper and requires stronger model assumptions.

V.A Method of Quantifying Indirect Effects

For each sample city that experiences a TFP shock, we use our estimated direct effect on employment and data on city-to-city migration links to estimate how that TFP shock alone would induce employment changes in the other sample cities. We then use data on cities'

elasticity of housing supply, along with an assumption about the elasticity of labor demand, to quantify the indirect effects on housing costs and worker earnings in these other sample cities. Specifically, we proceed in three steps.

Step (1). For each of the 193 sample cities c, we use estimates from Section IV to calculate the number of workers drawn to city c from 1980 to 2000 based on its change in TFP from 1980 to 1990. This number is the product of city c's change in TFP from 1980 to 1990, times the estimated long-run impact on employment (Table 2, Panel A, Column 2), times city c's baseline employment in 1980 (Appendix Table 1).

Step (2). Given an increase in workers in city c, we calculate the associated number of workers that would leave each of the other 192 cities o due to TFP growth in city c. Because we do not observe where these workers would move from, in response to increasing TFP in city c only, we use data on observed city-to-city migration rates to characterize the typical cross-city migration links. As a baseline assumption, we assume that workers are drawn to city c from city o in proportion to observed migration flows from 1975 to 1980 in the 1980 Census of Population.[36] For example, if Houston would have added 1,000 new workers between 1980 and 2000 (based on its TFP gains from 1980 to 1990 and the estimated impact of local TFP growth on local employment), and 15% of migrants to Houston were from Dallas (from 1975 to 1980), then we would calculate an employment decline of 150 workers in Dallas.[37]

Our baseline assumption on worker mobility is that mobility flows between US cities, induced by TFP shocks from 1980 to 1990, occur in similar proportion to mobility flows observed in Census data between 1975 and 1980. Note that this assumption is about migrant shares, rather than the levels of migration. We are not assuming that the same number of people move between Dallas and Houston between 1975 and 1980 as they do following TFP shocks from 1980 to 1990; rather, we are assuming that the share of migrants to Houston from Dallas following a TFP shock in Houston is the same as the share of migrants to Houston from Dallas between 1975 and 1980.

To assess the sensitivity of the estimated indirect effects to this baseline assumption, we also consider two alternative assumptions on mobility. Under one alternative assumption, we assume that workers moving to city c are drawn from all other locations in proportion to their size (which holds fixed the relative sizes of other cities). Under another alternative assumption, we use predicted migrant flows based on the log size of origin city o, the log size of city c, the

[36] We assume a closed economy without international migration, in which a fixed number of workers move across cities. We do allow for workers to migrate from non-sample cities, as measured in the migration flow data, such that we estimate indirect effects in the sample cities along with the estimated direct effects in the sample cities.

[37] These numbers reflect how many workers may have moved from Dallas to Houston due to TFP growth in Houston, holding all else equal. This is distinct from how many workers actually moved from Dallas to Houston, given changes in TFP across all cities. The population in Dallas may not actually decline, as TFP growth in Dallas raises labor demand in Dallas and TFP growth in Houston and other cities lowers labor supply in Dallas.

log geographic distance between city o and city c, and the log economic distance between city o and city c.[38] These alternative assumptions result in less concentrated migrant flows between particular cities, compared to the observed migrant flows from 1975 to 1980, but yield similar estimates of total indirect effects.

Step (3). Given the change in employment in each other origin city o, we calculate the resulting change in housing costs and earnings. For housing, we calculate the decline in households in city o based on the decline in workers and the average number of workers per household in city o, and then calculate the resulting change in housing costs based on estimated city-level elasticities of housing supply from Saiz (2010).[39]

Similarly, we calculate the resulting change in earnings based on an assumed elasticity of labor demand. Our baseline calculations calibrate labor demand using standard first order conditions, assuming an average labor share of 0.65 and a flexible capital share of 0.20, which yields a demand elasticity of -0.15 (1 - 0.65 - 0.20). To assess the sensitivity of the estimated indirect effects to the labor demand calibration, we also report estimates allowing for heterogeneity across cities in the elasticity of labor demand due to variation in city industry mix.[40] Empirically, the results are not sensitive to this heterogeneity in city elasticity of labor demand.

For each city c, the above procedure provides an estimate of how a local TFP shock in city c indirectly affects wages and housing costs in each other city o. We then sum these indirect effects across all cities o. We then sum the indirect effects from each city c and compare these to the direct effects on all cities c. Overall, this procedure depends on data, estimated parameters from Table 2, an assumption on mobility flows between cities, and an assumption on the elasticity of labor demand.

V.B Estimated Indirect Effects: Three Examples

We illustrate this approach with the examples of Houston, San Jose, and Cincinnati. We calculate that real TFP growth from 1980 to 1990 in these cities was 2.4%, 16.4%, and 2.0%,

[38] Drawing on a literature estimating "gravity equations" in migration flows, we regress city-to-city migrant flows between 1975 and 1980 on the origin city size, destination city size, geographic distance, and economic distance (defined as the vectorial distance in the cities' industry output shares). We use these predicted migrant flows from each city o, and the total predicted migrant flows to city c, to assign predicted shares of migrants to city c from each city o.

[39] The estimated elasticities of housing supply from Saiz (2010) reflect the responsiveness of local house prices to local demand shocks, whereas our estimated impacts on "purchasing power" use the responsiveness of rental costs to local demand shocks. We estimate that rental costs are less responsive than house prices, as is typical in the literature, and so we scale the estimates from Saiz (2010) by the ratio of our estimated impacts on rental costs and housing prices (Table 2, Column 2, Panels C and D) to obtain an elasticity of rental costs with respect to local demand. The resulting average elasticity is 2.7, weighting by worker population, such that a 1% decrease in workers would decrease rental costs by 0.37%.

[40] We use data on labor shares by 2-digit SIC industry, and calculate industry-specific labor demand functions assuming the elasticity of labor demand is equal to one minus the labor share minus the flexible capital share (0.20). We then calculate city-level labor demand elasticities by weighting each industry based on its initial output share.

respectively.

For Houston, we calculate that this TFP increase alone would be associated with an increase in employment of 86,031 workers in Houston between 1980 and 2000. Panel A of Figure 6 shows our estimates of where these workers would come from, and which other labor markets and housing markets would be more affected indirectly. For example, 4,551 workers come from Dallas (0.5% of its initial employment), 3,218 from Austin (3.1% of its initial employment), and 2,617 from San Antonio (1.5% of its initial employment). These estimated declines in employment reflect the 1975 to 1980 flow of workers from each city to Houston. The map shows that geographic distance has an important influence, with cities further from Houston experiencing a smaller employment decline following increases in Houston TFP. For example, the employment declines in Portland (OR), Boston, and Madison are 33, 374, and 33, respectively. Panels B and C show the implied indirect effect on per-capita earnings and per-capita housing costs in each city, based on the elasticity of labor demand and the elasticity of housing supply in that city.

Figure 7 shows the corresponding impacts for San Jose. We estimate that San Jose would experience an increase in city-level employment of 361,765 due to substantial increases in TFP from 1980 to 1990. Panel A shows that other West Coast cities were most closely linked to San Jose through migration flows, though San Jose would also attract new workers from cities on the East Coast and upper Midwest. Panels B and C show the associated impacts on earnings and housing costs in those other cities, as a consequence of the worker flows. Figure 8 shows the corresponding impacts for Cincinnati.

Table 7 reports the direct effects and indirect effects of TFP growth in Houston (Panel A), San Jose (Panel B), and Cincinnati (Panel C).[41] Column 1 reports the direct effects as a reference: in Houston, TFP growth from 1980 to 1990 caused employment to increase by 86,031 workers in the period 1980-2000, earnings to increase by $1,490 per worker, and housing costs to increase by $501 per worker (in 2017 dollars).[42] These increases amount to annual increases of $75 and $25, respectively, from 1980 to 2000. Column 2 reports that local TFP growth in Houston, all else equal, would have induced employment declines in each of the other 192 cities, on average, by 291 workers from 1980 to 2000. This employment decline is associated with a $9 increase in earnings and a $8 decline in rent, on average, from 1980 to 2000 for workers in other cities (or annual effects of $0.45 and $0.40, respectively). These indirect effects in each of the other cities are small, on average, but these indirect effects will be economically substantial

[41]The standard errors on the indirect effects follow from the variance-covariance structure of the previous estimates.

[42]For comparability to our analysis in Table 2, and our discussion of changes in purchasing power, we assume that workers' baseline housing costs equal 0.33 times their baseline earnings. This assumption results in housing costs being measured on a comparable scale as earnings, given that earnings are greater than expenditures (e.g., due to taxes). For this table, we report numbers for renters.

when summed across all cities.

TFP growth in San Jose generates substantially larger direct effects and indirect effects (Panel B), due in part to greater TFP growth in San Jose than in Houston. San Jose generates larger indirect effects on housing costs relative to earnings, as compared to Houston, because San Jose is drawing more workers from cities with a more inelastic housing supply than the cities losing workers to Houston.

The direct effects and indirect effects from TFP growth in Cincinnati (Panel C) are substantially smaller. These effects are smaller than those for San Jose because San Jose experienced a substantially larger increase in local TFP. The direct effects on earnings and rents are similar to those for Houston, given their similar estimated changes in TFP from 1980 to 1990, but Cincinnati generates smaller indirect effects because it is substantially smaller than Houston.

Columns 3, 4, and 5 report that the estimated indirect effects are not sensitive to alternative assumptions about worker migration flows and allowing the elasticity of labor demand to vary across cities. Columns 3 and 4 report similar indirect effects on earnings and housing costs in the average other city, assuming that workers are drawn from other cities in proportion to those other cities' population (Column 3) or assuming that workers are drawn from other cities based on predicted migrant flows (Column 4). Column 5 reports similar indirect effects on earnings, allowing for the elasticity of labor demand to vary across cities according to their baseline industry shares and industry-level labor shares.[43]

VI Estimated Combined Impacts of Local TFP Growth

VI.A Direct Effects, Indirect Effects, and Combined Effects

Table 8 reports the long-run impact from direct effects (columns 1 – 4), indirect effects (columns 5 – 8), and the combined effect on worker purchasing power (columns 9 – 13). These long-run effects are the effect of TFP growth from 1980 to 1990 on changes in outcomes from 1980 to 2000, in 2017 dollars. We calculate the combined effects by summing the direct effects and indirect effects from local TFP growth in each city, as described in the calculations for Table 7.[44] We show the standard error of the combined effect, which follows from the variance-covariance structure of the estimated direct effects and the estimated correlation across MSAs between the direct effects and indirect effects. We report separate impacts on renters (33.6% of workers) and homeowners (66.4% of workers).

Panel A reports that local TFP growth had substantial long-run direct effects on the average renter's earnings ($3,823), housing costs ($1,286), and costs of other local goods ($900) in the

[43]For Column 5, we assume that workers are drawn from other cities according to the data on migration flows from 1975-1980 (as in Column 2).

[44]We calculate the the absolute effect in each city, sum these effects across each city, and then divide by the total number of workers in all cities.

cities directly hit by TFP shocks.[45] The direct effect on purchasing power for renters ($1,636), defined as the increase in earnings minus increases in housing costs and costs of other local goods, suggests that increased cost of living offsets two-thirds of the increase in earnings (as in Table 2). Summing the indirect effects of local TFP growth in each city, the average renter received a substantial further increase in earnings ($931), decrease in housing costs (-$1,059), and decrease in cost of other local goods (-$741). These indirect effects contributed a net increase of $2,731 in renters' purchasing power (Column 8).

Summing the direct effect and indirect effect, we calculate that renters' purchasing power increased by $4,367 (Column 9), which is an 11.3% increase on 1980 earnings in 2017 dollars (Column 10). Dividing this number by 20, there was a 0.56% annual combined increase in purchasing power for renters from 1980 to 2000 (Column 12). These numbers are similar under alternative assumptions for worker migration flows (Columns 12 and 13).

Indirect effects make up almost two-thirds of the combined increase in purchasing power for renters. As TFP increases in other cities and attracts workers to those cities, renters receive both increases in earnings and decreases in cost of living. For renters, most of the increase in local housing costs from increased local TFP is offset by decreases in local housing costs from increased TFP in other cities.[46]

We have seen in the previous section that there are small indirect effects of TFP growth in one city on each other city. Table 8 shows that, when summing these indirect effects across all other cities, the sum is quite large. Thus, neglecting these indirect effects would provide an incomplete picture of the overall impacts from local TFP growth.

Panel B reports impacts on homeowners. Homeowners receive a larger increase in earnings than renters (Column 1), largely because their baseline average earnings are higher.[47] The geographic distribution of homeowners and renters also matters, as homeowners and renters may be disproportionately in cities that experience different changes in TFP.

Compared to renters, homeowners receive notably larger direct effects on purchasing power, in Case A (defined as Column 1 minus Column 3), as homeowners do not pay higher housing rents. In Case B (defined as Column 1 plus Column 2 minus Column 3), there are even larger direct effects on homeowners' purchasing power because homeowners benefit from local

[45]Following our discussion of impacts on "purchasing power," we assume that the dollar cost of other local goods increases by 0.70 times the dollar increase in housing costs (which reflects a 0.35% increase in the cost of other goods from a 1% increase in housing costs, along with an expenditure share on other goods that is twice the expenditure share on housing (0.33).

[46]Note that the positive effects on housing costs do not need to equal the absolute value of the negative indirect effects, if elasticities of housing supply are not the same across cities. As the elasticity of housing supply varies across cities, it matters which cities are experiencing local TFP growth and which cities are gaining or losing workers.

[47]From Table 2, our empirical specifications assume that local TFP growth has the same percent effect on local earnings of renters and homeowners.

increases in housing rents.[48] However, homeowners benefit less than renters from the indirect effects of TFP growth because of decreasing housing rents due to TFP growth in other cities (Columns 5 – 8). For homeowners, there is a spatial redistribution in housing costs, with gains for homeowners in some cities coming at the expense of homeowners in other cities. Compared to renters, there is less indirect increase in purchasing power for homeowners (Case A) or substantially less indirect increase in purchasing power (Case B). For homeowners, 26% of their combined increase in purchasing power comes from indirect effects, taking the average of Case A and Case B. For the average worker, taking a weighted average over renters and homeowners, 38% of the overall increase in workers' purchasing power occurs *outside* cities directly affected by local TFP growth.[49]

Overall, renters and homeowners receive notably similar percent increases in purchasing power from TFP growth when including both direct effects and indirect effects (Columns 10 and 11).[50] This finding is in sharp contrast to our earlier finding based only on the direct effects, which suggested that purchasing power gains are much larger for homeowners. At the local level, TFP growth disproportionately benefits homeowners compared to renters, because it raises both their earnings and the value of their asset. However, the overall incidence of TFP growth is the same for renters and owners, as impacts on land in one city are roughly counterbalanced by impacts on land in other cities. This suggests that looking only at the local effects of local TFP shocks would yield an incomplete and inaccurate picture of the overall effect of TFP shocks.

VI.B Combined Effects by Skill Level

Table 9 reports the direct effects and indirect effects, separately for higher-skill workers (Panel A) and lower-skill workers (Panel B). Workers with more education are moderately more likely to be homeowners, and in Panel C we report the average impact by skill group (averaging over renters and homeowners).[51]

The direct effects on purchasing power are only moderately higher in levels for higher-skill workers (Table 9, Panel C, Column 4), despite substantially higher baseline earnings among higher-skill workers, because of the larger estimated percent gains for lower-skill workers (Table 5). This is also despite a slightly higher share of homeowners among higher-skill workers, which

[48] For this Case B, as above, we assume that homeowners can consume the increase in housing rents that would have been faced by renters of their home. Homeowners' annual housing rents are unobserved, so we assume homeowners and renters in the same city spend the same share of earnings on annual housing rents.

[49] For calculating this weighted average, the weights reflect the share of workers that are renters (33.6%) and homeowners (66.4%). For homeowners, we take the average of Case A and Case B.

[50] The overall dollar increases are larger for homeowners (Column 9), as they have higher average earnings.

[51] For homeowners, we take the average impact on purchasing power for Case A and Case B. We then calculate the weighted average impact within each skill group, weighting by the fraction of workers in that skill group that are renters or homeowners.

increases the direct effect on purchasing power from local TFP growth.[52] These estimates also reflect the geography of TFP shocks, which matters due to variation across cities in their share of higher-skill workers.

The indirect effects on purchasing power, however, are substantially higher for higher-skill workers (Table 9, Panel C, Column 8).[53] Because of higher geographic mobility among higher-skill workers, there are substantially greater indirect increases in earnings of higher-skill workers in both levels and percentage terms. Indeed, the indirect effect on higher-skill renter earnings is 64% of the direct effect on higher-skill renter earnings (Panel A, Columns 5 and 1). By contrast, the indirect effect on lower-skill renter earnings is 18% of the direct effect on lower-skill renter earnings (Panel B, Columns 5 and 1).

As a consequence, there are similar annual percent increases in purchasing power for higher-skill workers (0.52%) as for lower-skill workers (0.45%) when summing these direct effects and indirect effects (Panel C, Column 11). Indirect effects make up 56% of the overall effect for higher-skill workers, compared to 35% of the overall effect for lower-skill workers (Panel C, Columns 8 and 9). While we estimated local TFP growth to compress local inequality, the presence of indirect effects causes TFP growth to have little effect on inequality by worker skill. There are larger dollar increases for higher-skill workers, however, given higher baseline earnings. TFP growth from 1980 to 1990 increased purchasing power for the average higher-skill worker by $7,338 from 1980 to 2000 (Panel C, Column 9), or $367 per year. For the average lower-skill worker, purchasing power increased by $3,788 or $189 per year.

TFP shocks do have substantial redistributive effects across workers in different locations, however, by skill group and homeownership status. Local TFP shocks benefit local lower-skill workers more than local higher-skill workers, and benefit higher-skill workers in other cities more than lower-skill workers in other cities. More-mobile higher-skill workers benefit wherever local TFP increases, whereas less-mobile lower-skill workers are more sensitive to productivity shocks within their city. Local TFP shocks also benefit local homeowners more than local renters, whereas these shocks benefit renters in other cities more than homeowners in other cities. These effects have important implications for the geographic distribution of gains from productivity growth, as well as who benefits from productivity growth within those areas.

Overall, higher-skill and lower-skill workers receive roughly similar percent increases in purchasing power from TFP growth when including both direct effects and indirect effects. This finding is in sharp contrast to our earlier finding based only on the direct effects, which suggested

[52] Among higher-skill workers, 31.3% are renters and 68.7% are homeowners. Among lower-skill workers, 34.6% are renters and 65.4% are homeowners.

[53] Note that we assume no imperfect substitution between higher-skill and lower-skill workers, as well as no externalities across workers. That is, when calculating indirect effects by skill group, we assume that out-migration of higher-skill workers affects only higher-skill worker earnings and that out-migration of lower-skill workers affects only lower-skill worker earnings.

that the gains were larger for less-skilled workers. At the local level, TFP growth disproportionately benefits the earnings of less-skilled workers due to their lower mobility. Since college graduates are more mobile, their local earnings increase by less. However, the overall incidence of TFP growth is the same. College-graduates receive greater indirect effects, precisely because they are more mobile. This finding further underscores our conclusion above that looking only at the local effects of local TFP shocks would yield an incomplete and inaccurate picture of the overall effect of TFP shocks.

VI.C Combined Effects by Location

We have found that, including both direct effects and indirect effects, the combined effect of TFP shocks is similar for homeowners and renters and similar for higher-skill workers and lower-skill workers. However, the impact of TFP shocks is very different in different parts of the country. Partly, this is because TFP growth is very heterogeneous across locations and so the direct effects vary across cities. In addition, the indirect effects vary substantially across cities because each city is connected differentially to different cities that experience different TFP shocks.

Thus, local TFP shocks have important redistributive effects across space. A positive TFP shock in Houston benefits workers there and landowners there; less obviously, it also benefits workers in Dallas (although less than for workers in Houston), benefits renters in Dallas, and hurts landowners in Dallas. These effects do not necessarily even out over geographic space, as some cities are positioned to receive larger indirect effects independent of the magnitude of their own direct effects.

Figure 9 maps the spatial distribution of direct effects, indirect effects, and combined effects for renters. There are large differences across cities, both in the direct effect and the indirect effect. Renters in some cities receive large direct effects from local TFP growth, and renters in other cities receive small direct effects. Renters in some cities receive large indirect effects from local TFP growth elsewhere, while renters in other cities receive small indirect effects.

Interestingly, there is little inherent correlation between cities that receive large direct effects and cities that receive large indirect effects. This limited correlation is seen in Figure 10, which plots the direct effect in each city against the indirect effects in each city. The implication is that, while indirect effects magnify the direct effects of local TFP growth, the indirect effects of TFP growth elsewhere do not inherently compensate workers for the relative absence of direct effects in their city.

Table 10 divides cities based on the terciles of direct effects and indirect effects on renters and lists example cities that received: large direct effects and large indirect effects (Panel A), large direct effects and small indirect effects (Panel B), small direct effects and large indirect effects (Panel C), and small direct effects and small indirect effects (Panel D). Renters in some

cities disproportionately benefit from large indirect effects, which are largely independent of the direct effect magnitude in that city. Example cities in the top group are Binghamton, Charleston, New Orleans, and San Jose. Example cities in the bottom group are Dallas, St. Louis, Tulsa, and Youngstown. Appendix Table 9 reports these effects for homeowners.

Table 11 summarizes the effects by Census division. The largest beneficiaries, in terms of combined increases in purchasing power, are residents in the South Atlantic division. Due to strong direct effects and indirect effects, renters in this area received annual gains in purchasing power of 0.93% per year. Renters in the Middle Atlantic received similarly large direct effects, but somewhat smaller indirect effects, and overall annual gains of 0.65% per year. Renters in New England received smaller direct effects, but larger indirect effects than in the Middle Atlantic division, and so also received annual gains of 0.65%. The smallest overall gains were in the West North Central and West South Central divisions. At this regional level, there remains substantial variation in the relative contribution of indirect effects, and workers' location matters substantially for the benefits they receive from productivity growth.

VII Conclusion

The goal of this paper is to understand better who benefits when areas experience revenue TFP growth, and increasingly produces greater value output from given inputs. We find that the average US worker benefited substantially from revenue TFP growth in manufacturing. Our estimates indicate that purchasing power gains from local manufacturing TFP growth, from 1980 to 1990, are economically large: on the order of 0.5-0.6% per year, between 1980 and 2000, for the average US full-time worker. Notably, these overall gains do not depend much on a worker's education or homeownership status. Rather, who benefits from this productivity growth mainly depends on where a worker lives.

In particular, we find that when a city experiences productivity gains in manufacturing, local workers enjoy higher earnings but in-migration of workers raises local housing costs. For workers who rent their home, increased earnings are in large part offset by increased cost of living, while the benefits for homeowners are more substantial. Thus, at the local level, productivity growth benefits the average local worker but much of the benefits come through the housing market rather than through the labor market.

Local productivity growth also *reduces* local inequality. Local TFP shocks have more impact on the earnings of local less-skilled workers than the earnings of local more-skilled workers, as less-skilled workers are less mobile on average.

However, local productivity growth also has important indirect effects on other cities, and these effects are large enough to alter the ultimate incidence of productivity growth. We estimate that 38% of the overall increase in purchasing power for the average worker occurs *outside* cities directly affected by local TFP growth. Neglecting these indirect effects, generated

by worker mobility, would substantially understate the gains from local productivity growth.

Importantly, the indirect effects on worker earnings are substantially greater for more-skilled workers, who are more mobile, which increases inequality in other cities. Higher mobility implies that more of the incidence of local TFP shocks occurs outside the city directly hit by the shock. Notably, the net percent impact on purchasing power is then similar across less-skilled and more-skilled workers, with less-skilled workers benefiting more locally and more-skilled workers benefiting more elsewhere. These estimated effects of factor-neutral TFP growth complement the large literature on skill-biased technological change.

In addition, the net impact on purchasing power is similar for renters and homeowners, with homeowners benefiting more locally and renters benefiting more elsewhere. Due to these indirect effects, the impacts on landowners are largely a transfer from one location to another. Thus, the overall incidence of TFP growth falls mainly on workers.

Neglecting indirect effects from worker mobility would lead to incorrect conclusions, not just on the overall magnitude of the effect of TFP shocks but also on their distributional consequences. A similar problem may arise in other studies that analyze local labor market outcomes in a variety of settings, from immigration to trade shocks and infrastructure investment. We suggest an approach to gauge the magnitude of indirect effects from worker mobility, which can be used in other contexts by drawing on reduced-form estimates in a way that imposes fewer assumptions than the full structural estimation of general equilibrium models (but also considers fewer types of general equilibrium effects).

While the average US worker benefits substantially from productivity growth, these gains depend in large part on where the worker lives. Because average nationwide productivity growth reflects a great deal of geographic variation in local productivity growth, a high-level view of average changes would mask substantial variation in benefits across areas and people.

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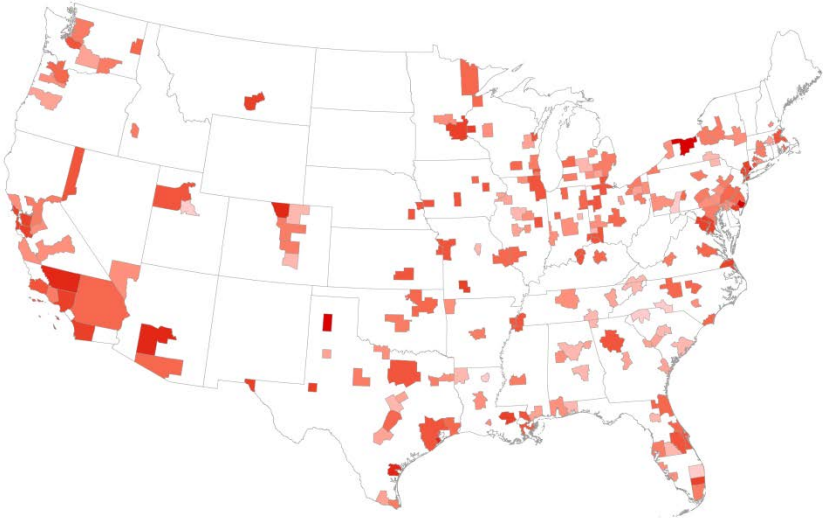
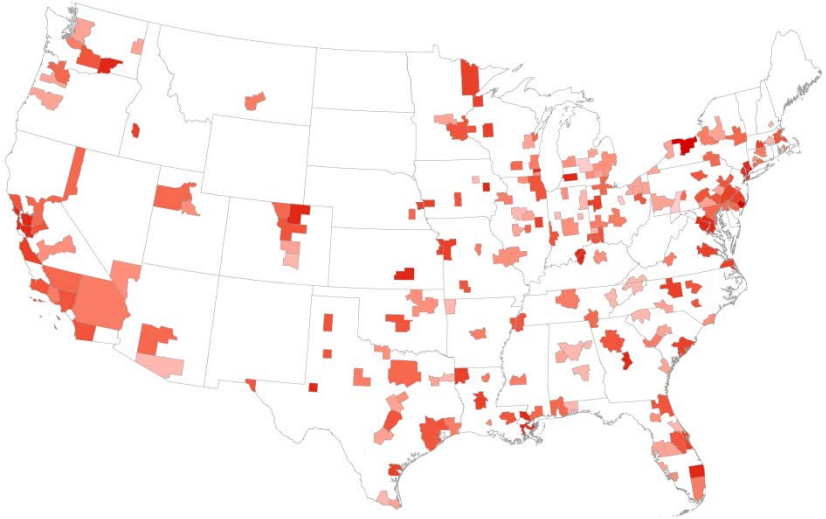
Figure 1. Spatial Distribution of Total Factor Productivity, 1980 and 1990

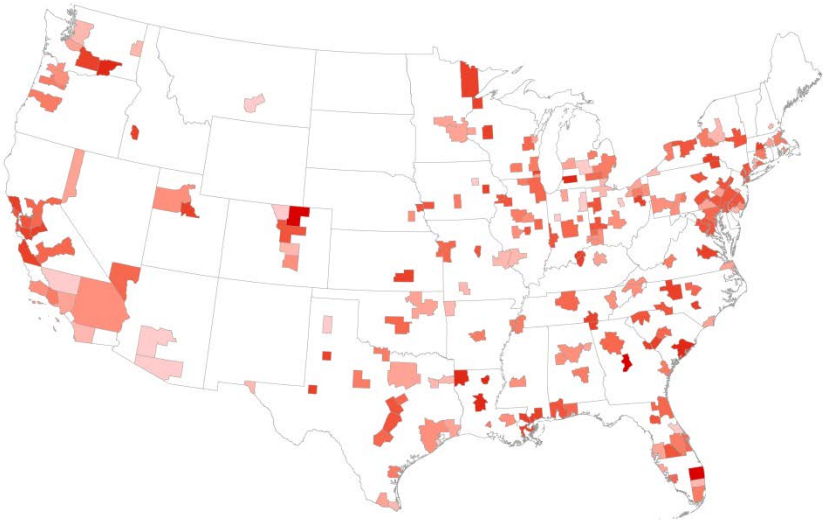
Panel A. TFP in 1980



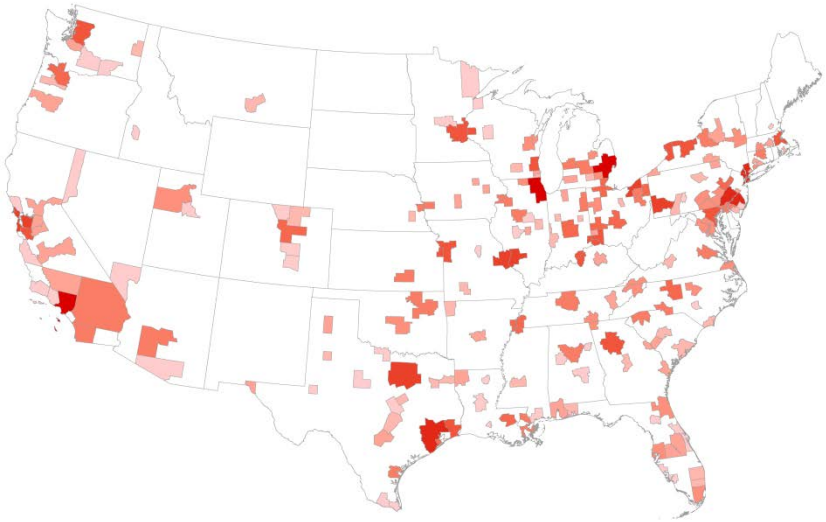
Panel B. TFP in 1990



Panel C. Change in TFP from 1980 to 1990



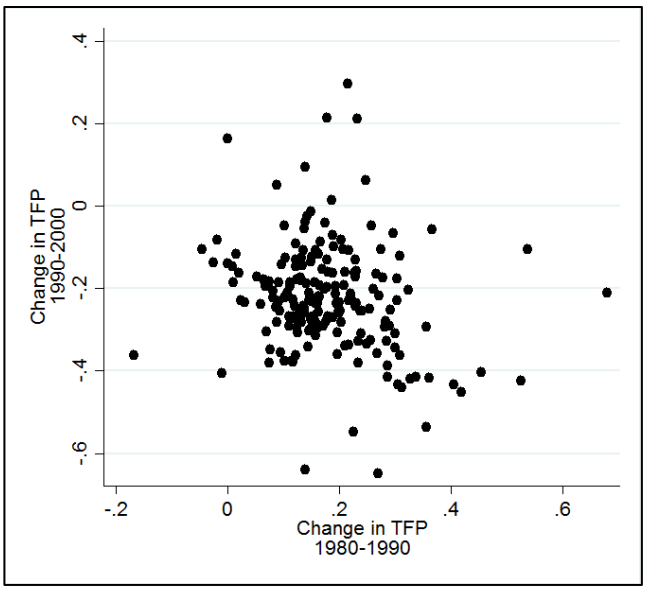
Panel D. Total Manufacturing Output by MSA



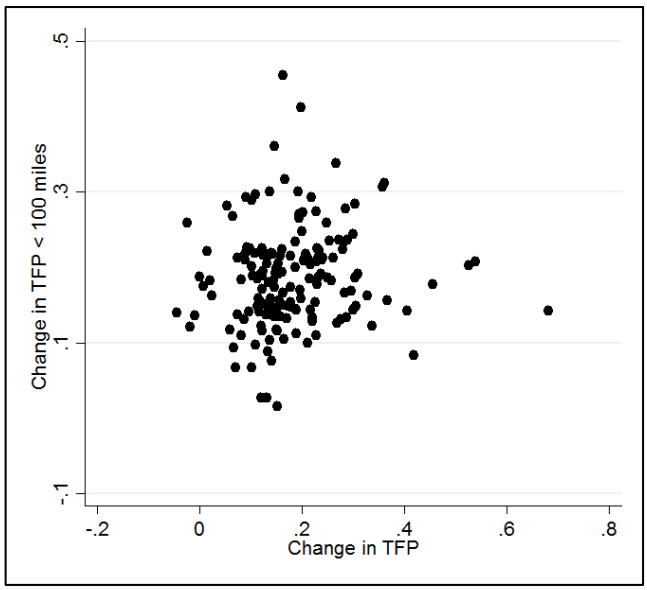
Notes: Panels A and B show total factor productivity (TFP) in 1980 and 1990 for the 193 sample MSAs, and Panel C shows the change in TFP from 1980 to 1990. MSAs are separated into 10 groups, with darker shaded groups representing MSAs with greater TFP (or a greater relative change in TFP). Panel D shows manufacturing output for each sample MSA in 1980, with darker shades representing greater manufacturing output.

Figure 2. Serial Correlation and Spatial Correlation in TFP Changes

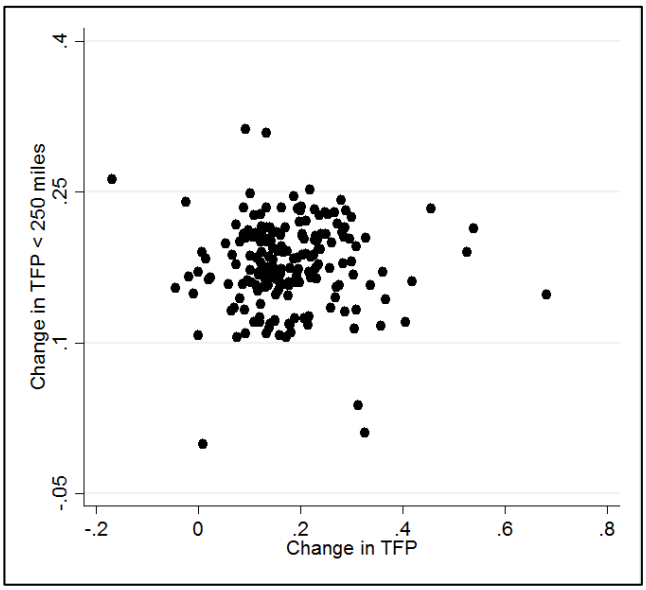
Panel A. 1980-1990 vs. 1990-2000



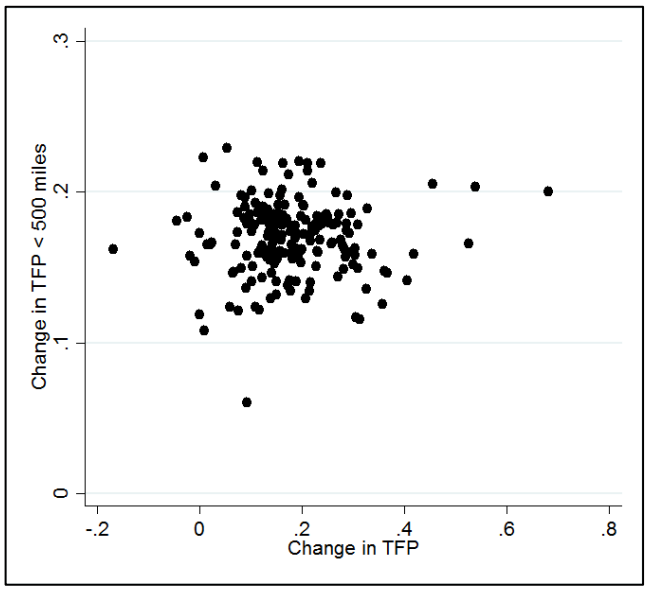
Panel B. Local vs. Within 100 Miles



Panel C. Local vs. Within 250 Miles



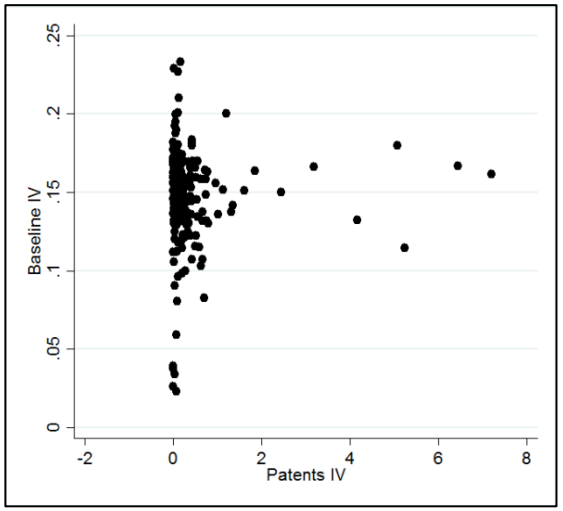
Panel D. Local vs. Within 500 Miles



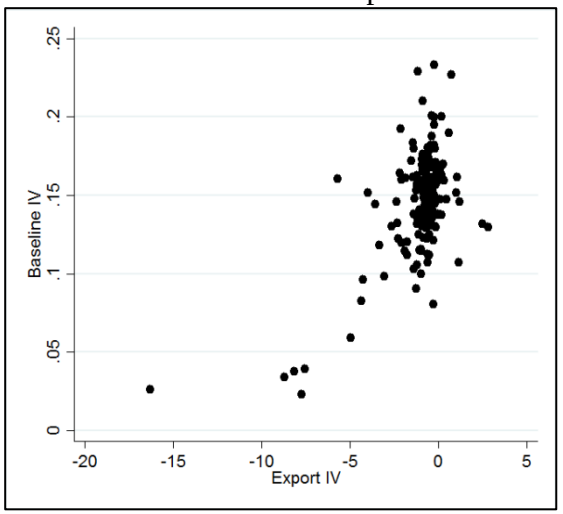
Notes: Panels show correlations between changes in TFP. Panel A: changes in city TFP from 1980 to 1990 vs. changes in city TFP from 1990 to 2000 (coefficient -0.232, standard error 0.136, R-squared 0.025). Panels B – D: changes in city TFP from 1980 to 1990 vs. changes in nearby cities' average TFP from 1980 to 1990 within 100 miles (coefficient 0.062, standard error 0.046, R-squared 0.009) within 250 miles (coefficient -0.004, standard error 0.036, R-squared 0.000) or within 500 miles (coefficient 0.009, standard error 0.018, R-squared 0.001).

Figure 3. Pairwise Correlations Between Alternative Instrumental Variables (Baseline, Patent, Export, Stock)

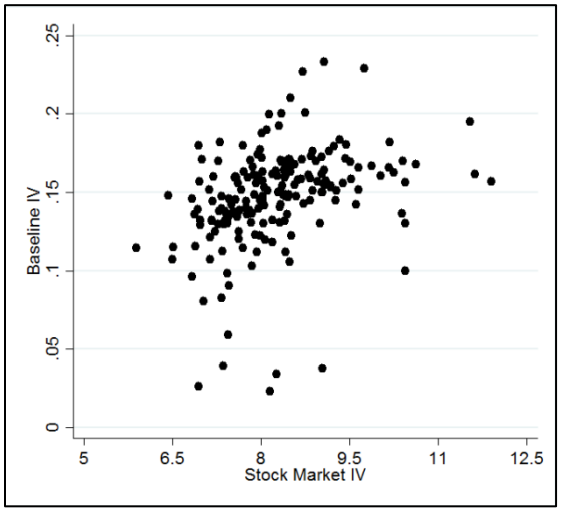
Panel A. Baseline IV vs. Patent IV



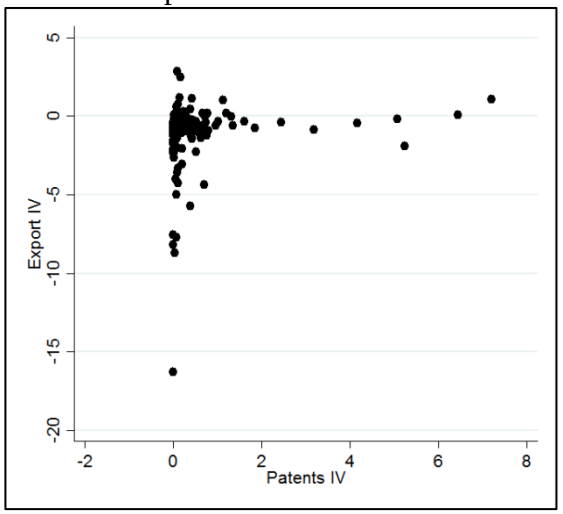
Panel B. Baseline IV vs. Export IV



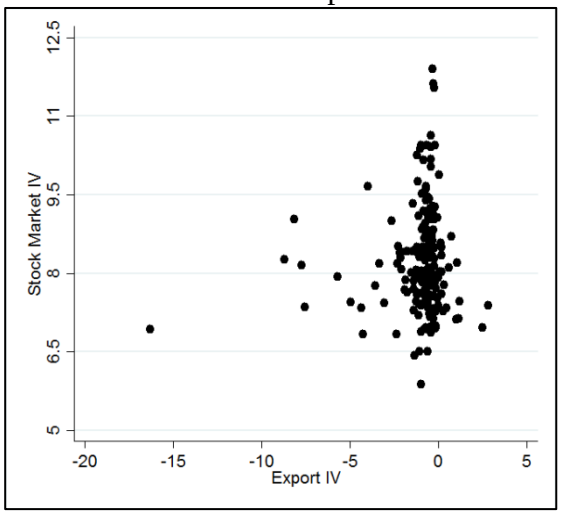
Panel C. Baseline IV vs. Stock IV



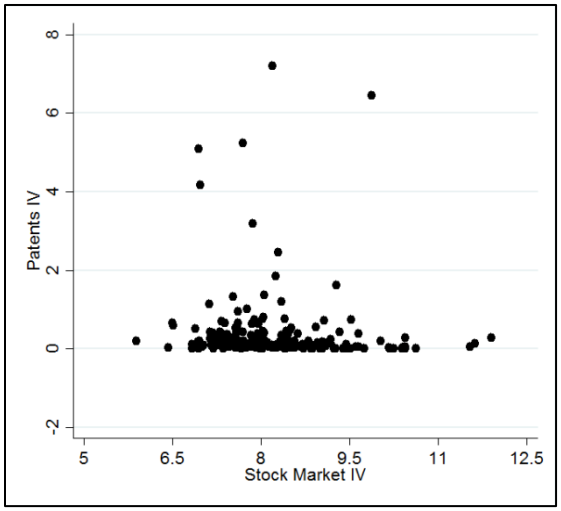
Panel D. Export IV vs. Patent IV



Panel E. Stock IV vs. Export IV



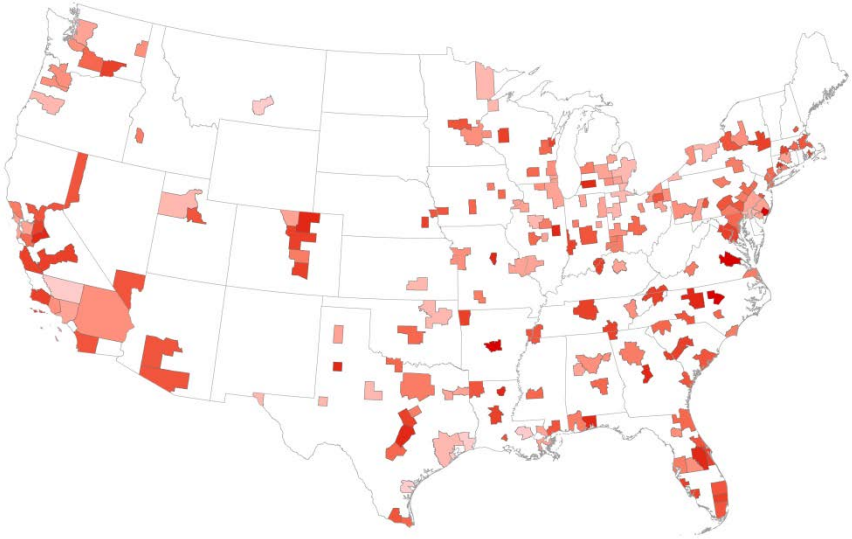
Panel F. Patent IV vs. Stock IV



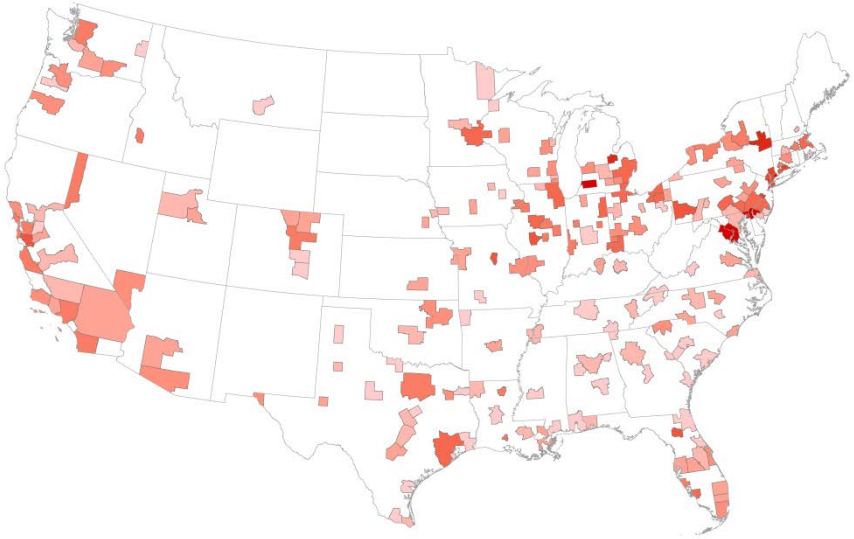
Notes: Each Panel shows the pairwise correlation between two alternative instruments for predicting TFP growth between 1980 and 1990: Baseline IV vs. Patent IV (coefficient 0.001, standard error 0.002, R-squared 0.002); Baseline IV vs. Export IV (coefficient 0.010, standard error 0.002, R-squared 0.358); Baseline IV vs. Stock Market IV (coefficient 0.011, standard error 0.002, R-squared 0.123); Export IV vs. Patent IV (coefficient 0.221, standard error 0.079, R-squared 0.014); Stock Market IV vs. Export IV (coefficient 0.041, standard error 0.030, R-squared 0.006); and Patent IV vs. Stock Market IV (coefficient -0.076, standard error 0.072, R-squared 0.006).

Figure 4. Spatial Distribution of Instrumental Variables

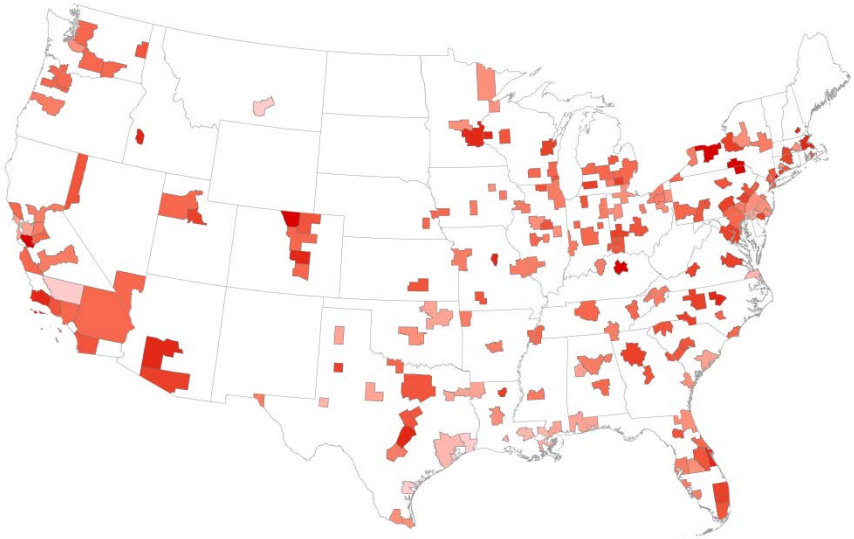
Panel A. Baseline IV



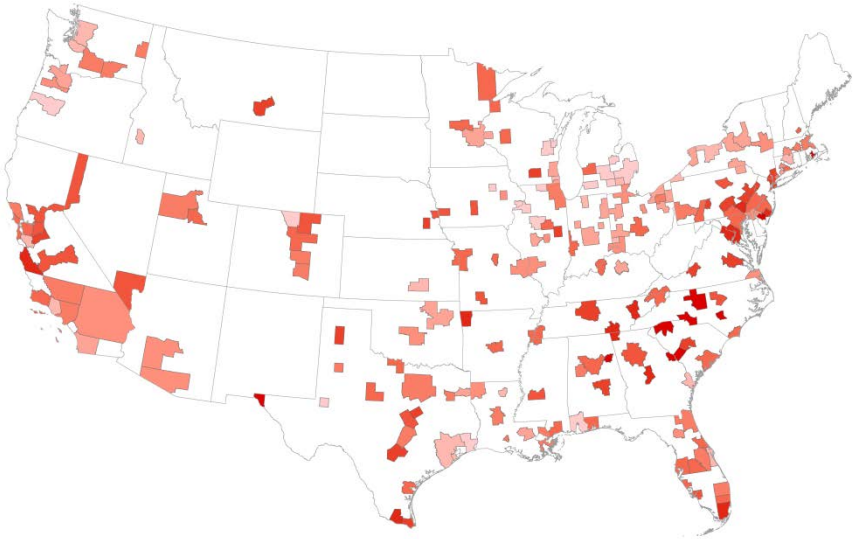
Panel B. Patent IV



Panel C. Export IV



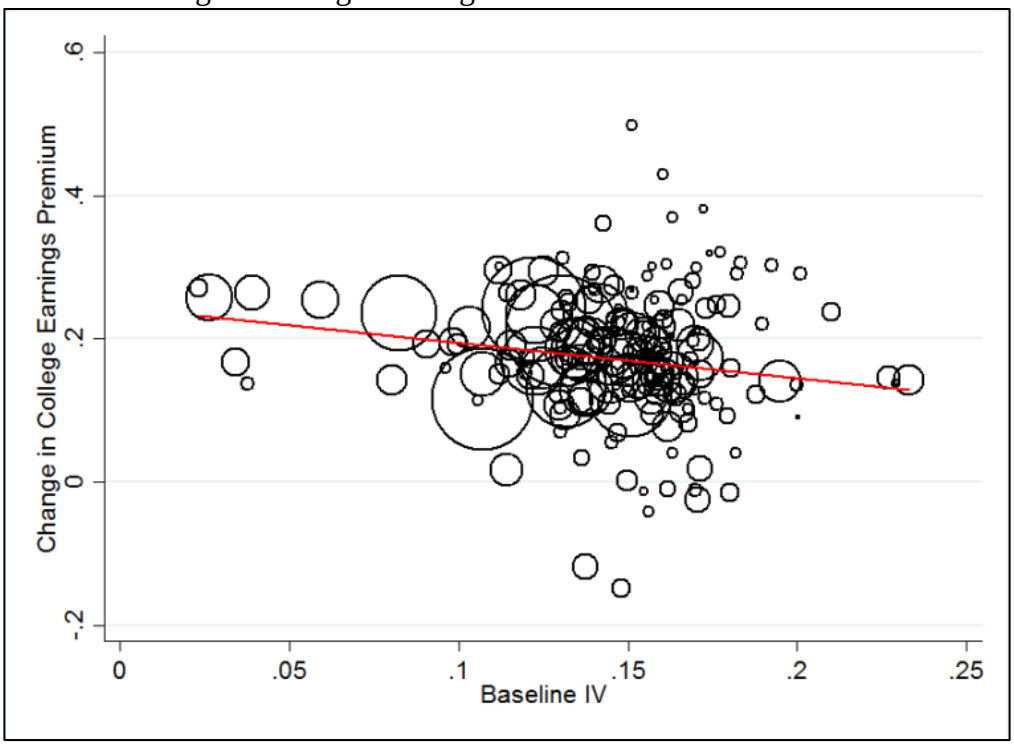
Panel D. Stock Market IV



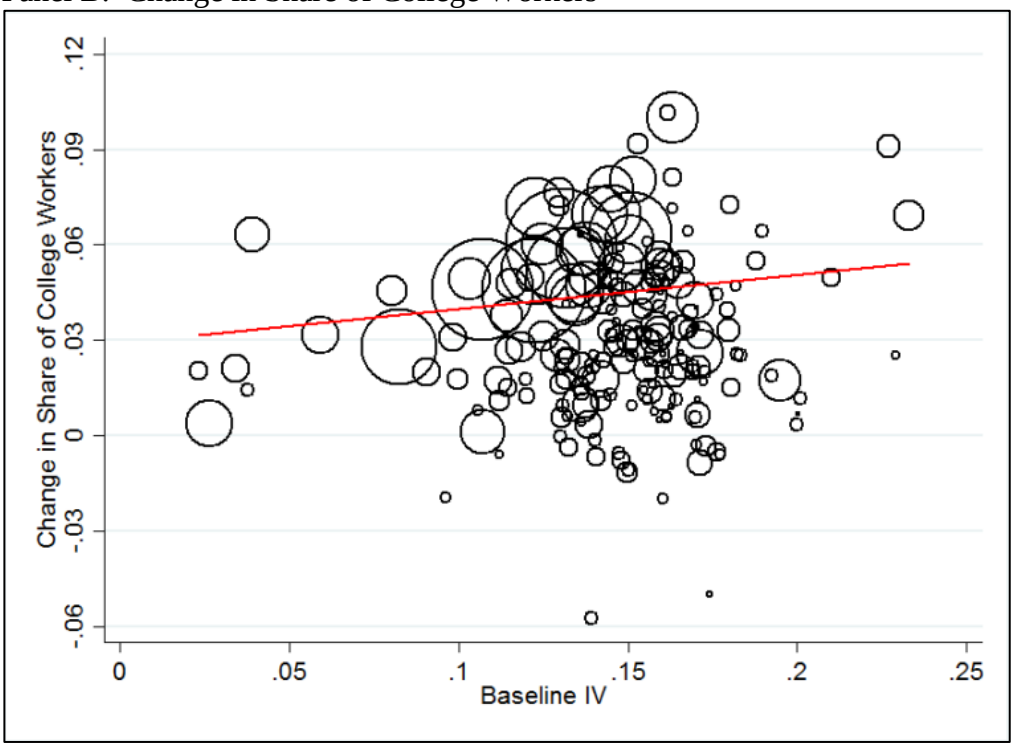
Notes: For each indicated instrument, each Panel shows the geographic variation in predicted TFP growth from 1980 to 1990. Darker shaded MSAs correspond to larger values of the instrument (and larger predicted growth in TFP), with MSAs grouped into 10 equal-sized bins.

Figure 5. Local TFP Growth and Labor Market Outcomes by Education Group

Panel A. Change in College Earnings Premium



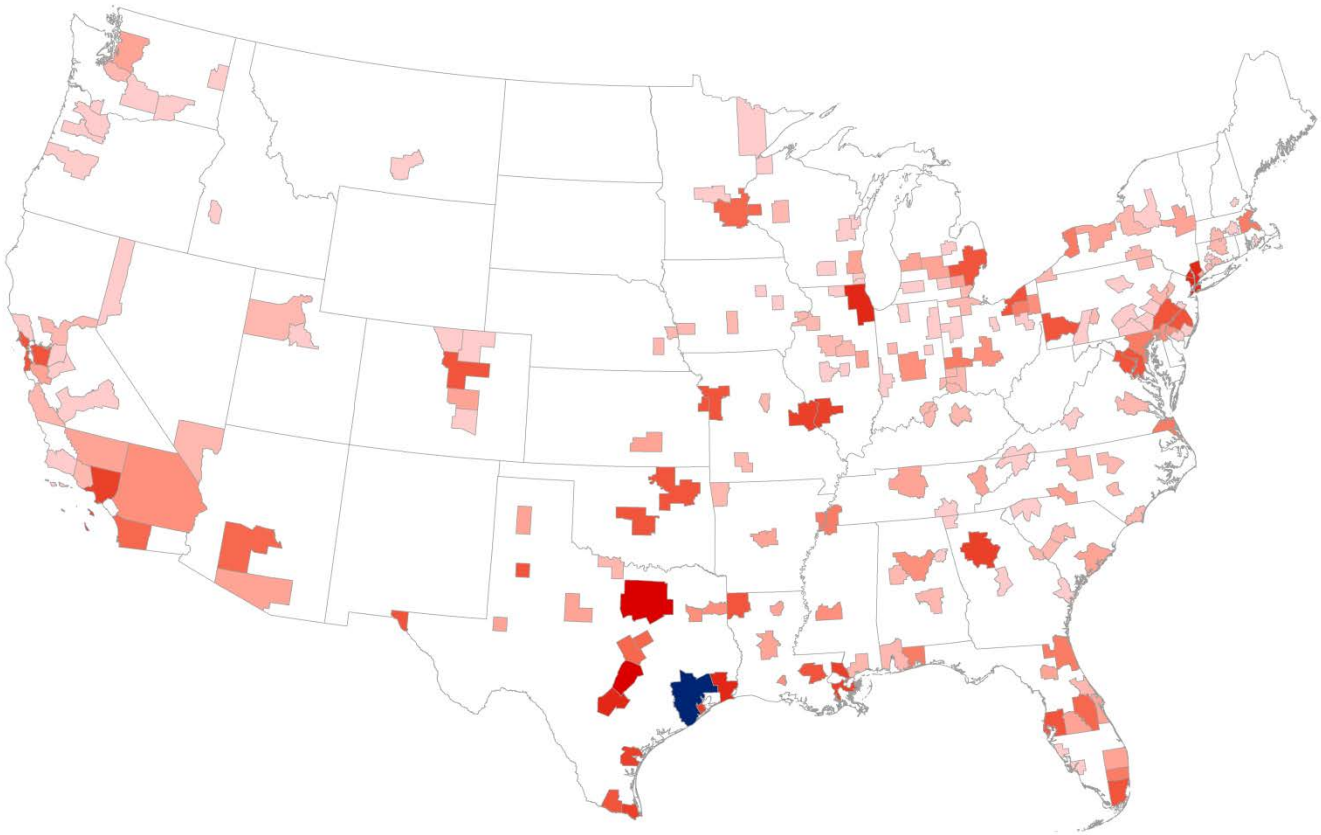
Panel B. Change in Share of College Workers



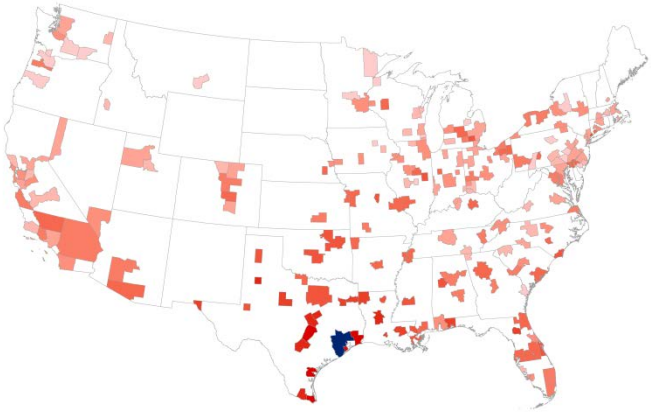
Notes: Panel A plots the change in city-level college earnings premium from 1980 to 1990 (log earnings of workers with four years of college education – log earnings of workers with no college education) against predicted local TFP growth from 1980 to 1990 (based on our baseline instrument). The estimated coefficient is -0.495 (0.183). Panels B plots the change in city-level share of college workers with estimated coefficients of 0.108 (0.072). Circle sizes reflect MSA manufacturing output.

Figure 6. Indirect Effects of a TFP Shock in Houston (Blue) on Other MSAs

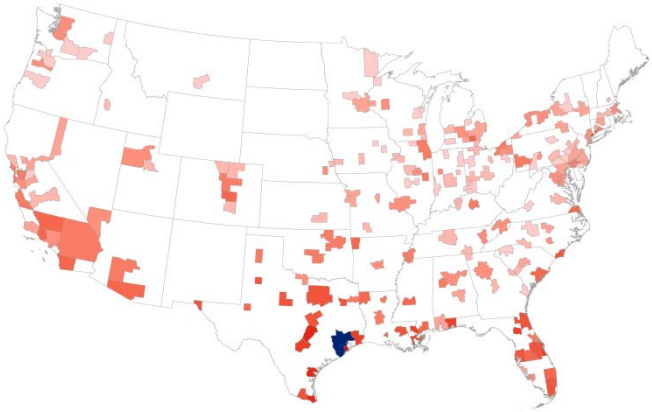
Panel A. Indirect Effects on Employment in Other MSAs



Panel B. Indirect Effects on Earnings



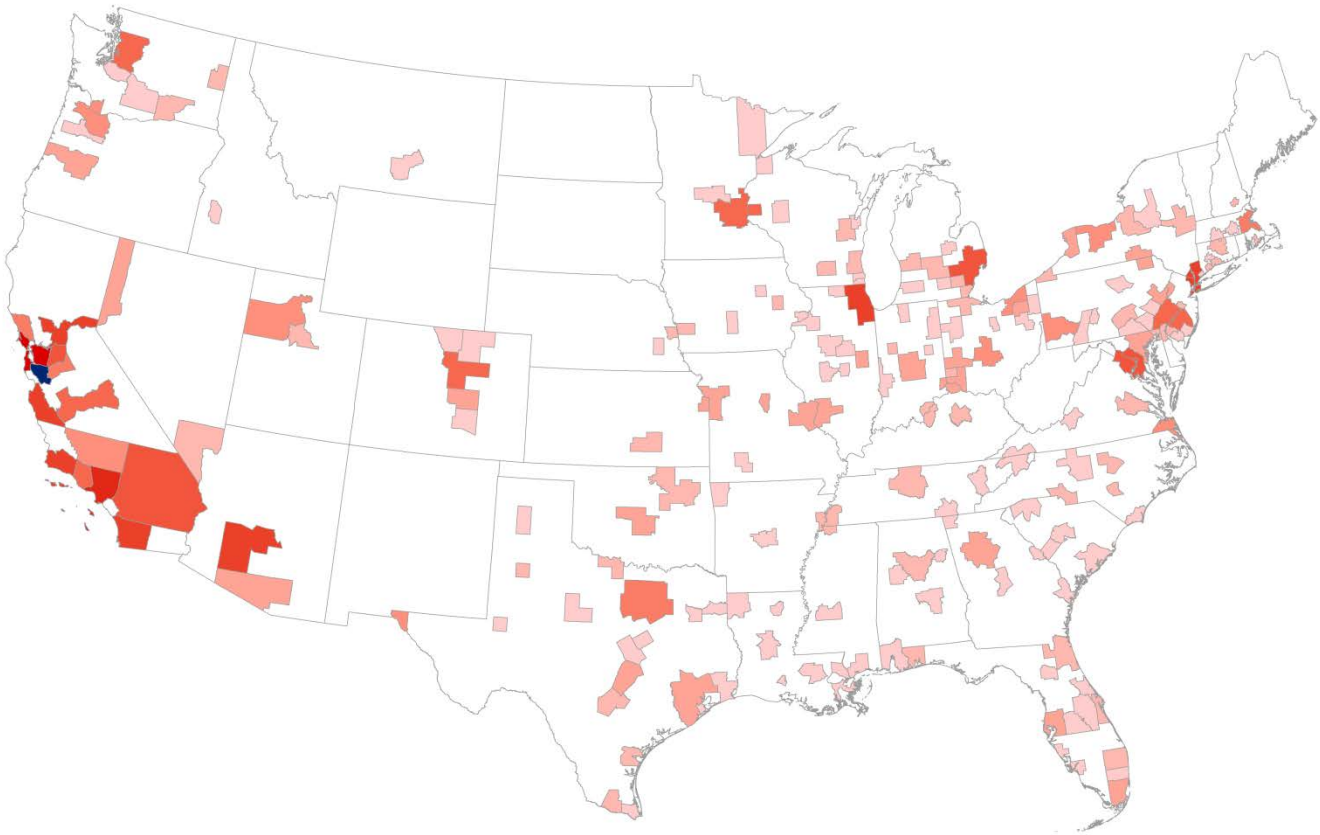
Panel C. Indirect Effects on Housing Rent



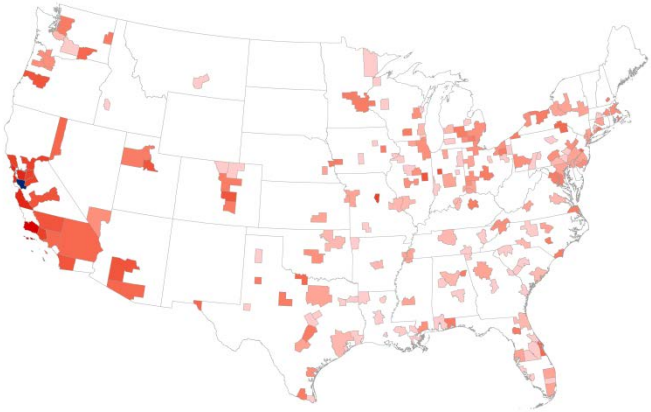
Notes: Each Panel shows the geographic distribution of estimated indirect effects from local TFP growth in Houston (1980 to 1990) on employment, earnings, and housing rent in other MSAs (1980 to 2000). MSAs are in 10 equal-sized bins, with darker-shaded MSAs receiving larger indirect effects (negative in Panels A and C, positive in Panel B). Houston is in dark blue.

Figure 7. Indirect Effects of a TFP Shock in San Jose (Blue) on Other MSAs

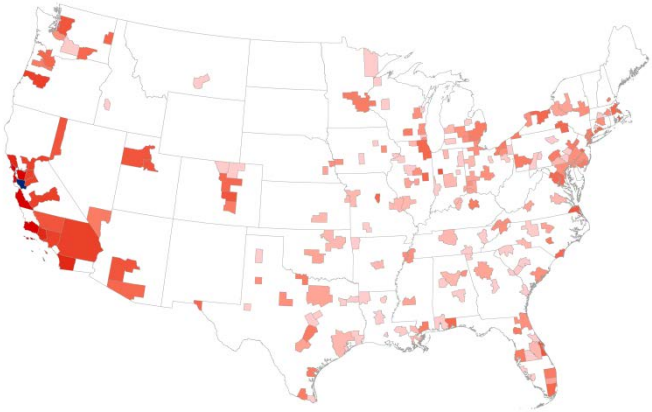
Panel A. Indirect Effects on Employment in Other MSAs



Panel B. Indirect Effects on Earnings



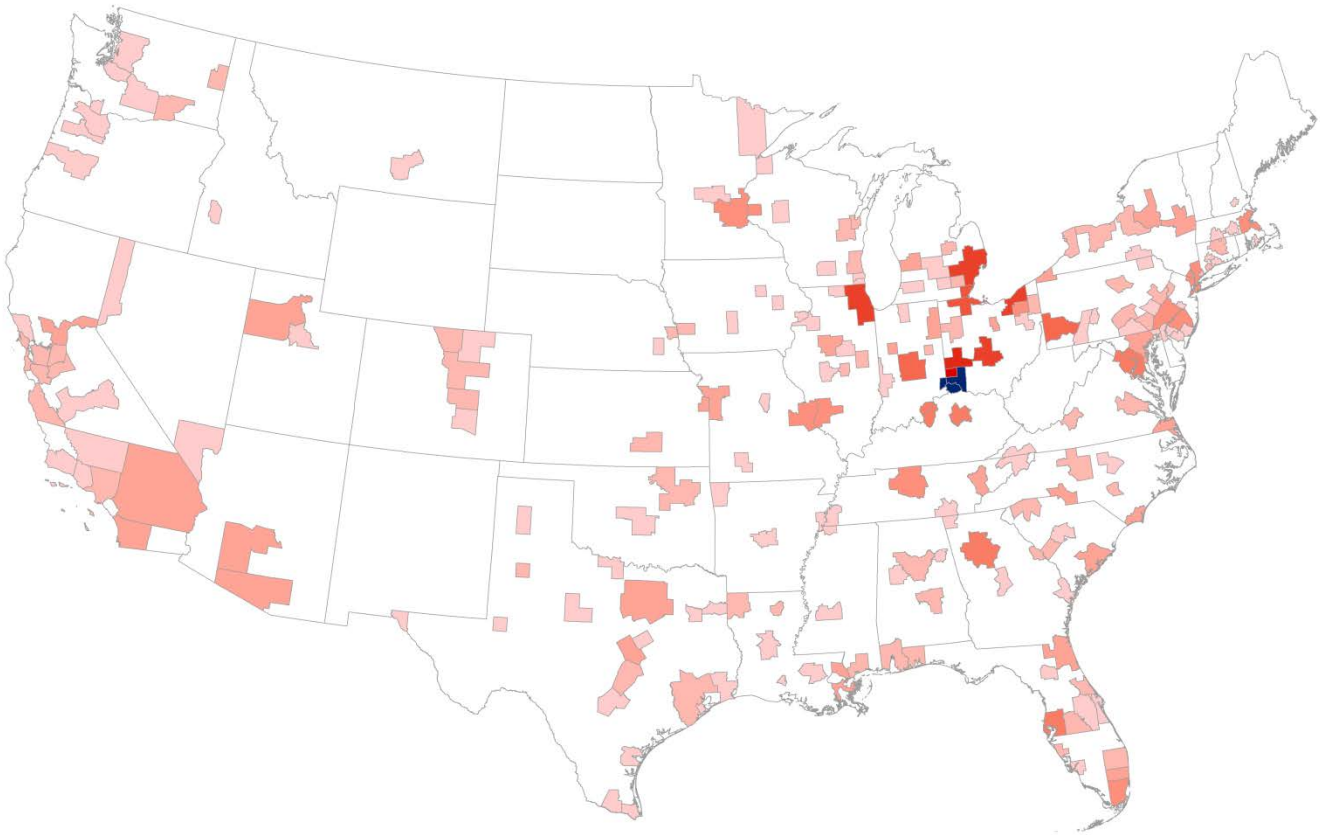
Panel C. Indirect Effects on Housing Rent



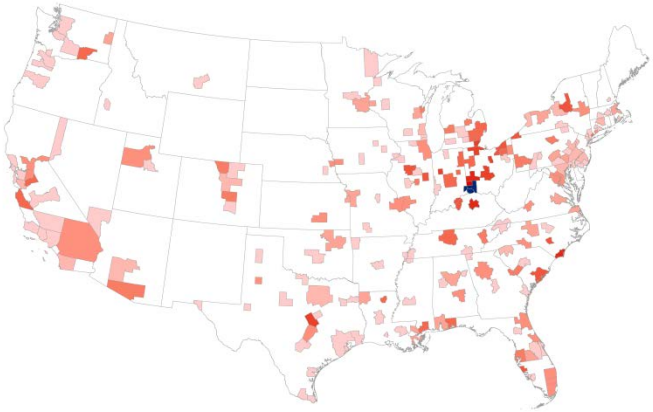
Notes: Each Panel shows the geographic distribution of estimated indirect effects from local TFP growth in San Jose (1980 to 1990) on employment, earnings, and housing rent in other MSAs (1980 to 2000). MSAs are in 10 equal-sized bins, with darker-shaded MSAs receiving larger indirect effects (negative in Panels A and C, positive in Panel B). San Jose is in dark blue.

Figure 8. Indirect Effects of a TFP Shock in Cincinnati (Blue) on Other MSAs

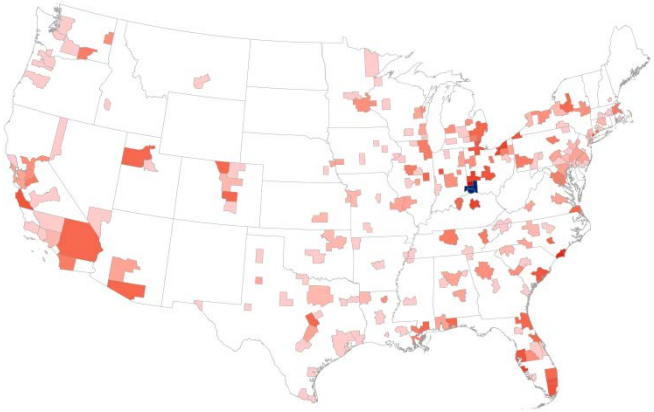
Panel A. Indirect Effects on Employment in Other MSAs



Panel B. Indirect Effects on Earnings



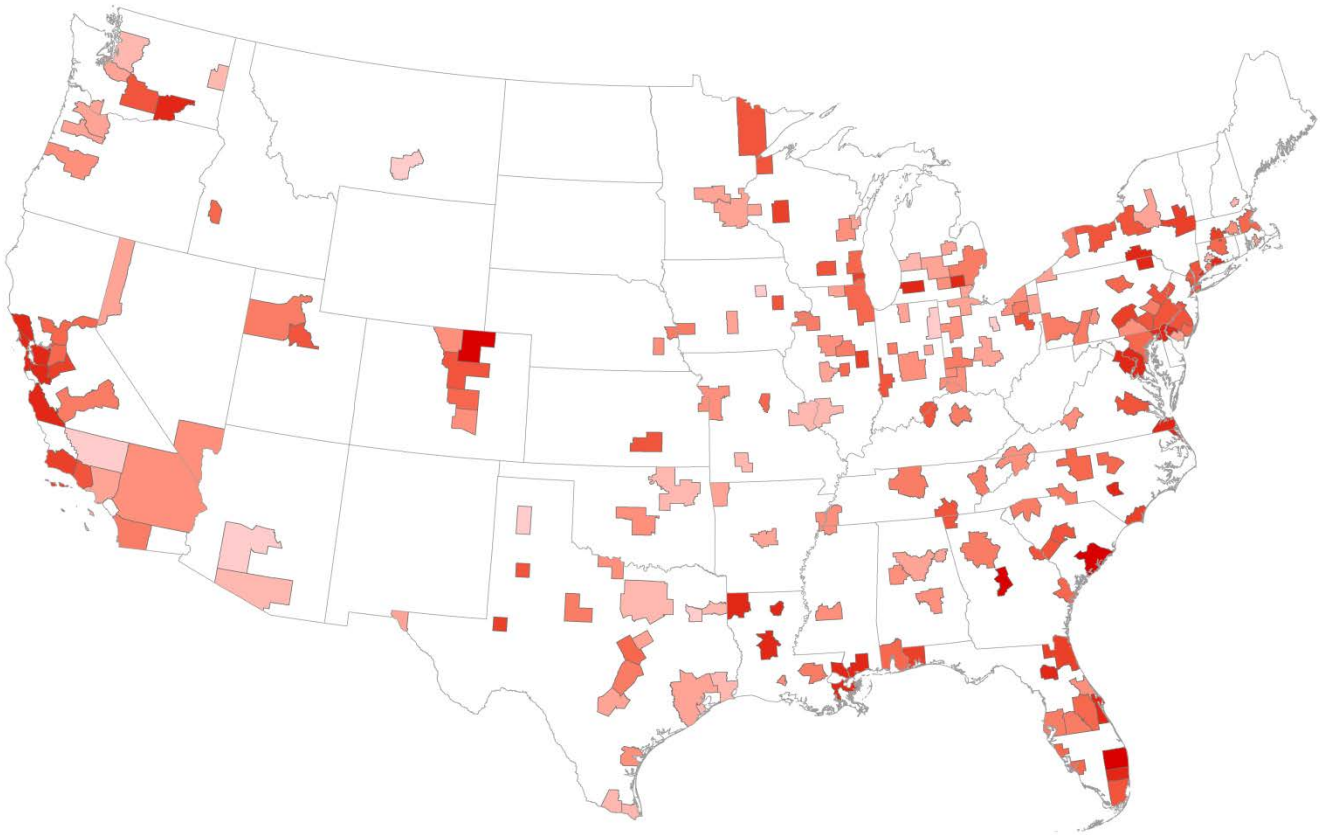
Panel C. Indirect Effects on Housing Rent



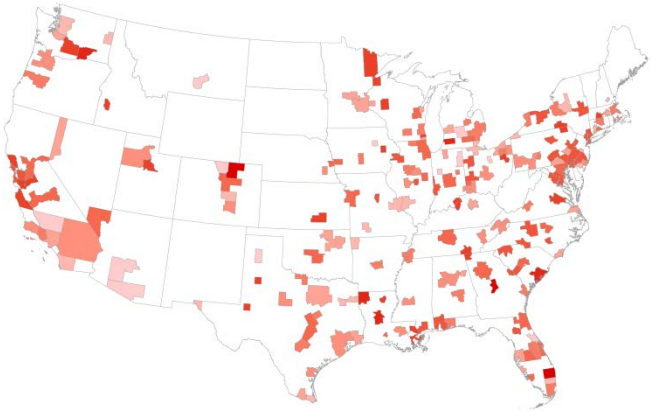
Notes: Each Panel shows the geographic distribution of estimated indirect effects from local TFP growth in Cincinnati (1980 to 1990) on employment, earnings, and housing rent in other MSAs (1980 to 2000). MSAs are in 10 equal-sized bins, with darker-shaded MSAs receiving larger indirect effects (negative in Panels A and C, positive in Panel B). Cincinnati is in dark blue.

Figure 9. Direct, Indirect, and Combined Effects of TFP Growth on Purchasing Power of Renters

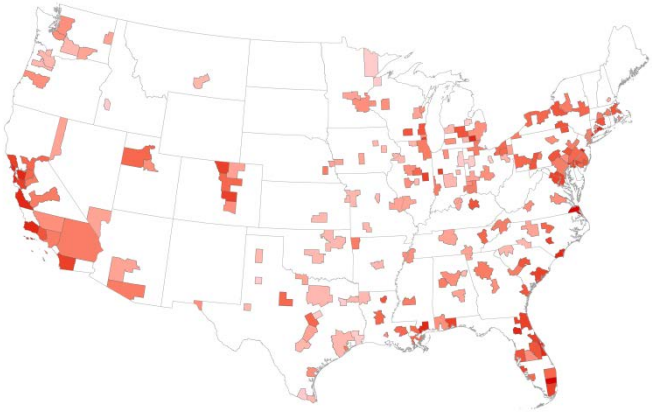
Panel A. Combined Effects of TFP Growth in All MSAs



Panel B. Direct Effects

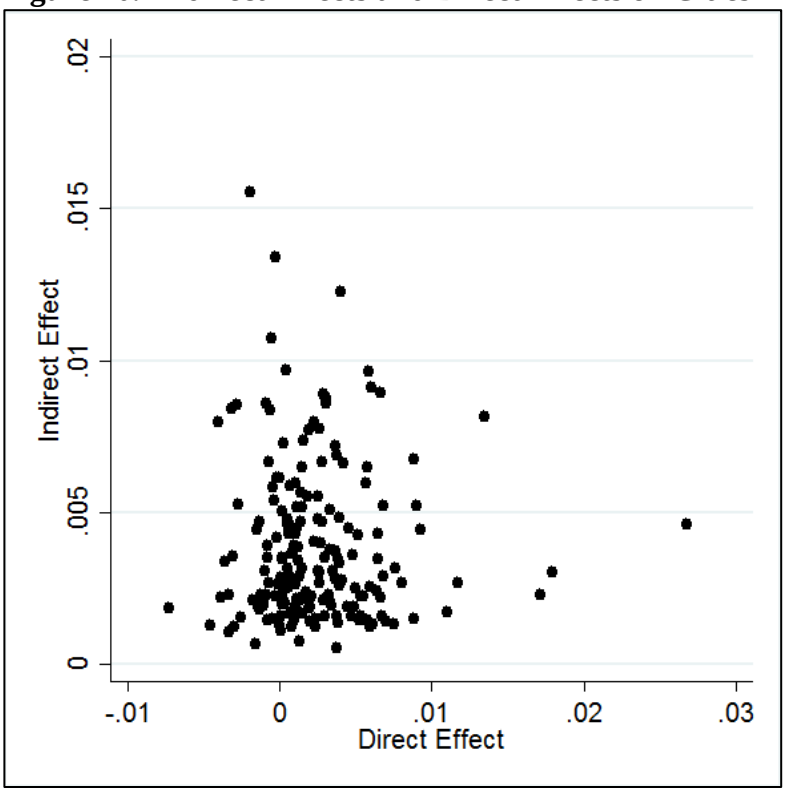


Panel C. Indirect Effects



Notes: Each Panel shows the geographic distribution of estimated combined effects on purchasing power of renters (Panel A), direct effects on purchasing power of renters (Panel B), and indirect effects on purchasing power of renters (Panel C) from TFP growth in each MSA. MSAs are in 10 equal-sized bins, with darker-shaded MSAs receiving larger effects.

Figure 10. Indirect Effects and Direct Effects on Cities from TFP Growth



Notes: For each city (MSA), this figure plots the annualized indirect effect of TFP growth on purchasing power of renters (in percentage terms) against the annualized direct effect of TFP growth on log purchasing power of renters (in percentage terms). The estimated coefficient is -0.064, with a standard error of 0.043, and an R-squared of 0.000.

Table 1. Cities (MSAs) with the Largest and Smallest Predicted Changes in TFP

	Baseline IV (1)	Patent IV (2)	Export IV (3)	Stock Market IV (4)
Largest Values				
1.	Richmond, VA	Stamford, CT	Lexington, KY	Greenville, SC
2.	Atlantic City, NJ	Washington, DC	Fort Collins, CO	Charlotte, NC
3.	Raleigh-Durham, NC	Wilmington, DE	Binghamton, NY	Greensboro, NC
4.	Little Rock, AR	Kalamazoo, MI	Rochester, NY	Augusta, GA
5.	Greeley, CO	Saginaw, MI	Stamford, CT	Fayetteville, NC
6.	Columbia, MO	Albany, NY	San Jose, CA	Vineland, NJ
7.	Lubbock, TX	New Haven, CT	Raleigh-Durham, NC	El Paso, TX
8.	Greensboro, NC	Trenton, NJ	Austin, TX	New Bedford, MA
9.	Pensacola, FL	New York, NY	Boise City, ID	Anniston, AL
10.	Austin, TX	Pittsburgh, PA	Phoenix, AZ	McAllen, TX
Smallest Values				
1.	Bakersfield, CA	Billings, MT	Beaumont-Port Arthur,TX	Eugene-Springfield, OR
2.	Beaumont-Port Arthur,TX	Montgomery, AL	Corpus Christi, TX	Waterloo-Cedar Falls, IA
3.	Corpus Christi, TX	Mobile, AL	Billings, MT	Detroit, MI
4.	Billings, MT	Alexandria, LA	Bakersfield, CA	Peoria, IL
5.	Galveston-Texas City, TX	Augusta-Aiken, GA-SC	Galveston-Texas City, TX	Odessa, TX
6.	Baton Rouge, LA	Abilene, TX	Lafayette, LA	Mobile, AL
7.	Wichita, KS	Nashville, TN	Baton Rouge, LA	Rockford, IL
8.	Houston-Brazoria, TX	Fayetteville, AR	Houston-Brazoria, TX	Davenport, IA
9.	Lima, OH	McAllen-Edinburg, TX	Odessa, TX	Jackson, MI
10.	Odessa, TX	Galveston-Texas City, TX	Anchorage, AK	Beaumont-Port Arthur,TX

Notes: Entries are the sample cities (MSAs) with the largest and smallest predicted growth in TFP from 1980 to 1990 for each of the instrumental variables: the baseline instrument (Column 1), the intensity of patenting activity instrument (Column 2), the export exposure instrument (Column 3), and stock market return instrument (Column 4).

Table 2. Direct Effect of Local TFP Growth on Local Employment, Earnings, and Housing Costs (Baseline IV)

	Medium-run Effect: Change from 1980 to 1990	Long-run Effect: Change from 1980 to 2000	Longer-run Effect: Change from 1980 to 2010
	(1)	(2)	(3)
Panel A. Log Employment	2.38***	4.16***	4.03***
	(0.80)	(1.26)	(1.52)
Panel B. Log Earnings	0.91***	1.45***	1.46***
	(0.32)	(0.47)	(0.50)
Panel C. Log Cost of Rent	0.98**	1.47***	1.09**
	(0.43)	(0.46)	(0.48)
Panel D. Log Home Value	1.74**	2.46***	3.05***
	(0.72)	(0.78)	(0.98)
Panel E. Log Purchasing Power			
Renters	0.36**	0.62**	0.85***
	(0.18)	(0.26)	(0.30)
Homeowners (Case A)	0.68***	1.11***	1.21***
	(0.24)	(0.37)	(0.41)
Homeowners (Case B)	1.01***	1.60***	1.57***
	(0.35)	(0.51)	(0.54)
First Stage Coefficient	0.80***	0.80***	0.80***
	(0.17)	(0.17)	(0.17)
Instrument F-statistic	23.64	23.64	23.64

Notes: Columns 1 to 3 report estimates from equations 8, 9, and 10 in the text, respectively. Entries are the estimated coefficient on the change in city TFP from 1980 to 1990. In Column 1, the dependent variables are in changes from 1980 to 1990. In Columns 2 and 3, the dependent variables are in changes from 1980 to 2000 (Column 2) and in changes from 1980 to 2010 (Column 3). In each column, we instrument for changes in city TFP using the predicted change in TFP, based on our baseline instrument. The corresponding first-stage estimate is reported in the row at the bottom of the Table, with the associated F-statistic on the excluded instrument. In all specifications, the sample is our balanced sample of 193 MSAs. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 3. Direct Effect of Local TFP Growth on Local Employment, Earnings, and Housing Costs (Additional IVs)

	Medium-run Effect: Change from 1980 to 1990				Long-run Effect: Change from 1980 to 2000				Longer-run Effect: Change from 1980 to 2010			
	Baseline IV	Patent IV	Export IV	3 IVs Combined	Baseline IV	Patent IV	Export IV	3 IVs Combined	Baseline IV	Patent IV	Export IV	3 IVs Combined
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Log Employment	2.38***	0.66	3.94***	1.88***	4.16***	1.71	5.89***	3.50***	4.03***	1.31	5.31**	3.36***
	(0.80)	(0.82)	(1.44)	(0.63)	(1.26)	(1.46)	(2.25)	(0.96)	(1.52)	(1.64)	(2.58)	(1.15)
P-value of over-id test				0.18				0.32				0.45
Panel B. Log Earnings	0.91***	1.11**	1.53***	0.88***	1.45***	2.08*	2.27***	1.48***	1.46***	2.22*	1.85**	1.57***
	(0.32)	(0.53)	(0.56)	(0.28)	(0.47)	(1.14)	(0.81)	(0.45)	(0.50)	(1.33)	(0.72)	(0.52)
P-value of over-id test				0.23				0.25				0.61
Panel C. Log Cost of Rent	0.98**	2.13**	1.72***	1.12***	1.47***	1.90*	2.13***	1.49***	1.09**	2.54*	1.47**	1.33***
	(0.43)	(1.03)	(0.57)	(0.41)	(0.46)	(1.05)	(0.68)	(0.45)	(0.48)	(1.35)	(0.70)	(0.50)
P-value of over-id test				0.09				0.30				0.29
Panel D. Log Home Value	1.74**	3.73**	2.98***	2.00***	2.46***	2.86*	3.55***	2.42***	3.05***	4.33*	3.81***	3.21***
	(0.72)	(1.87)	(0.90)	(0.71)	(0.78)	(1.49)	(0.95)	(0.74)	(0.98)	(2.29)	(1.24)	(1.00)
P-value of over-id test				0.13				0.33				0.59
Panel E. Log Purchasing Power												
Renters	0.36**	-0.09	0.57*	0.25*	0.62**	1.02	1.08**	0.65***	0.85***	0.79	1.02**	0.82***
	(0.18)	(0.22)	(0.31)	(0.14)	(0.26)	(0.63)	(0.46)	(0.24)	(0.30)	(0.77)	(0.40)	(0.31)
P-value of over-id test				0.29				0.30				0.85
Homeowners (Case A)	0.68***	0.62*	1.13**	0.62***	1.11***	1.64*	1.78***	1.14***	1.21***	1.63	1.51***	1.26***
	(0.24)	(0.33)	(0.44)	(0.20)	(0.37)	(0.92)	(0.66)	(0.36)	(0.41)	(1.07)	(0.58)	(0.42)
P-value of over-id test				0.38				0.26				0.72
Homeowners (Case B)	1.01***	1.32**	1.70***	0.99***	1.60***	2.27*	2.49***	1.63***	1.57***	2.47*	1.99**	1.70***
	(0.35)	(0.63)	(0.61)	(0.31)	(0.51)	(1.24)	(0.88)	(0.49)	(0.54)	(1.45)	(0.78)	(0.56)
P-value of over-id test				0.20				0.25				0.57
First Stage Coefficient	0.80***	0.016**	0.008**		0.80***	0.016**	0.008**		0.80***	0.016**	0.008**	
	(0.17)	(0.007)	(0.003)		(0.17)	(0.007)	(0.003)		(0.17)	(0.007)	(0.003)	
Instrument F-statistic	23.64	5.86	6.31	9.48	23.64	5.86	6.31	9.48	23.64	5.86	6.31	9.48

Notes: The estimates correspond to those in Table 2, using alternative instrumental variables. Columns 1, 5, and 9 correspond to columns 1, 2, and 3 in Table 2, as a basis for comparison. Columns 2, 6, and 10 use an instrument based on patenting activity. Columns 3, 7, and 11 use an instrument based on increased exposure to export markets. Columns 4, 8, and 12 use the baseline IV in combination with the patent IV and exports IV, and below each estimate we report the p-value of the over-identification test (Hansen J statistic)Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 4. Direct Effect of Local TFP Growth on Local Employment, Earnings, and Housing Costs (Additional IVs)

	Medium-run Effect: Change from 1980 to 1990				Long-run Effect: Change from 1980 to 2000				Longer-run Effect: Change from 1980 to 2010			
	Baseline IV	Stock IV	Both IVs	4 IVs Combined	Baseline IV	Stock IV	Both IVs	4 IVs Combined	Baseline IV	Stock IV	Both IVs	4 IVs Combined
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Log Employment	2.38***	2.20***	2.34***	1.90***	4.16***	2.92**	3.91***	3.35***	4.03***	3.47*	3.91***	3.32***
	(0.80)	(0.78)	(0.71)	(0.57)	(1.26)	(1.21)	(1.11)	(0.87)	(1.52)	(1.81)	(1.43)	(1.10)
P-value of over-id test			0.85	0.32			0.42	0.47			0.75	0.66
Panel B. Log Earnings	0.91***	1.20***	0.97***	0.94***	1.45***	1.72***	1.50***	1.54***	1.46***	2.04***	1.58***	1.67***
	(0.32)	(0.38)	(0.30)	(0.27)	(0.47)	(0.56)	(0.44)	(0.43)	(0.50)	(0.75)	(0.50)	(0.51)
P-value of over-id test			0.40	0.25			0.59	0.31			0.32	0.45
Panel C. Log Cost of Rent	0.98**	1.75***	1.13***	1.26***	1.47***	2.25***	1.63***	1.63***	1.09**	2.24**	1.32***	1.52***
	(0.43)	(0.56)	(0.41)	(0.40)	(0.46)	(0.69)	(0.46)	(0.45)	(0.48)	(0.93)	(0.51)	(0.53)
P-value of over-id test			0.09	0.10			0.16	0.16			0.09	0.20
Panel D. Log Home Value	1.74**	3.03***	2.00***	2.22***	2.46***	2.45**	2.46***	2.44***	3.05***	4.41***	3.32***	3.45***
	(0.72)	(1.07)	(0.69)	(0.69)	(0.78)	(1.12)	(0.78)	(0.74)	(0.98)	(1.65)	(1.00)	(1.01)
P-value of over-id test			0.15	0.14			1.00	0.48			0.27	0.49
Panel E. Log Purchasing Power												
Renters	0.36**	0.22	0.33**	0.24*	0.62**	0.46	0.59**	0.63***	0.85***	0.79*	0.84***	0.81***
	(0.18)	(0.20)	(0.17)	(0.14)	(0.26)	(0.30)	(0.25)	(0.24)	(0.30)	(0.41)	(0.29)	(0.29)
P-value of over-id test			0.47	0.47			0.56	0.48			0.50	0.96
Homeowners (Case A)	0.68***	0.80***	0.71***	0.65***	1.11***	1.20***	1.13***	1.17***	1.21***	1.53***	1.27***	1.32***
	(0.24)	(0.28)	(0.22)	(0.19)	(0.37)	(0.43)	(0.35)	(0.34)	(0.41)	(0.58)	(0.40)	(0.41)
P-value of over-id test			0.66	0.43			0.82	0.38			0.27	0.67
Homeowners (Case B)	1.01***	1.38***	1.08***	1.07***	1.60***	1.94***	1.67***	1.71***	1.57***	2.27***	1.71***	1.82***
	(0.35)	(0.43)	(0.33)	(0.30)	(0.51)	(0.62)	(0.48)	(0.47)	(0.54)	(0.83)	(0.54)	(0.56)
P-value of over-id test			0.34	0.21			0.53	0.29			0.88	0.40
First Stage Coefficient	0.80***	0.021***			0.80***	0.021***			0.80***	0.021***		
	(0.17)	(0.006)			(0.17)	(0.006)			(0.17)	(0.006)		
Instrument F-statistic	23.64	13.34	14.43	9.47	23.64	13.34	14.43	9.47	23.64	13.34	14.43	9.47

Notes: The estimates correspond to those in Table 3, using an additional instrument on its own or in combination with the other instruments from Table 3. Columns 2, 6, and 10 use an instrument based on stock market returns. Columns 3, 7, and 11 use the stock market IV and the baseline IV. Columns 4, 8 , and 12 use all four instrumental variables in combination. Below each estimate using multiple instruments, we report the p-value of the over-identification test (Hansen J statistic). In the bottom row of the Table, we report the F-statistic on the excluded instruments. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 5. Direct Effect of Local TFP Growth, by Education Level

	Medium-run Effect: Change from 1980 to 1990 (2SLS)				Long-run Effect: Change from 1980 to 2000 (2SLS)			
	College	Some College	High School or less	Difference: (1) - (3)	College	Some College	High School or less	Difference: (5) - (7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Log Employment	2.79**	2.60***	2.31***	0.48	5.82***	4.88***	3.23***	2.58***
	(1.13)	(0.73)	(0.78)	(0.66)	(1.88)	(1.25)	(1.15)	(1.16)
Panel B. Log Earnings	0.60**	0.67***	1.12***	-0.52***	0.87**	1.06***	1.23***	-0.36
	(0.24)	(0.26)	(0.30)	(0.20)	(0.34)	(0.33)	(0.35)	(0.29)
Panel C. Log Cost of Rent	0.55	1.02***	1.08**	-0.53**	1.01*	1.50***	1.48***	-0.47
	(0.44)	(0.38)	(0.47)	(0.27)	(0.53)	(0.40)	(0.47)	(0.39)
Panel D. Log Home Value	1.59***	1.69**	1.99***	-0.40*	1.83***	2.14***	2.54***	-0.70***
	(0.58)	(0.74)	(0.77)	(0.30)	(0.59)	(0.71)	(0.77)	(0.31)
Panel E. Log Purchasing Power								
Renters	0.30	0.10	0.51***	-0.22	0.31	0.22	0.40**	-0.09
	(0.22)	(0.13)	(0.16)	(0.25)	(0.20)	(0.15)	(0.18)	(0.28)
Homeowners (Case A)	0.48**	0.43**	0.87***	-0.39**	0.64**	0.72***	0.89***	-0.25
	(0.20)	(0.18)	(0.21)	(0.21)	(0.25)	(0.25)	(0.26)	(0.26)
Homeowners (Case B)	0.66**	0.77***	1.23***	-0.57***	0.97**	1.21***	1.38***	-0.40*
	(0.27)	(0.29)	(0.34)	(0.20)	(0.38)	(0.37)	(0.39)	(0.31)

Notes: Columns 1 - 3 report estimates that correspond to those in column 1 of Table 2, but separately by skill group: completed 4 years of college or more (column 1), completed between 1 and 3 years of college (column 2), and completed 12 years of education or fewer (column 3). Column 4 reports the difference between column 1 and column 3. Columns 5 - 8 report analogous estimates for the long-run effect by skill-group, corresponding to the estimates in column 2 of Table 2. All entries are based on the baseline IV. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 6. Direct Effect of Local TFP Growth on Local Inequality

	Medium-run Effect: Change from 1980 to 1990	Long-run Effect: Change from 1980 to 2000
	(1)	(2)
Panel A. 90/10 Centile Difference in Log Earnings	-0.632***	-0.998**
	(0.225)	(0.420)
Panel B. 90/50 Centile Difference in Log Earnings	-0.574***	-0.930***
	(0.222)	(0.320)
Panel C. 50/10 Centile Difference in Log Earnings	-0.058	-0.068
	(0.236)	(0.292)

Notes: Column 1 reports estimates analogous to those reported in Column 1 of Table 2 (and Column 2 reports estimates analogous to those reported in Column 2 of Table 2), but for MSA-level outcomes that correspond to earnings inequality: the difference between log earnings at the 90th centile and the 10th centile of the MSA's earnings distribution (Panel A), the difference between log earnings at the 90th centile and the 50th centile (Panel B), and the difference between log earnings at the 50th centile and the 10th centile (Panel C). All entries are based on the baseline IV. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 7. Long-Run Direct And Indirect Effects of TFP Growth in Three Cities

	Direct Effects on Indicated City	Indirect Effects on Average Other City:			
		Baseline	Robustness		
	(1)	(2)	(3)	(4)	(5)
Panel A. Houston TFP Growth					
Employment	86,031	-291	-291	-291	-291
	(27,371)	(93)	(93)	(93)	(93)
Earnings	1,490	8.9	9.9	8.3	8.0
	(488)	(2.8)	(3.1)	(2.6)	(2.5)
Rent	501	-8.4	-12.4	-7.4	-8.4
	(160)	(2.6)	(3.9)	(2.3)	(2.6)
Panel B. San Jose TFP Growth					
Employment	361,765	-1,413	-1,413	-1,413	-1,413
	(151,101)	(590)	(590)	(590)	(590)
Earnings	11,756	51.1	47.0	42.4	48.1
	(4251)	(20.1)	(19.5)	(17.4)	(18.9)
Rent	3,957	-78.5	-57.7	-45.1	-78.5
	(1395)	(30.7)	(23.9)	(18.5)	(30.7)
Panel C. Cincinnati TFP Growth					
Employment	26,002	-84	-84	-84	-84
	(8,199)	(27)	(27)	(27)	(27)
Earnings	1,115	2.3	2.8	2.3	1.9
	(364)	(0.7)	(0.9)	(0.7)	(0.6)
Rent	375	-1.9	-3.5	-2.0	-1.9
	(119)	(0.6)	(1.1)	(0.6)	(0.6)

Notes: All monetary values are in 2017 dollars. Column 1 reports the direct effects of 1980 to 1990 TFP growth in Houston (panel A), San Jose (Panel B) and Cincinnati (Panel C) on 1980 to 2000 changes in employment, earnings, and rent in that same city. Column 2 reports indirect effects of 1980 to 1990 TFP growth in Houston (panel A), San Jose (Panel B), and Cincinnati (Panel C) on 1980 to 2000 changes in employment, earnings, and rent in the average other city, under our baseline assumption on migration flows that is based on measured migrant flows from 1975 to 1980. Columns 3 and 4 report indirect effects under alternative assumptions on migration flows: in Column 3, that migration flows from other sample cities are proportion to their population sizes; in Column 4, that migration flows are based on predicted migration flows only (taking the predicted values from regressing 1975-1980 migrant flows on log origin city size, log destination city size, log geographic distance, and log economic distance). Column 5 reports indirect effects for our baseline assumption on migration flows, but it allows the elasticity of labor demand to vary across cities according to their industry shares. Robust standard errors are reported in parantheses.

Table 8. Long-Run Direct Effects, Indirect Effects, and Combined Effects of Local TFP Growth

	Long-run Direct Effects on:				Long-run Indirect Effects on:								
	Earnings	Housing	Non-Tradables	Purchasing Power	Earnings	Housing	Non-Tradables	Purchasing Power	Total Effect	Total % Effect	Annual Total % Effect	Robustness: Annual Total % Effect	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. Renters													
	3,823	1,286	900	1,636	931	-1,059	-741	2,731	4,367	11.3%	0.56%	0.61%	0.49%
	(1,368)	(449)	(314)	(605)	(395)	(451)	(316)	(1,162)	(1,320)	(3.4%)	(0.17%)	(0.20%)	(0.15%)
Panel B. Homeowners													
Case A	5,008	-	1,180	3,828	1,343	-	-959	2,302	6,130	11.4%	0.57%	0.59%	0.56%
	(1,807)		(415)	(1,392)	(569)		(406)	(975)	(1,711)	(3.2%)	(0.16%)	(0.17%)	(0.16%)
Case B	5,008	1,685	1,180	5,514	1,343	-1,370	-959	932	6,445	12.0%	0.60%	0.60%	0.61%
	(1,807)	(593)	(415)	(1,985)	(569)	(580)	(406)	(395)	(2,028)	(3.8%)	(0.19%)	(0.19%)	(0.19%)

Notes: Entries are the average per-worker direct effects, indirect effects, and combined total effects of 1980 to 1990 TFP growth on 1980 to 2000 changes in outcomes in 2017 dollars. Columns 1 to 3 report direct effects of TFP growth on earnings, housing costs, and the cost of non-housing non-tradable goods. Column 4 reports the direct effect on purchasing power. The effect on purchasing power for renters (Panel A) is defined as Column 1 - Column 2 - Column 3. For homeowners (Panel B), the effect on purchasing power in Case A is defined as Column 1 - Column 3; in Case B, it is defined as Column 1 + Column 2 - Column 3. Columns 5 to 7 report indirect effects of TFP growth on earnings, housing costs, and the cost of non-housing non-tradable goods. Column 8 reports the indirect effect on purchasing power. Column 9 reports the total effect, defined as the sum of the direct effect and indirect effect. Columns 10 expresses the total effect as a percent increase relative to 1980 average earnings (in 2017 dollars). Column 11 expresses these numbers in annual terms, dividing column 10 by 20. Columns 12 and 13 report robustness to alternative assumptions on mobility: in Column 12, that migration flows from other sample cities are proportion to their population sizes; in Column 13, that migration flows are based on predicted migration flows only (taking the predicted values from regressing 1975-1980 migrant flows on log origin city size, log destination city size, log geographic distance, and log economic distance). Robust standard errors are reported in parentheses.

Table 9. Long-Run Direct Effects, Indirect Effects, and Combined Effects of Local TFP Growth by Worker Education Group

	Long-run Direct Effects on:				Long-run Indirect Effects on:								
	Earnings	Housing	Non-Tradables	Purchasing Power	Earnings	Housing	Non-Tradables	Purchasing Power	Total Effect	Total % Effect	Annual Total % Effect	Robustness: Annual Total % Effect	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. Workers with College Education													
Renters	3,173	1,223	856	1,094	2,030	-1,445	-1,012	4,487	5,581	10.9%	0.55%	0.65%	0.48%
	(1,311)	(692)	(485)	(136)	(509)	(401)	(281)	(822)	(830)	(1.6%)	(0.08%)	(0.20%)	(0.14%)
Homeowners													
Case A	4,514	-	1,219	3,294	3,542	-	-1,509	5,051	8,346	10.4%	0.52%	0.58%	0.53%
	(1,877)		(695)	(1,182)	(1,099)		(416)	(1,149)	(1,624)	(2.0%)	(0.10%)	(0.19%)	(0.16%)
Case B	4,514	1,742	1,219	5,036	3,542	-2,156	-1,509	2,896	7,932	9.9%	0.49%	0.53%	0.53%
	(1,877)	(993)	(695)	(2,175)	(1,099)	(594)	(416)	(1,305)	(2,504)	(3.1%)	(0.16%)	(0.16%)	(0.18%)
Panel B. Workers with High School Education or Less													
Renters	2,853	1,156	809	889	526	-948	-663	2,137	3,026	8.6%	0.43%	0.47%	0.38%
	(884)	(408)	(286)	(192)	(245)	(259)	(181)	(638)	(669)	(1.9%)	(0.10%)	(0.10%)	(0.08%)
Homeowners													
Case A	3,558	-	1,010	2,548	692	-	-803	1,495	4,043	8.8%	0.44%	0.45%	0.44%
	(1,108)		(359)	(749)	(321)		218	(502)	(912)	(2.0%)	(0.09%)	(0.10%)	(0.10%)
Case B	3,558	1,443	1,010	3,991	692	-1,146	-803	348	4,339	9.4%	0.47%	0.47%	0.48%
	(1,108)	(513)	(359)	(1,262)	(321)	(311)	(218)	(152)	(1,280)	(2.8%)	(0.14%)	(0.14%)	(0.14%)
Panel C. Average Impacts by Worker Education													
Workers with College Education				3,204				4,134	7,338	10.4%	0.52%	0.59%	0.51%
				(1,195)				(1,100)	(1,675)	(2.3%)	(0.12%)	(0.17%)	(0.15%)
Workers with High School Education				2,446				1,342	3,788	8.9%	0.45%	0.47%	0.43%
				(724)				(412)	(904)	(2.1%)	(0.11%)	(0.11%)	(0.10%)

Notes: Panels A and B report estimates similar to Table 8, but separately by worker education group. Panel C reports average impacts for each worker education group, weighting by the fraction of renters or homeowners (for homeowners, we take the average of Case A and Case B). Robust standard errors are reported in parentheses.

Table 10. Geographic Variation in Annualized Direct Effects and Indirect Effects on Purchasing Power of Renters

	Direct Effect (1)	Indirect Effect (2)	Total Effect (3)	Total % Effect (4)
Panel A: Top Tercile Direct Effect & Top Tercile Indirect Effect				
Group Average (N = 21)	252	256	508	1.4%
Examples:				
Binghamton, NY	237	180	417	1.2%
Charleston-N.Charleston,SC	431	262	693	2.2%
New Orleans, LA	245	162	408	1.1%
San Jose, CA	252	285	537	1.2%
Panel B: Top Tercile Direct Effect & Bottom Tercile Indirect Effect				
Group Average (N = 22)	220	59	279	0.8%
Examples:				
Chattanooga, TN/GA	194	83	277	0.8%
Decatur, IL	155	65	220	0.5%
Greenville-Spartanburg-Anderson SC	152	52	204	0.6%
Omaha, NE/IA	119	74	193	0.5%
Panel C: Bottom Tercile Direct Effect & Top Tercile Indirect Effect				
Group Average (N = 21)	-29	260	231	0.7%
Examples:				
Cleveland, OH	-6	173	167	0.4%
Lexington-Fayette, KY	-16	193	177	0.5%
Salt Lake City-Ogden, UT	17	160	177	0.5%
Trenton, NJ	-25	333	308	0.8%
Panel D: Bottom Tercile Direct Effect & Bottom Tercile Indirect Effect				
Group Average (N = 23)	-52	59	8	0.0%
Examples:				
Dallas-Fort Worth, TX	-15	55	40	0.1%
St. Louis, MO-IL	-50	71	21	0.1%
Tulsa, OK	-17	53	36	0.1%
Youngstown-Warren, OH-PA	15	82	97	0.2%

Notes: This table shows geographical differences in the long-run annualized effects of TFP growth on renters' purchasing power. All entries are in 2017 dollars. Column 1 shows the direct annualized effect of TFP growth on per-worker purchasing power, column 2 shows the indirect annualized effect, column 3 shows the total annualized effect (sum of direct and indirect effect), and column 4 shows the annual percent effect with respect to 1980 average earnings for renters in each city. Panel A shows example cities (out of a group of 21) that belong both to the top tercile of the distribution of direct effects and to the top tercile of the distribution of indirect effects from TFP growth. Panel B shows example cities (out of a group of 22) that belong both to the top tercile of the distribution of direct effects and to the bottom tercile of the distribution of indirect effects from TFP growth. Panel C shows example cities (out of a group of 21) that belong both to the bottom tercile of the distribution of direct effects and to the top tercile of the distribution of indirect effects from TFP growth. Panel D shows example cities (out of a group of 23) that belong both to the bottom tercile of the distribution of direct effects and to the bottom tercile of the distribution of indirect effects from TFP growth.

Table 11. Geographic Differences in the Long-run Annualized Impacts of Local TFP Growth on Purchasing Power

	Renters				Homeowners			
	Direct Effect	Indirect Effect	Total Effect	Total % Effect	Direct Effect	Indirect Effect	Total Effect	Total % Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Northeast								
New England	68	176	245	0.65%	167	148	315	0.60%
Middle Atlantic	128	136	264	0.65%	307	113	420	0.77%
Midwest								
East North Central	69	111	180	0.44%	171	100	271	0.48%
West North Central	29	71	100	0.27%	73	72	145	0.27%
South								
South Atlantic	122	204	326	0.93%	306	178	484	0.99%
East South Central	83	101	183	0.54%	204	98	302	0.64%
West South Central	47	70	117	0.32%	118	70	188	0.36%
West								
Mountain	23	120	143	0.41%	69	114	183	0.35%
Pacific	53	158	211	0.53%	135	129	264	0.46%

Notes: Entries are the long-run annualized effects of TFP growth on purchasing power, by Census region and division, for renters (columns 1 - 4) and homeowners (columns 5 - 8). All entries are in 2017 dollars. Columns 1 and 5 show the direct effect of TFP growth on per-worker purchasing power, columns 2 and 6 show the indirect effect, columns 3 and 7 show the total effect (sum of direct and indirect effect), and columns 4 and 8 shows the percent effect with respect to 1980 average earnings in each Census Division. The 9 rows correspond to US Census divisions, grouped by region.

Appendix A: Estimation of Total Factor Productivity

To measure total factor productivity we use confidential plant-level data from the Census of Manufacturers (CMF) in 1977, 1987, and 1997. We adopt an econometric approach similar to that used in our previous work based on the same data from the Census of Manufacturers (Greenstone, Hornbeck and Moretti, 2010). We assume each plant p in year t uses the following Cobb-Douglas technology:

$$S_{pt} = A_{pt} L_{pt}^{\beta_1} K_{pt}^{\beta_2} M_{pt}^{\beta_3}, \tag{4}$$

where S is total value of shipments minus changes in inventories, A is TFP, L is total labor input, K is book value of capital stock, and M is value of material inputs. An important issue is that worker quality is likely to differ across establishments in systematic ways. Failure to account for differences in worker quality would cause measured TFP to reflect differences in labor inputs. We define total labor input in plant p and year t as the weighted sum of hours worked by production workers (H_{pt}^P) and non-production workers (H_{pt}^{NP}), with non-production worker hours weighted by their relative hourly wage: $L_{pt} = H_{pt}^P + (w_{pt}^{NP}/w_{pt}^P)H_{pt}^{NP}$. This procedure assumes that the relative productivity of production and non-production workers is equal to their relative wage.[54] Capital values are defined as the average total book value of capital stock at the beginning and end of the year, plus the total value of rentals.[55] Material inputs are defined as the total value of materials purchased minus changes in inventories.[56]

Using the confidential plant-level data, we regress log output on log labor, log capital, log materials, and city fixed effects for each year separately. The regressions are weighted by plant output. The estimated 193 city fixed effects reflect average total factor productivity in each city and year, which also satisfy confidentiality restrictions on Census plant-level data.[57] To interpret the magnitudes, we normalize our estimates of nominal TFP changes to the average real change estimated in the NBER Productivity Database. This normalization of mean changes does not affect the coefficients estimated in our empirical specifications, but benchmarks the reported magnitudes associated with real TFP growth from 1980 to 1990.

There are well-known challenges in estimating total factor productivity. An important con-

[54] Note that the Census does not report hours of non-production workers, but this formula is equivalent to the standard approach using these data of measuring total labor input as production worker hours, multiplied by total wages paid to production workers and non-production workers, and divided by total wages paid to production workers. This formula assumes that these two types of workers are perfect substitutes, and to the extent that there is imperfect substitution between these worker types it will be another source of measurement error in TFP.

[55] We are unable to use the permanent inventory method because annual investment data are unavailable for all plants in the Census of Manufacturers.

[56] The real quantity of material inputs will be mis-measured if local TFP growth increases local prices of non-traded materials, which would understate local TFP growth, but the instrumented change in local TFP (discussed below) would not reflect local changes in prices.

[57] Due to Census Bureau confidentiality restrictions, city-year groups are omitted when they include a very small number of manufacturing firms or a very high degree of output concentration among a few firms.

cern is that establishments may adjust their input choices in response to unobserved shocks, causing bias in the estimated coefficients on inputs (see, e.g., Griliches and Mairesse (1995)). This has been a topic of considerable research, and three points are worth considering in this regard. First, we have explored potential sources of bias on these data and found limited evidence of significant bias in the production function β's (Greenstone, Hornbeck and Moretti, 2010). In particular, we found the production function coefficients to be consistent with cost-share methods of estimating TFP as well as other standard methods to deal with input endogeneity, including: controlling for flexible functions of investment, capital, materials, and labor; and instrumenting for current inputs with lagged changes in inputs (Olley and Pakes, 1996; Blundell and Bond, 1998; Levinsohn and Petrin, 2003; Syverson, 2004*a*,*b*; Van Biesebroeck, 2007; Ackerberg, Caves and Frazer, 2015).

Second, the main parameters of interest in our context are not the β's in the production function; rather, the parameters of interest are the effects of TFP on local labor market outcomes and local housing market outcomes, which we estimate using instrumental variables. This means that, in our context, any bias in the estimation of TFP stemming from endogenous input choices will only be a concern to the extent that this bias is systematically correlated with our instruments.[58]

Third, a substantial separate problem arises in that estimated changes in TFP are likely to contain substantial measurement error. This problem also motivates our use of instrumental variables.

[58]For example, while factor mobility may contribute to endogenous changes in input usage across cities due to productivity growth, our instrumental variables approach will estimate nationwide industry-level changes in TFP and assign these nationwide increases in TFP to particular cities according to their initial industry concentrations.

Appendix B: Theoretical Framework

This Appendix presents a simple spatial equilibrium model of the labor market and housing market, which is useful for considering both the direct effects of local TFP growth in that city and indirect effects on other cities. The goals are twofold. First, we aim to clarify what influences who benefits from local TFP growth. Local TFP growth increases local labor demand, which results in higher nominal wages and also higher cost of housing. The model clarifies how the local gains from TFP growth are split between workers and landowners. We show that incidence depends on relative elasticities, and which of the two factors (labor or housing) is supplied more elastically. The second goal is to clarify how a local shock to one city might indirectly affect other cities through worker mobility.

We adopt the standard assumptions of Rosen-Roback spatial equilibrium models, with specific functional form assumptions similar to those in Moretti (2011). For brevity, we focus on the simplest version of the model with intuitive closed-form solutions (see Moretti, 2011; Kline and Moretti, 2014, for extensions).

Setup

There are two cities, a and b. Each city is a competitive economy, producing a single output good Y that is traded on the international market at a fixed price normalized to 1. The production function in city c is: $\ln Y_c = A_c + (1-h)n_c$, where A_c is city-specific log TFP; n_c is the log of the share of employment in city c; and $0 < h < 1$. Workers are paid their marginal product, and labor demand is derived from the usual first order conditions.[59] We assume a fixed number of workers in the economy.

Indirect utility of worker i in city c is given by: $v_{ic} = w_c - \beta r_c + x_c + e_{ic}$, where w_c is the log of nominal wage, r_c is the log of cost of housing, x_c is the log value of amenities, and β measures the importance of housing consumption in utility and equals the budget share spent on housing. Since people do not spend their entire budget on housing, the effect of a 1% increase in rent is smaller than the effect from a 1% decrease in wage.

The random variable e_{ic} is an idiosyncratic location preference, for which a large draw of e_{ic} means that worker i particularly likes city c aside from real wages and amenities. We assume that worker i's relative preference for city b over city a ($e_{ib} - e_{ia}$) is distributed uniformly $U[-s, s]$. The assumption of a uniform distribution is analytically convenient, allowing us to derive closed-form expressions for the endogenous variables in equilibrium. The comparative statics are unchanged in an extended version of this model that assumes the e_{ic}'s are distributed according to a type I Extreme Value distribution.

Workers locate wherever utility is maximized. Worker i chooses city b, rather than city a, if and only if the strength of location preferences exceeds any real wage premium and higher

[59] We abstract from labor supply decisions and assume each worker supplies one unit of labor.

amenity value: $e_{ib} - e_{ia} > (w_a - \beta r_a) - (w_b - \beta r_b) + (x_a - x_b)$. In equilibrium, there is a marginal worker who is indifferent between city a and b.

The parameter s governs the strength of idiosyncratic preferences for location and, therefore, the degree of labor mobility and the city's elasticity of local labor supply. If s is large, many workers will require large differences in real wages or amenities to be compelled to move, and the local labor supply curve is less elastic. If s is small, most workers are not particularly attached to one city and will be willing to move in response to small differences in real wages or amenities, and cities face a more elastic local labor supply curve. In the extreme case where s is zero, there are no idiosyncratic preferences for location and there is perfect labor mobility. In this case, workers will arbitrage any differences in real wages adjusted for amenities and local labor supply is infinitely elastic.

We characterize the elasticity of housing supply by assuming the log price of housing is governed by: $r_c = k_c n_c$. This is a reduced-form relationship between the log cost of housing and the log number of residents in city c.[60] The parameter k_c reflects differences in the elasticity of housing supply, which varies across cities due to differences in geographic constraints and local regulations on land development (Glaeser and Gyourko, 2005; Glaeser, Gyourko and Saks, 2006; Gyourko, 2009; Saiz, 2010). In cities where the geography and regulatory structure make it relatively easy to build new housing, k_c is relatively smaller. In the extreme case where there are no constraints on building housing, k_c is zero and the supply curve is horizontal. In the extreme case where it is impossible to build new housing, k_c is infinite and the supply curve is vertical.[61]

Direct Effects of Local TFP Growth

We now explore how local productivity growth in city b directly affects equilibrium wages, housing rents, and employment in that city. We assume the two cities are initially identical and that TFP increases in city b by an amount Δ. If A_{b1} is initial TFP, the TFP gain is $A_{b2} - A_{b1} = \Delta$. TFP in city a does not change.

Increased productivity in city b shifts the local labor demand curve to the right, resulting in higher employment and higher nominal wages. Higher employment leads to higher housing costs. Assuming an interior solution, the changes in equilibrium employment, nominal wage, and housing rent in city b are:

$$n_{b2} - n_{b1} = \frac{1}{\beta(k_a + k_b) + 2h + s}\Delta > 0, \tag{5}$$

[60] The model assumes that housing is of constant quality, such that housing supply costs increase only with the number of residents. Our focus is on changes in real housing costs, holding quality fixed, and in the empirical analysis we also present estimates that control for potential changes in housing quality.

[61] For simplicity, we are ignoring durability of the housing stock and the asymmetry between positive and negative shocks uncovered by Glaeser and Gyourko (2005).

$$w_{b2} - w_{b1} = \frac{\beta(k_a + k_b) + h + s}{\beta(k_a + k_b) + 2h + s}\Delta > 0, \tag{6}$$

$$r_{b2} - r_{b1} = \frac{k_b}{\beta(k_a + k_b) + 2h + s}\Delta > 0. \tag{7}$$

The magnitudes of these effects depend on the elasticities of labor supply and housing supply. Employment increases more when the elasticity of labor supply is higher (s is smaller) and the elasticity of housing supply in b is higher (k_b is smaller). A smaller s means workers have less idiosyncratic preference for locations, so workers are more mobile in response to differences in wages. A smaller k_b means that city b can add more housing units to accommodate in-migration with less increase in housing cost. Nominal wages increase more when the elasticity of labor supply is lower (s is larger), and housing costs increase more when the elasticity of housing supply in b is lower (k_b is larger).[62]

The increase in real wages, or purchasing power, in city b reflects the increase in nominal wage minus the budget-share weighted increase in housing cost:

$$(w_{b2} - w_{b1}) - \beta(r_{b2} - r_{b1}) = \frac{\beta k_a + h + s}{\beta(k_a + k_b) + 2h + s}\Delta > 0. \tag{8}$$

Equation 8 shows how the benefits from productivity growth are split between workers and landowners, with the relative incidence depending on which of the two factors (labor or land) is supplied more elastically at the local level. Intuitively, inelastically supplied factors should bear more incidence.

For a given elasticity of housing supply, a lower local elasticity of labor supply (larger s) implies that a larger fraction of the productivity shock in city b accrues to workers in city b and that a smaller fraction accrues to landowners in city b. When workers are less mobile, they capture more of the economic gains from local productivity growth. In the extreme case, if labor is completely immobile ($s = \infty$), then equation 8 becomes: $(w_{b2} - w_{b1}) - \beta(r_{b2} - r_{b1}) = \Delta$. The real wage (or purchasing power) in city b then increases by the full amount of the productivity shock, such that the benefit of the shock accrues entirely to workers in city b. That is, when labor is a fixed factor, workers in the city directly impacted by the TFP shock will capture the full economic gain generated by the shock.

For a given elasticity of labor supply, a lower elasticity of housing supply in city b (larger k_b)

[62] To obtain equations 5, 6, and 7, we equate local labor demand to local labor supply in each city and equate local housing demand to local housing supply in each city. From the spatial equilibrium condition, the (inverse of) the *local* labor supply to city b in period t is: $w_{bt} = w_{at} + \beta(r_{bt} - r_{at}) + (x_{at} - x_{bt}) + 2s(N_{bt} - 1)$, where N_{bt} is the share of employment in city b. Since N_{bt} is in levels, rather than logs, to obtain closed-form solutions in equations 5, 6, and 7, we use a linear approximation around 1/2: $n_{bt} = \ln N_{bt} \approx \ln(1/2) + 2N_{bt} - 1$, so that we can assign $N_{bt} \approx (1/2)(n_{bt} - \ln(1/2) + 1)$ in the above equation for the (inverse of) the *local* labor supply to city b in period t. We approximate around 1/2 because of the assumption that the two cities are initially identical, which implies that their employment share is initially 1/2. We assume that local housing demand is proportional to city population.

implies more of the productivity shock in city b accrues to landowners in city b and less accrues to workers in city b. When housing supply is more inelastic, the quantity of housing increases less in city b and housing prices increase more following the local productivity shock. In the extreme case, if housing supply in city b is fixed ($k_b = \infty$), the entire productivity increase is capitalized into land values in city b and worker purchasing power is unchanged.

Motivated by equations 5 to 8, the empirical analysis explores who benefits from local TFP shocks. The model has assumed that workers are renters, though in the empirical estimates we also allow for some workers to be homeowners. The model has also assumed that people consume only housing and a traded good with fixed price. In our analysis of real wages, or purchasing power, we will also allow for the consumption of non-housing non-traded goods whose prices vary across cities.

Indirect Effects of Local TFP Growth

We now consider indirect effects on city a from TFP growth in city b. While city a does not experience any *direct* effect, city a receives *indirect* effects from the TFP shock in city b. Labor mobility is the mechanism through which city a is indirectly affected by the TFP shock in city b.

In particular, TFP growth in city b causes some workers to leave city a for city b. As workers leave, city a experiences an increase in equilibrium wage and a decrease in equilibrium rent. The wage increases in city a because labor demand is downward sloping; the rent decreases in city a because housing supply is upward sloping. This process continues until spatial equilibrium is restored, and the marginal worker is indifferent between city a and city b.[63]

In equilibrium, real wages increase in city a by:

$$(w_{a2} - w_{a1}) - \beta(r_{a2} - r_{a1}) = \frac{\beta k_a + h}{\beta(k_a + k_b) + 2h + s}\Delta > 0. \tag{9}$$

Thus, real wages increase in city a despite TFP being unchanged in city a. Comparing equations 9 and 8, the increase in city a is smaller than the increase in city b. Real wages increase more in city b, which is the city directly hit by the TFP shock. Only in the special case of perfect labor mobility, i.e., in the absence of location preferences ($s = 0$), would the increase in real wages be the same in city a and city b.

In this model, with only two cities, the indirect effects on city a are concentrated and large. In our data, however, migrants to city b have many possible origins and the indirect effects on each other city are diffused and small. Though the indirect effects on each other city are small, their sum across all cities is potentially large.

[63]The decrease in employment in city a is equal to the increase in city b, since we have assumed that there is a fixed number of workers in the economy and city a and city b are initially of the same size. We rule out international migration, estimating incidence within the United States, though in principle these cities could be in different countries.

Appendix C: Measuring Changes in Local Purchasing Power

An increase in local TFP increases both local labor demand and local housing demand, which raises earnings and cost of living. We are interested in quantifying the net effect on worker "purchasing power" in a city, defined as the increase in local earnings net of the increase in local cost of living. This Appendix motivates and derives our measurement of changes in purchasing power.

Renters. For renters, the calculation of changes in "purchasing power" is conceptually straightforward: it is the percent change in earnings, minus the properly-weighted percent change in housing rent, minus the properly-weighted percent change in cost of non-housing non-tradable goods.

Consider a worker who consumes a traded good (T), housing (H), and a non-housing non-traded good (NT). The price of T is fixed nationally, and is therefore independent of local demand and supply. The rental price of housing (p_H) and the price of the non-housing non-tradable good (p_{NT}) are set locally. We assume Cobb-Douglas utility with fixed consumption shares ($\beta_T + \beta_H + \beta_{NT} = 1$):

$$U = T^{\beta_T} H^{\beta_H} N^{\beta_{NT}}, \tag{10}$$

which implies that worker indirect utility is:

$$\ln V = \ln w - \beta_T \ln p_T - \beta_H \ln p_H - \beta_{NT} \ln p_{NT}. \tag{11}$$

The increase in local purchasing power of renters, from an increase in local TFP, is then given by:

$$\Delta \ln V = \Delta \ln w - \beta_H \Delta \ln p_H - \beta_{NT} \Delta \ln p_{NT}. \tag{12}$$

This definition reflects the percent increase in earnings minus the properly-weighted percent increase in housing rent and cost of non-housing non-tradables. The weights correspond to the share of total expenditures that is spent on housing and non-housing non-tradables, respectively. Intuitively, if housing expenditures make up roughly 33% of total expenditures (U.S. Bureau of Labor Statistics, 2000), then a 1% increase in housing rent would reduce purchasing power by 0.33%.

This is the definition of changes in "real wages" used by Moretti (2013). Note that this definition is based on how the BLS measures the official CPI. The official CPI is the weighted average of the price changes of each good, with weights that correspond to the share of total expenditures spent on that good. The key difference is that, unlike the official CPI that measures average price changes for the entire country, our measure varies at the local level.

We estimate the impact of local TFP increases on local earnings and the local rental price

of housing, but the important data limitation is that changes in local prices of non-housing non-tradable goods are not available for most cities in our period. To overcome this limitation, we follow the approach adopted by Moretti (2013) to impute the systematic component of p_N that varies with housing prices.

Moretti (2013) uses a local consumer price index, released by the BLS for 23 large cities (U.S. Bureau of Labor Statistics, 2000), to estimate the relationship between local prices of non-housing goods and the local cost of housing. This local CPI is normalized to 1 in a given year, which precludes cross-sectional comparisons, but it can be used to infer how local non-housing prices increase along with increases in the cost of housing. Moretti estimates that, from 1980 to 2000, a 1% increase in the local rental price of housing is associated with a 0.35% increase in the local prices of all non-housing goods. Moretti uses this estimate to predict changes in the prices of non-tradable goods, as a function of changes in housing costs, in those cities for which the BLS does not report a local CPI. Moretti (2013) also uses data on non-housing prices from the Accra dataset, collected by the Council for Community and Economic Research, and shows that the imputed local prices are highly correlated with the local CPI based on the Accra data.

Using the above notation, the estimates from Moretti (2013) imply that:

$$\frac{\beta_T}{\beta_T + \beta_{NT}} \times \Delta \ln p_T + \frac{\beta_{NT}}{\beta_T + \beta_{NT}} \times \Delta \ln p_{NT} = 0.35 \times \Delta \ln p_H. \tag{13}$$

Given this relationship between prices, and a housing share of total expenditures equal to 0.33 (U.S. Bureau of Labor Statistics, 2000), we calculate that:

$$\beta_{NT} \Delta \ln p_{NT} = 0.35 \times (1 - \beta_H) \times \Delta \ln p_H = 0.23 \times \Delta \ln p_H. \tag{14}$$

This equation captures how the properly-weighted change in cost of non-housing non-traded goods varies with the estimated change in housing rents. Inserting this into equation 12, we calculate the estimated impact on renters' purchasing power as the estimated increase in log earnings minus 0.56 times the impact on log rent, where 0.56 includes both increases in housing cost (0.33) and increases in cost of non-housing non-tradable goods (0.23).

Homeowners. For homeowners, the calculation of changes in "purchasing power" is more complicated conceptually. We focus on homeowners who purchased their home prior to the TFP shock and the associated increase in housing prices, whereas a homeowner who purchased their home after the TFP shock is affected similarly as the renter discussed above. Following an increase in local TFP, the homeowner receives an equity gain and an increase in the user cost of housing. The total impact on homeowner purchasing power is difficult to characterize exactly because it depends on particular homeowner characteristics, such as their expected lifespan and prospects of moving. Instead, we consider two bounds on the changes in homeowners' purchasing power.

As one extreme case (Case A), we consider an infinitely-lived and immobile homeowner. This homeowner does not move after the TFP shock, and is infinitely-lived in the sense that the homeowner plans to pass on the home to heirs that will continue to live in that city. The homeowner receives an increase in home value, which generates income equal to the increased annual rental return on the home, but the homeowner pays an equivalently higher opportunity cost for living in the home. The homeowner's purchasing power is effectively insulated from increases in local housing costs, though the homeowner does face increased local prices for other non-housing goods. In this Case A, the homeowner's change in purchasing power is defined as:

$$\Delta \ln V = \Delta \ln w - \beta_{NT} \Delta \ln p_{NT}. \tag{15}$$

As above, for renters, we calculate the properly-weighted increase in cost of non-housing non-traded goods. We then calculate the estimated impact on homeowner's purchasing power (Case A) as the estimated increase in log earnings minus 0.23 times the impact on log rent, which reflects the increase in cost of non-housing non-tradable goods.

As another extreme case (Case B), we consider a homeowner who is able to consume the income stream associated with the increase in home value. This homeowner anticipates moving to another city, or leaving a bequest to heirs that will live in another city, whose housing prices have not increased. This Case B assumes that homeowners can consume in perpetuity the annual return associated with increased housing rents in their city, which increases their earnings by the percent increase in housing rents multiplied by the expenditure share on housing. That is, homeowners can consume the increase in housing rents that would have been faced by renters of their home.[64] The homeowner still faces increased local prices for other non-housing goods. In this Case B, the homeowner's change in purchasing power is defined as:

$$\Delta \ln V = \Delta \ln(w) + \beta_H \Delta \ln p_H - \beta_{NT} \Delta \ln p_{NT}. \tag{16}$$

In practice, we then calculate the estimated impact on homeowners' purchasing power (Case B) as the estimated increase in log earnings plus 0.10 times the impact on log rent (where 0.10 = 0.33 - 0.23), which includes both income received from housing rents (0.33) and an increase in cost of non-housing non-tradable goods (0.23).

Note that we consider impacts on the purchasing power of workers, renters or homeowners, who do not own other assets. Some workers may be shareholders in firms whose profits increase with productivity growth, or some workers may be invested in real estate in cities whose housing rents increase with local productivity.

[64] Because homeowners' annual housing rents are unobserved, we assume homeowners and renters in the same city spend the same share of consumption on housing.

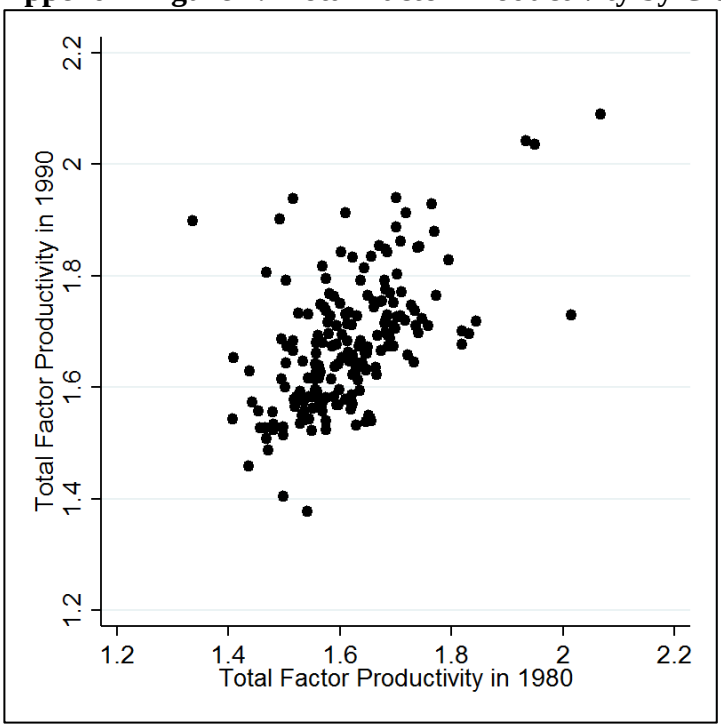
In summary, we consider changes in "purchasing power" following an increase in local TFP that both increases earnings and local cost of living. Renters and homeowners both face the same increased cost of non-housing non-tradable goods, but changes in housing costs have different effects on renters and homeowners:

1. Renters must pay increased housing costs, equal to the estimated increase in local rents. Their change in purchasing power, including increased costs for housing and other non-tradables, is defined in equation 12.

2. Homeowners (Case A) are insulated from increases in local housing costs, but must pay higher prices for non-housing non-traded goods. Their change in purchasing power is defined in equation 15.

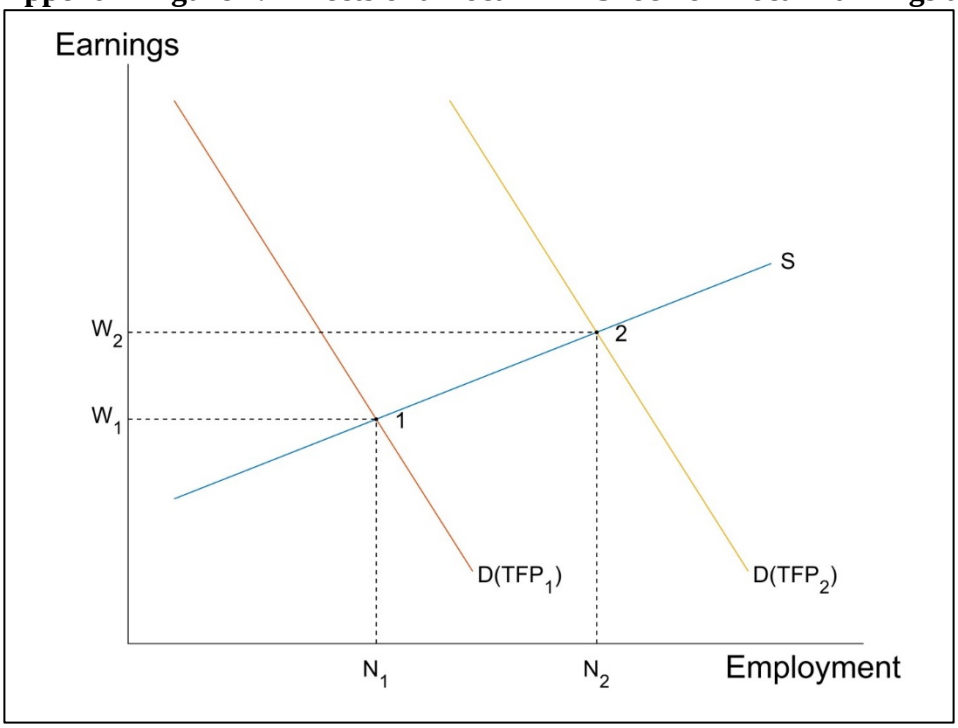
3. Homeowners (Case B) are insulated from increases in local housing costs, and receive even greater benefits from increases in the value of their home. In this extreme case, they can consume the annual rental return associated with the increased home value, but must pay higher prices for non-housing non-traded goods. Their change in purchasing power is defined in equation 16.

Appendix Figure 1. Total Factor Productivity by City, 1980 and 1990



Notes: For each city (MSA), the figure plots TFP in 1990 against TFP in 1980. The estimated coefficient is 0.610, with a standard error 0.099, and an R-squared of 0.298.

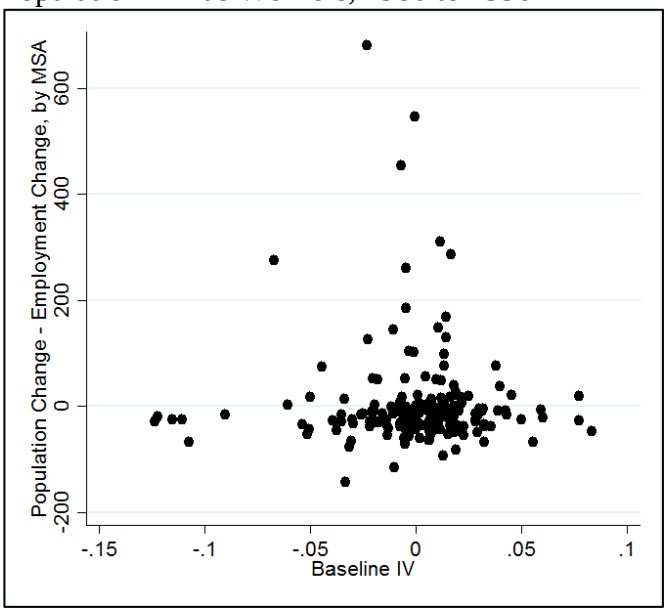
Appendix Figure 2. Effects of a Local TFP Shock on Local Earnings and Local Employment



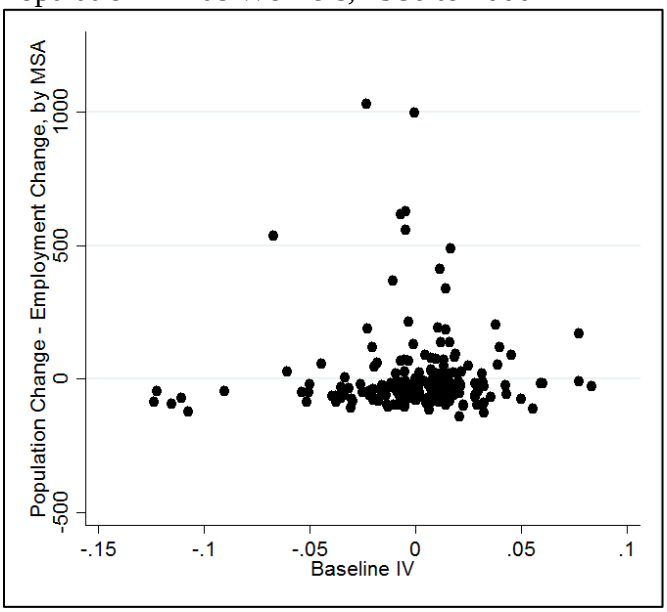
Notes: S is local labor supply and D(TFP) is local labor demand as a function of TFP. Point 1 represents the equilibrium before the TFP shock. The TFP shock shifts the demand curve to the right, $D(TFP_2)$. The new equilibrium is point 2.

Appendix Figure 3. Local TFP Growth and Changes in Working-Age Population and Workers

Panel A. City-level Changes in Working-Age Population Minus Workers, 1980 to 1990



Panel B. City-level Changes in Working-Age Population Minus Workers, 1980 to 2000



Notes: Each Panel shows city-level changes in the working-age population (ages 19 to 65) minus the number of workers (in thousands), plotted against the city-level predicted change in TFP (based on our baseline instrument). In Panel A, the estimated coefficient is -48.78 with a standard error of 164.91. In Panel B, the estimated coefficient is 104.54 with a standard error of 298.74.

Appendix Table 1. City Characteristics in 1980 and Average Changes Over Time

	City Mean in:	Log Change in City Mean from:	
	1980	1980 to 1990	1980 to 2000
MSA Characteristic:	(1)	(2)	(3)
Employment	174,361	0.105	0.300
	[355,906]	[0.188]	[0.241]
Employment, College	31,725	0.321	0.668
	[74,496]	[0.211]	[0.266]
Employment, Some College	36,297	0.557	0.492
	[74,509]	[0.170]	[0.244]
Employment, High School or less	106,338	-0.193	0.081
	[209,462]	[0.198]	[0.261]
Employment, Manufacturing Sector	57,906	-0.096	-0.061
	[120,535]	[0.237]	[0.300]
Employment, Non-Manufacturing	116,455	0.211	0.467
	[240,047]	[0.168]	[0.217]
Annual Earnings	45,824	0.083	0.186
	[5,349]	[0.074]	[0.108]
Annual Earnings, College	65,848	0.145	0.277
	[7,114]	[0.059]	[0.091]
Annual Earnings, Some College	46,093	0.036	0.112
	[4,763]	[0.070]	[0.081]
Annual Earnings, High School or less	40,792	-0.032	0.017
	[4,850]	[0.070]	[0.076]
Annual Housing Rent	9,730	0.153	0.154
	[1,272]	[0.127]	[0.118]
Home Value	166,071	0.101	0.208
	[51,886]	[0.269]	[0.190]
Number of Housing Units	137,291	0.063	0.259
	[276,743]	[0.179]	[0.237]
Homeowners	117,700	0.075	0.335
	[211,976]	[0.191]	[0.248]
Renters	56,660	0.176	0.288
	[150,510]	[0.200]	[0.249]
Total Factor Productivity	1.649	0.053	0.110
	[0.088]	[0.074]	[0.122]
Number of MSAs	193	193	193

Notes: Column 1 reports average city (MSA) characteristics in 1980. Column 2 reports the average change (in logs) in city characteristics from 1980 to 1990 and Column 3 reports the average change (in logs) from 1980 to 2000, weighted by city manufacturing output in 1980. Dollar values are reported in 2017 US dollars (CPI). Education groups are defined as: "College" includes workers who have completed 4 or more years of college, "Some College" includes workers who completed between 1 and 3 years of college, "High School or less" includes workers who completed 12 years of education or fewer. Standard deviations are reported in brackets.

Appendix Table 2. OLS Impacts of TFP Growth on Employment, Earnings, and Housing Costs

	Cross-section, 1980 and 1990	Change from 1980 to 1990	Change from 1980 to 2000	Change from 1980 to 2010
	(1)	(2)	(3)	(4)
Panel A. Log Employment	3.59***	0.05	0.15	-0.04
	(1.06)	(0.17)	(0.23)	(0.27)
Panel B. Log Earnings	0.33***	0.14*	0.29**	0.28**
	(0.07)	(0.07)	(0.12)	(0.14)
Panel C. Log Cost of Rent	0.54***	0.25**	0.42***	0.34**
	(0.11)	(0.12)	(0.14)	(0.14)
Panel D. Log Home Value	1.10***	0.48**	0.68***	0.72***
	(0.24)	(0.23)	(0.20)	(0.25)
Panel E. Log Purchasing Power				
Renters	0.03	0.01	0.06	0.09
	(0.04)	(0.04)	(0.06)	(0.10)
Homeowners (Case A)	0.21***	0.08	0.19**	0.20*
	(0.05)	(0.05)	(0.09)	(0.12)
Homeowners (Case B)	0.39***	0.16**	0.33**	0.32**
	(0.08)	(0.08)	(0.13)	(0.15)

Notes: The reported estimates are from OLS specifications. Column 1 reports estimates from a pooled cross-section: the indicated city characteristic from each panel is regressed on city total factor productivity (TFP) in 1980 and 1990, controlling for region-by-year fixed effects and weighting each city by its total manufacturing output. Columns 2, 3, and 4 report OLS estimates that correspond to the IV estimates in Table 2. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 3. Direct Effect of Local TFP Growth on Local Housing Costs, by City Elasticity of Housing Supply

	Medium-run Effect: Change from 1980 to 1990		Difference:	Long-run Effect: Change from 1980 to 2000		Difference:
	Below Mean Housing Elasticity	Above Mean Housing Elasticity	(2) - (1)	Below Mean Housing Elasticity	Above Mean Housing Elasticity	(5) - (4)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Log Cost of Rent	1.131*	0.641	-0.490	2.335**	1.195***	-1.140
	(0.613)	(0.410)	(0.738)	(1.095)	(0.441)	(1.181)
Panel B. Log Home Value	1.809*	1.490**	-0.319	3.373*	2.168***	-1.205
	(0.993)	(0.735)	(1.236)	(1.723)	(0.638)	(1.838)

Notes: The reported estimates correspond to those in Table 2, but estimated separately for cities with below-mean housing elasticity (Columns 1 and 4) and above-mean housing elasticity (Columns 2 and 5). Columns 3 and 6 report the difference in the estimated coefficients. The regressions include the 171 cities for which Saiz (2010) reports housing supply elasticities. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 4. Direct Effect of Local TFP Growth, Alternative Specifications

	Outcome Change from 1980 to 2000:					
	Long-run Effect: TFP Change from 1980 to 1990		Medium-Run Effect: TFP Change from 1980 to 2000	Long-run Effect: TFP Change from 1980 to 1990		
				Control: Instrumented TFP Change from 1980 to 1990 in Nearby MSAs		
	Control: TFP Change from 1990 to 2000	Control: Instrumented TFP Change from 1990 to 2000		Within 500 Miles	Within 250 Miles	Within 100 Miles
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Log Employment	3.73***	4.36**	1.79***	4.04***	4.53***	3.66***
	(1.09)	(1.80)	(0.61)	(1.26)	(1.70)	(1.35)
Panel B. Log Earnings	1.31***	1.06*	0.75***	1.36***	1.27**	1.11**
	(0.40)	(0.60)	(0.28)	(0.43)	(0.55)	(0.48)
Panel C. Log Cost of Rent	1.39***	1.09	0.79**	1.43***	1.14**	1.04**
	(0.42)	(0.68)	(0.32)	(0.44)	(0.49)	(0.44)
Panel D. Log Home Value	2.21***	2.48**	1.14**	2.02***	1.92**	1.95***
	(0.70)	(1.17)	(0.47)	(0.67)	(0.82)	(0.71)
Panel E. Log Purchasing Power						
Renters	0.53**	0.45	0.31**	0.56**	0.63**	0.52*
	(0.22)	(0.30)	(0.13)	(0.24)	(0.32)	(0.28)
Homeowners (Case A)	0.99***	0.81*	0.57***	1.03***	1.01**	0.87**
	(0.32)	(0.46)	(0.21)	(0.35)	(0.45)	(0.39)
Homeowners (Case B)	1.45***	1.16*	0.83***	1.51***	1.38**	1.21**
	(0.44)	(0.66)	(0.31)	(0.47)	(0.59)	(0.52)
First Stage Coefficient	0.89***	0.76***	0.88***	0.85***	0.70***	0.84***
(See Table Notes)	(0.17)	(0.19)	(0.20)	(0.19)	(0.19)	(0.19)
Instrument F-statistic	26.06	11.96	19.68	21.26	13.18	18.65

Notes: Column 1 reports estimates corresponding to those in Column 2 of Table 2, but controlling for the change in TFP from 1990 to 2000. Column 2 reports estimates from the same specification, but instrumenting for the change in TFP from 1990 to 2000 with the predicted change in TFP from 1990 to 2000 constructed as in our baseline instrument. Column 3 reports estimates from a long-difference specification, regressing changes in each outcome on changes in TFP from 1980 to 2000, and instrumenting using the predicted change in TFP from 1980 to 2000 constructed as in our baseline instrument. Columns 4, 5, and 6 report estimates corresponding to those in Column 2 of Table 2, but controlling for average changes in TFP from 1980 to 1990 in cities within 500 miles, 250 miles, or 100 miles. TFP changes in nearby cities are instrumented using the predicted change in TFP for those cities, constructed as in our baseline instrument. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 5. Direct Effect of Local TFP Growth, Additional Control Variables

	Medium-run Effect: Change from 1980 to 1990				Long-run Effect: Change from 1980 to 2000			
	Control for 1980 MFG Share	Controls for Broad Industry Shares	Control for 1980 O&G Share	Controls for Changes in Composition	Control for 1980 MFG Share	Controls for Broad Industry Shares	Control for 1980 O&G Share	Controls for Changes in Composition
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Log Employment	2.30***	1.85**	2.43**	-	4.01***	3.61***	4.39***	-
	(0.74)	(0.77)	(1.04)		(1.12)	(1.17)	(1.64)	
Panel B. Log Earnings	0.87***	0.67**	0.84**	0.89***	1.39***	1.22***	1.44**	1.12***
	(0.29)	(0.26)	(0.40)	(0.29)	(0.40)	(0.41)	(0.62)	(0.32)
Panel C. Log Cost of Rent	0.95**	0.46	0.51	1.17**	1.43***	1.16***	1.15**	1.61***
	(0.42)	(0.30)	(0.41)	(0.52)	(0.43)	(0.42)	(0.53)	(0.49)
Panel D. Log Home Value	1.69**	0.79	0.94	1.85**	2.42***	1.70**	1.53*	2.49***
	(0.70)	(0.56)	(0.75)	(0.74)	(0.76)	(0.69)	(0.81)	(0.81)
Panel E. Log Purchasing Power								
Renters	0.34**	0.41**	0.55**	0.31*	0.59***	0.57***	0.79**	0.46***
	(0.15)	(0.17)	(0.25)	(0.17)	(0.22)	(0.22)	(0.36)	(0.17)
Homeowners (Case A)	0.65***	0.56***	0.72**	0.69***	1.06***	0.96***	1.17**	0.95***
	(0.21)	(0.22)	(0.33)	(0.23)	(0.32)	(0.32)	(0.51)	(0.26)
Homeowners (Case B)	0.97***	0.71**	0.89**	1.07***	1.53***	1.34***	1.55**	1.44***
	(0.32)	(0.29)	(0.44)	(0.37)	(0.44)	(0.44)	(0.67)	(0.39)
First Stage Coefficient	0.81***	0.86***	0.79***	0.81***	0.81***	0.86***	0.79***	0.81***
(See Table Notes)	(0.16)	(0.19)	(0.21)	(0.16)	(0.16)	(0.19)	(0.21)	(0.16)
Instrument F-statistic	25.21	20.48	13.70	24.21	25.21	20.48	13.70	24.21

Notes: The estimates correspond to those in Table 2, with additional control variables. Columns 1 and 5 control for the city manufacturing employment share in 1980. Columns 2 and 6 control for the city employment share in 1980 in broad industry categories: Agriculture, Forestry, and Fishing; Mining; Construction and Manufacturing; Transportation and Public Utilities; Wholesale Trade and Retail Trade; and Finance, Insurance, and Real Estate and Services. Columns 3 and 7 control for the city employment share in 1980 in the oil and gas industry. Columns 4 and 8, in Panel B, are individual-level regressions that adjust annual earnings for worker composition by controlling for age, age squared, education (high school, some college, college), race, and gender (and cluster standard errors at the city level). Columns 4 and 8, in Panels C and D, are also individual-level regressions that adjust housing costs for physical characteristics by controlling for the number of rooms and number of bedrooms (dummy variables for each number), whether the home is part of a multi-unit structure, and the presence of a kitchen or plumbing (and cluster standard errors at the city level). Columns 4 and 8, Panel E, include both sets of individual-level controls. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 6. OLS Impacts of TFP Growth, by Education Level

	Pooled Cross-Section:			Medium-run Effect: Change from 1980 to 1990			Long-run Effect: Change from 1980 to 2000		
	College	Some College	No College	College	Some College	No College	College	Some College	No College
	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Log Employment	4.72***	3.90***	3.24***	-0.05	0.03	0.11	0.26	0.18	0.02
	(1.13)	(1.03)	(1.04)	(0.21)	(0.18)	(0.18)	(0.27)	(0.28)	(0.25)
Panel B. Log Earnings	0.29***	0.25***	0.12**	0.13**	0.14**	0.15**	0.24**	0.20**	0.18**
	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.09)	(0.08)	(0.08)
Panel C. Log Cost of Rent	0.56***	0.54***	0.51***	0.17	0.27**	0.25**	0.37**	0.37***	0.37***
	(0.13)	(0.11)	(0.10)	(0.14)	(0.11)	(0.12)	(0.16)	(0.12)	(0.12)
Panel D. Log Home Value	0.87***	0.94***	1.04***	0.47**	0.53**	0.52**	0.63***	0.65***	0.68***
	(0.22)	(0.22)	(0.24)	(0.19)	(0.23)	(0.25)	(0.17)	(0.18)	(0.20)
Panel E. Log Purchasing Power									
Renters	-0.03	-0.05	-0.16***	0.03	-0.01	0.01	0.03	-0.01	-0.03
	(0.05)	(0.04)	(0.05)	(0.06)	(0.04)	(0.06)	(0.06)	(0.05)	(0.06)
Homeowners (Case A)	0.16***	0.13***	0.01	0.09**	0.08	0.09*	0.15**	0.12*	0.10
	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)	(0.06)	(0.06)
Homeowners (Case B)	0.34***	0.31***	0.17***	0.14**	0.17**	0.17**	0.27**	0.24**	0.22***
	(0.07)	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)	(0.11)	(0.09)	(0.08)

Notes: The reported estimates are analogous to those in Table 5, but correspond to the OLS specifications (as in Appendix Table 2). Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 7. OLS Impacts of TFP Growth on Earnings Inequality

	Cross-section, 1980 and 1990 (1)	Change from 1980 to 1990 (2)	Change from 1980 to 2000 (3)
Panel A. 90/10 Centile Difference in Log Earnings	0.155***	-0.032	0.070
	(0.054)	(0.067)	(0.132)
Panel B. 90/50 Centile Difference in Log Earnings	0.144***	-0.075*	-0.099
	(0.047)	(0.044)	(0.061)
Panel C. 50/10 Centile Difference in Log Earnings	0.011	0.043	0.169
	(0.043)	(0.059)	(0.103)

Notes: The reported estimates are analogous to those in Table 6, but correspond to the OLS specifications (as in Appendix Table 2). Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 8. Direct Effects of Local TFP Growth, by Sector

	Medium-run Effect: Change from 1980 to 1990		Long-run Effect: Change from 1980 to 2000	
	Manufacturing	Non-Manufacturing	Manufacturing	Non-Manufacturing
	(1)	(2)	(3)	(4)
Panel A. Log Employment	2.61***	2.17***	3.75***	4.13***
	(0.95)	(0.70)	(1.26)	(1.17)
Panel B. Implied Multiplier	1.62***		2.21***	
	(0.25)		(0.32)	
Panel C. Log Earnings	0.74**	0.83***	0.88**	1.45***
	(0.30)	(0.29)	(0.38)	(0.46)

Notes: In Panel A, columns 1 and 2 report estimates that correspond to those in column 1 of Table 2, but separately for the manufacturing sector (column 1) and non-manufacturing sectors (column 2). Columns 3 and 4 report analogous estimates for the long-run effect by sector, corresponding to the estimates in column 2 of Table 2. Panel B reports the implied multiplier effect: the number of additional jobs in non-manufacturing sectors associated with a increase of one job in the manufacturing sector. Panel C reports estimated impacts on log earnings, as in Table 2, but separately for the manufacturing sector and non-manufacturing sector. Robust standard errors are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Appendix Table 9. Geographic Variation in Annualized Direct Effects and Indirect Effects on Purchasing Power of Homeowners

	Direct Effect (1)	Indirect Effect (2)	Total Effect (3)	Total % Effect (4)
Panel A: Top Tercile Direct Effect & Top Tercile Indirect Effect				
Group Average (N = 19)	538	232	770	1.5%
Examples:				
Binghamton, NY	582	175	757	1.6%
Charleston-N.Charleston,SC	1,021	213	1,234	2.9%
San Jose, CA	649	231	880	1.4%
Wilmington, DE/NJ/MD	433	263	696	1.2%
Panel B: Top Tercile Direct Effect & Bottom Tercile Indirect Effect				
Group Average (N = 23)	529	61	591	1.2%
Examples:				
Chattanooga, TN/GA	472	79	550	1.2%
Decatur, IL	357	69	425	0.8%
Greenville-Spartanburg-Anderson SC	350	49	399	1.0%
Wichita, KS	473	66	539	1.1%
Panel C: Bottom Tercile Direct Effect & Top Tercile Indirect Effect				
Group Average (N = 18)	-83	247	164	0.3%
Examples:				
Bridgeport, CT	20	179	199	0.3%
Lexington-Fayette, KY	-43	212	169	0.3%
Santa Cruz, CA	46	340	386	0.7%
Trenton, NJ	-62	319	258	0.5%
Panel D: Bottom Tercile Direct Effect & Bottom Tercile Indirect Effect				
Group Average (N = 25)	-137	60	-77	-0.2%
Examples:				
Dallas-Fort Worth, TX	-38	56	18	0.0%
Grand Rapids, MI	4	59	63	0.1%
St. Louis, MO-IL	-124	70	-54	-0.1%
Tulsa, OK	-44	59	15	0.0%

Notes: The reported estimates correspond to those in Table 10, but for homeowners rather than renters. The table shows geographical differences in the long-run annualized effects of TFP growth on homeowners' purchasing power. All entries are in 2017 dollars. Column 1 shows the direct annualized effect of TFP growth on per-worker purchasing power, column 2 shows the indirect annualized effect, column 3 shows the total annualized effect (sum of direct and indirect effect), and column 4 shows the annual percent effect with respect to 1980 average earnings for homeowners in each city.