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Journal of Urban Economics 53 (2003) 1–28

JOURNAL OF Urban Economics

www.elsevier.com/locate/jue

Marshall's scale economies

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Received 7 January 2002; revised 10 June 2002

Abstract

Using panel data this paper estimates plant level production functions for machinery and high-tech industries that allow for scale externalities from other plants in the same industry locally and from the scale or diversity of local economic activity outside the own industry. The paper finds that the count of other own industry plants, representing a count of local information spillover sources, has strong productivity effects in high tech but not machinery industries. Single plant firms both benefit more from and generate greater external benefits than corporate plants, given their greater reliance on external environments. On dynamic externalities, high-tech single plant firms benefit also from the scale of past own industry activity. I find little evidence of economies from the diversity or scale of local economic activity outside the own industry.

JEL classification: R000; O300; L600; S620

Keywords: Scale economies; Agglomeration; Dynamic externalities; Productivity

0. Introduction

In this paper, using panel data, I estimate plant level production functions that include variables that allow for two types of scale externalities which plants experience in their local industrial environments. First are externalities from other plants in the same industry locally, usually called localization economies or, in a dynamic context, Marshall, Arrow, Romer (MAR) economies. Second are externalities from the scale or diversity of local economic activity outside the own industry involving some type of cross-fertilization, usually called urbanization economies or, in a dynamic context, Jacobs economies. Estimating production functions for plants in high-tech industries and in capital goods,

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0094-1190/02/$ – see front matter
doi:10.1016/S0094-1190(02)00505-3

or machinery industries using the Longitudinal Research Data base (LRD), I find that local own industry scale externalities, as measured specifically by the count of other own industry plants locally, have strong productivity effects in high-tech but not machinery industries. I find evidence that single plant firms both benefit more from and generate greater external benefits than corporate plants. On timing, I find evidence that high-tech single plant firms benefit from the scale of past own industry activity, as well as current activity. I find no evidence of urbanization economies from the diversity of local economic activity outside the own industry and limited evidence of urbanization economies from the overall scale of local economic activity.

A number of productivity studies (e.g., Ciccone and Hall [9], Henderson [20], Nakamura [30], and Sveikauskas [37]) have attempted to sort out whether local scale externalities are localization-MAR economies from the scale of local own industry activity versus urbanization-Jacobs economies from cross-fertilization enhanced by the scale or diversity of activity outside the own industry locally. The issue is important for urban development. If an industry is subject to just MAR/localization economies, producers are likely to cluster together primarily in a few cities specialized in traded good production in just that activity, or a closely interconnected set of related activities. Specialization enhances full exploitation of scale externalities, while conserving on local land rent and congestion cost increases. And, indeed, many standardized manufacturing activities such as textiles, food processing, steel, auto production, and wood products tend to be found disproportionately in smaller specialized metro areas (Black and Henderson [6]).

However, if an industry is subject more to Jacobs/urbanization economies, to thrive it needs to be in a more diverse, and hence usually larger local environment. So high-fashion apparel and publishing manufactures and financial, business, research and development and management services tend to be found disproportionately in larger metro areas (Kolko [26]). There is a general notion, now formally modeled in an innovative paper by Duranton and Puga [12], that the nature of externalities changes with product development. In a product cycle type situation, experimental activity is initially found in large diverse, cross-fertilizing metro areas; but standardized production is decentralized in smaller more specialized (and lower cost) metro areas (Duranton and Puga [12]). My finding that externalities for production plants are primarily localization-MAR may not be surprising, given we are examining externalities in standardized manufacturing production activity, not experimental activity.

The finding that scale economies in manufacturing production are primarily localization ones is not new, although findings in the literature vary. However, previous studies such as those referenced above have relied on city-industry aggregate productivity data, usually in cross-section analysis. This is the first study to estimate the effect of externalities on productivity using plant level data in a panel context. Panel data allow us to start to deal with some of the selectivity issues that have been ignored in cross-section work and may help in dealing with endogeneity issues. But a major benefit of having micro-data following individual plants over time is that, for the first time, we can explore of a variety of hypotheses about the details of the generation and reception of these externalities. Examples of these hypotheses are discussed next.

There is an analysis in the literature about the source, or micro-foundations of local scale externalities. As discussed in Marshall [28] and modeled in a variety of theoretical papers (e.g., Fujita and Ogawa [16] and Helsley and Strange [18,19], local scale economies may arise from information spillovers, search and matching processes in labor markets, local intra-industry specialization, and the like. Given the findings in Jaffe et al. [25] and Adams and Jaffe [1], there seems to be evidence that information spillovers are critical. While it is difficult to provide direct evidence on the issue, my results are suggestive also of information spillovers. In the results, localization-MAR economies arise specifically from the count of own industry plants, not from the local scale of own industry employment which measures the scale of the local industry-specific labor market. They are also Hicks' neutral, not interacting with material input usage, indicating no tendency of individual plants to outsource more (and hence specialize more) with local own industry scale. In terms of information spillovers, we might think of a model where each plant engages in a set of experiments about contemporaneous choices of suppliers, of specific fixed and variable inputs, and of methods for dealing with local regulators. All local plants benefit from learning the outcome of such experiments and the spillovers are proportional to the number of plants, or experiments.

In terms of the spatial decay of external effects, non-productivity based evidence in Jaffe et al. [25] and Rosenthal and Strange [35] suggests externalities attenuate sharply with distance.[1] I do not have detailed location information on plants to estimate decay functions; but I can examine whether externalities emanate just from plants in the own county, as opposed to, in addition, from plants in nearby counties in the same metro area.

Given these findings, the paper turns to a variety of more detailed issues. First is the issue of whether externalities apply to and derive more from single-plant firms than corporate multi-plant firms. Single plant firms may be more reliant on the external environment than corporate plants, which may exploit internal-firm networks. Corporate plants may be more isolated and insulated from local environments. I also ask whether plants get greater externalities from existing more mature plants or from an infusion of newborns, bringing new ideas and experimentation; and I ask whether the effect of externalities on productivity declines with plant age or plant vintage. These issues are relevant for thinking about spatial clustering and formation of industrial parks. What types of plants benefit from spatial proximity—when do same industry plants benefit from being grouped and what types of plants can be put in disparate clusters for, say, access to public infrastructure?

Another key issue concerns whether externalities derive only from the *current* local industrial environment. Or does what was going on around a plant several years ago also affect productivity today? That is, are there lagged effects? In an information spillover context certain "experiments" in choices of sellers, inputs and responses to regulators yield immediate results and improve current decision-making by other plants. But there may be other experiments that take several years to fruition or, alternatively, for which the results diffuse slowly, even locally, so effects are lagged. Once results are revealed that yields instantaneous improvements in plant operations if the information is still relevant. But this is a description of "static" spillovers from experiments that are repeated on

[1] Rosenthal and Strange [35] have a nice paper trying to examine spatial decay using plant birth data, rather than productivity data. Below we argue that in such modeling it is hard to separate out externality effects from other phenomena, such as mean reversion and local births as replacements for local plant deaths.

an on-going basis to assess changing local market conditions and choices of suppliers, buyers, etc. One could distinguish this from a cumulative, experimentation process building up a stock of local trade secrets, so that externalities are dynamic in nature. Dynamic externalities are the underpinnings of endogenous growth models (Romer [34]), including those in urban settings (Eaton and Eckstein [13], Black and Henderson [7]). In an urban context, each locality may, for example, build up a stock of local "trade secrets" dependent on current and past industrial activity (Glaeser et al. [17]), involving sets of cumulative "experiments." That local knowledge accumulation affects productivity of local firms. We cannot distinguish between these two interpretations to lagged effects. But we can assess whether lagged effects exist, where lagged effects of externalities may have strong implications for industrial mobility (Rauch [32]). New locations have trouble attracting industries subject to lagged external effects because they cannot offer information spillovers from the past.

So far, no productivity studies have investigated lagged external effects. Studies investigating the existence of so-called dynamic externalities (Glaeser et al. [17] and Henderson et al. [23] examine employment growth patterns between two time periods, asserting that, if an industry's growth is related to base period own industry concentration or to metro area scale, that is evidence of dynamic externalities). As I will show, such inferences are problematical. In examining employment growth, there are allocative shocks across locations (Davis et al. [10]), which underlie own industry mean reversion of local employment. It is difficult to disentangle dynamic externalities from mean reversion processes—both typically involve the same quantity, measures of past own industry employment. Second, if, for example, metro area scale affects own industry employment growth, while that could be due to scale externalities, it could also be due to time invariant aspects of the local environment such as resource endowments and regulatory structures, which affect both metro area sizes and specific industry growth rates. Examining the direct effects of historical environments on plant *productivity* in a panel context will permit isolation of lagged external effects from other factors.

In general, the literature presents conflicting evidence about the nature of scale externalities, depending on the specification used to identify scale effects, the level of aggregation of the data, and the extent to which estimation deals with potential sources of bias. By use of plant level data on productivity, in carefully chosen specifications, I not only avoid the flawed procedure of making scale externality inferences from city-industry employment growth equations, but I also can deal more effectively with the key selectivity and endogeneity issues in estimation, as well as separate current from lagged externality effects.

1. Industries and data

This paper uses plant level data on productivity for 1972–1992 from the LRD of the Census Bureau. It utilizes data from the same source for 1963–1992 to calculate various contemporaneous and historical attributes of scale and diversity of the local industrial environment that might affect productivity. The environments involved potentially cover 742 counties in 317 metropolitan areas. In terms of industries, I assembled data on the five major three-digit capital goods, or machinery industries (excluding the ill-defined residual SIC 359) and on the four major three-digit high-tech industries. The selection is detailed in Appendix A. The machinery industries are construction (SIC 353), metal working (354), special industrial (355), general industrial (356), and refrigeration (358) machinery and equipment. The high-tech ones are computers (357), electronic components (367), aircraft (372), and medical instruments (384).

Data on plants and localities come from the Census of Manufactures for 1963 and then for every 5 years from 1967 to 1992. For each county and each MSA we know by industry and for overall manufacturing, the number of plants, level of employment, births and deaths of plants, diversity across industries, the number of plants belonging to multi-plant versus single plant firms, and the like for the universe of plants. So we know the characteristics of the local industrial environment in considerable detail for both MSA's and counties within MSA's. Some local industrial environment characteristics that deal with the composition of the local economy outside of manufacturing come from County Business Patterns data for 1977–1992.

For machinery and high-tech industries, I examine plant productivity as influenced by the local industrial environment for two different samples of plants. The first is a basic sample drawn from the Census. Census years are the natural choice since they are the only years for which we have complete information on the external environment of plants to relate to plant productivity. However, as we will see, the drawn sample tends to cover only plants of multi-plant firms, which I call corporate plants. Since externalities may be more important for single plant firms which I call "non-affiliates," I draw a second separate sample of just non-affiliate plants from the Annual Survey of Manufactures (ASM) in non-Census years.

Let us start with the sample for corporate plants from the Census. In drawing any estimating sample, I must impose two restrictions. First is that estimation is based on surveyed (actually reported) inputs and outputs, as opposed to imputations. I avoid imputed records since imputations for capital, materials, or even sales are typically based on wage and employment numbers and production function estimation from imputations would in part reflect imputation rules and not productivity relationships. Details on sample selection are in Appendix A, but eliminating records with imputations generally only leaves plants that are also in the ASM for that Census year. That leaves 15–20% of plants from the Census in the first cut at creating an estimating sample for corporate plants.

A second restriction for any estimating sample is that each plant appears at least twice, which in this first sample means it must appear in two different Censuses. The restriction follows from the use of panel methods, either plant fixed effects or first differencing. Given the first restriction of no imputed data, we are thus generally also requiring a plant to appear in the ASM in those two different Census years. Unfortunately, the ASM is done in five-year waves where samples differ across waves. Each five-year wave starts the second year after a Census and is drawn from the sample of plants in the prior Census. So, for example, the 1979–1983 ASM wave covers just the 1982 Census in terms of data, but is picked from plants existing in the 1977 Census. There are several implications of this. Generally only large plants of large firms are covered in the sample, because only these plants are included in every wave of the ASM. The ASM includes a sampling of smaller plants and firms, but that part of the sample changes with each wave. Since a plant must appear in two Censuses and since the ASM waves will differ for those Censuses, only larger plants in larger firms survive in the estimating sample.

A second implication of the sampling procedure and the construction of ASM waves is that almost no plants in the corporate sample are new. For example, plants born between 1972 and 1977 would only generally first appear in the ASM in a Census year in 1982, 5–10 years after birth. Finally, because of compositional differences in ASM waves (as well as plant deaths), the requirement for a plant to appear in two Censuses reduces the estimating sample to 8% of producing plants across the nine-subindustries.[2] This sample consists mostly of plants belonging to multi-plant firms. Since I draw a separate sample of single plant firms (next paragraph), I further eliminate any single plant firms from the Census sample to have a sample of corporate plants belonging to multi-plant firms.[3] For high tech, this last restriction eliminates a further 20% of plants, with little effect on results. For machinery, the reduction is a further 35% and it affects results. Corporate and single plant firms in machinery appear to have rather different production processes, as we will see. Despite these reductions, the absolute samples still remain large and cover a very wide geography.

In drawing the second sample of non-affiliates, I use ASM data in non-Census years examining single plant-firms in the first and last year of a wave, to yield two plant observations. For the 1979–1983 wave, for example, I then link productivity growth between 1979 and 1983 to changes in the industrial environment between the Census years of 1977 and 1982. More details are given in Appendix A. In the non-affiliate sample, given the construction of ASM waves almost every plant only appears twice in the data set—at the beginning and end of a wave. For the corporate plant sample each plant appears on average a little over three times. Even though the data for any non-affiliate plant sample is usually within a single wave, because the sample for a wave is generally based on the prior (not most recent) Census, non-affiliate plants tend to also not be new. We will explore the effects of plant age on externality benefits, but generally we do not cover brand new plants for which benefits might be most critical. That is a drawback to measuring productivity benefits directly from productivity data.

To get a sense of the high tech and machinery sectors nationally, Table 1 gives basic numbers on the national sizes and the spatial distribution of these sectors nationally and how size and spatial distribution have changed since 1963. While the national average high-tech industry employment almost doubled from 1963 to 1992, machinery is unchanged. I examine spatial distributions at the MSA level, for 317 MSA's (defined, consistently, for the same counties in 1963 and 1992). All the industries are agglomerated: they have noticeable Ellison–Glaeser indices of concentration and significant fractions of MSA's have absolutely zero employment in any particular subindustries. High-tech industries are substantially more agglomerated than machinery. They have higher Ellison–Glaeser indices and more zero employment MSA's (despite higher national employment).

An interesting feature to Table 1 is how concentration has changed over time. The degree of concentration as measured by the Ellison–Glaeser index in the high-tech sector

2 While some weights exist to do weighted regressions from the complete ASM, it is impossible to determine the relative weights in the estimating sample, given the numerous and varied restrictions.

3 Earlier versions of the paper did not have this restriction.

Table 1
Industry size and agglomeration (average across industries)

	National employment (1000s)		Ellison–Glaeser[a] concentration index		Shares of national employment: 3 highest ranked city employers		4–32 ranked city employers		The rest		No. of zero employ MSA's Out of 317	
	1963	1992	1963	1992	1963	1992	1963	1992	1963	1992	1963	1992
High tech	239	399	0.026	0.028	26	24	54	46	20	30	173	90
Machinery	200	203	0.013	0.0071	19	13	46	35	35	52	106	51

[a] The Ellison–Glaeser index is $\sum_{j=1}^{317}(E_{ij}(t)/E_i(t) - E_j(t)/E_n(t))^2$, where E_{ij} is employment in industry i in city j, E_j is city j's total manufacturing employment, E_i is national employment in i, and E_N is national manufacturing employment. The index is the sum over cities of the squared deviations of each city's share of national employment in industry i from its share of national manufacturing employment. If for industry i, each city's share of industry i mimics its share of total manufacturing, industry i is perfectly deconcentrated and the index has a value of zero. The maximum value of the index when an industry is totally concentrated approaches two; in that case, one city's share of national employment in i is one, while national manufacturing employment is highly concentrated elsewhere.

stayed the same (or increased slightly), while that in machinery declined sharply from 1963 to 1992. That pattern is also reflected in the changes in the share of national employment of the three largest city employers (whose shares drive the magnitude of the squared elements of the Ellison–Glaeser index). However, at the lower end, in both industries there was a substantial spreading out of employment, not readily captured by the Ellison–Glaeser index. The number of zero employment MSA's fell in half and the share of national employment of the bottom 90 percentiles of cities increased. This increase is most noticeably at the expense of medium-large employer cities, those ranked 4–32. In the paper, we will try to relate the extent of and changes in agglomeration to the extent and changes in scale economy magnitudes.

2. Measuring effects

In this section, I estimate the nature and extent of agglomeration economies. Specifically I estimate production functions at the plant level, looking for direct effects on productivity of the current and historical local industrial environment. Based on a first-order Taylor series expansion (in logs) of a general production function for a plant in a particular subindustry, output of plant k in MSA/county j at time t, $y_k(t)$, is hypothesized to be

$$\ln y_k(t) = \alpha \ln X_k(t) + \sum_{s=0}^{2} \beta_s \ln E_j(t-s) + \delta(t) + f_{kj} - \varepsilon_{kj}(t). \qquad (1)$$

I also look at results for second-order (or translog) and TFP specifications of plant internal technology.

In (1), $\ln X_k(t)$ is the vector of plant inputs which are capital, labor, and materials. $\ln E_j(t-s)$ is a vector of industrial environment variables in $(t-s)$, such as the total number of plants in the same subindustry in the county in time $(t-s)$. Industrial environment variables are entered as having Hicks' neutral effects, a presumption I will test. $\delta(t)$ is a time fixed effect; f_{kj} is a plant location fixed effect; and $\varepsilon_{kj}(t)$ is the contemporaneous error term. Apart from the simplicity and convenience of the fixed effects formulation, the modeling of plants as having fixed effects per se (say, representing the entrepreneur/manager's ability) is supported by econometric testing in Roberts and Tybout [33] and by the analysis in Baily et al. [4], and is the subject of modeling (e.g., Lucas [27]). Given a fixed effects formulation, inferences about industrial environment variables will be based on how changes in an existing plant's environment affect its productivity. Also use of fixed effects is one way of dealing with selectivity issues, potentially a major problem.

As noted earlier, theory tells us that firms in the same industry that are subject to localization economies will cluster together, and empirical evidence supports the notion of spatial clustering of like activity (e.g., Ellison and Glaeser [14]). Evidence suggests that, for an industry, locations where firms cluster tend to change fairly quickly over time (Beardsell and Henderson [5]). In estimating the effects on productivity of local scale externalities, the question is whether, within the same industry, different types of plants are randomly distributed across big and small clusters in terms of unobserved plant characteristics. For example, plants with high f_{kj} might locate disproportionately in centers with greater concentrations of own industry activity, although where plants locate comes out of a general equilibrium analysis which also analyzes how wages, rents, and material costs vary across locations. Regardless, fixed effects is a first cut at controlling for plant unobservables that affect location selection.

In estimation, I pool high-tech industries and then machinery industries constraining within each group the α's and β's to be the same. Results for individual high-tech and machinery industries are in Henderson [20] and individual industry results are similar to the grouped results for the two sectors. Under pooling, $\delta(t)$ becomes $\delta_i(t)$ or there are a separate set of time fixed effects for each industry, i. Fixed effects (and OLS) estimation assumes exogeneity of RHS variables to the $\varepsilon_{kj}(t)$; the potential problem that $\varepsilon_{kj}(t)$ are not orthogonal to covariates will receive considerable attention in the paper.

In Eq. (1), the $\ln E_{kj}(t-s)$ variables are measures of the external environment. In assessing the nature of externalities, we want to know if a plant learns from existing plants, from new plants, within just its county, across the MSA, from the past, etc. For localization/MAR externalities, for Census years, I constructed county and metro (MSA) level measures of own industry employment, number of own industry plants of both multi- and single-plant firms and number of own industry births (since the prior Census), to try to assess the source of externalities. I examine static externalities, for $s=0$, or $\ln E_{kj}(t)$; and I examine lagged effects for $s=1$ and 2, or $\ln E_{kj}(t-1)$, and $\ln E_{kj}(t-2)$, where time intervals are five years. So I am asking if the local industrial environments from five or ten years ago affect productivity today.

In terms of urbanization/Jacobs economies, I start with lack of diversity measures at the MSA level, consistent with Jacobs' [24] notions that **metro-wide** diversity is critical to productivity gains from cross-fertilization. The various lack of diversity measures describe the degree of specialization of total private employment in the MSA outside the own industry, of total manufacturing employment and of employment in related industrial activities as described momentarily. The measure used is related to the Ellison–Glaeser [14] index in Table 1, but covers a different dimension. Specifically, for MSA j, the degree of MSA specialization in a set of activities is

$$S_j(t) = \sum_i \left(\frac{E_{ij}(t)}{E_j(t)} - \frac{E_i(t)}{E(t)} \right)^2, \tag{2}$$

where $E_{ij}(t)$ is employment in industry i in city j, $E_j(t) \equiv \sum_i E_{ij}(t)$ is total employment in city j summed over the relevant i, $E_i(t)$ is national employment in i, and $E(t) \equiv \sum_i E_i(t)$ is total national employment over the relevant i. $S_j(t)$ is the sum of squared deviations of industry i's share in city j of local relevant employment from industry i's national share. If a city's shares over all industries mimic national shares it is perfectly diverse; and $S_j(t)=0$. As city j's shares start to deviate from national shares $S_j(t)$ starts to rise. At the limit $S_j(t) \to 2$, where in city j industry i's share is one, while some other industry's share of national employment approaches one. In this case the city is completely specialized, or has no diversity within the relevant set of activities. A version of the Jacobs hypothesis is that as metro specialization, $S_j(t)$, rises, plant productivity declines.

In defining the relevant i, I experiment with five sets of activities:

1. Overall manufacturing employment for 20 two-digit manufacturing industries.

2. Overall private employment (80 two-digit industries).
3. For machinery industries, three-digit level employment within SIC 3500.
4. For high-tech industries, employment in high-tech manufacturing, defined as computers (357), communications (366), electronic components (367), aircraft (372), missiles and space vehicles (386), search and navigation equipment (381), measuring devices (382), and medical instruments (384).
5. For high-tech industries, employment in sophisticated private services (engineering and architectural, research and testing, computer programming, medical and dental labs, and private colleges and universities).

Besides measures of metropolitan specialization, or lack of diversity, given they will turn out to have no effect on productivity, I experiment with more traditional measures, the overall MSA scale or total employment in each of the listed activities and I also experiment with MSA scale measured by counts of plants. Finally, I consider county level, as opposed to MSA level, effects.

Any results on urbanization-Jacobs economies are subject to a proviso. Among Ciccone and Hall's [9] objections to a form such as Eq. (1) is that plant purchases of service (versus material) inputs are not recorded in Census data at all prior to 1992.[4] Then, for example, if a city diversifies over time in services, and plants purchase more outsourced services (accounting, janitorial, photocopying, payroll, etc.), output could rise, for the same observed inputs. In estimation of urbanization-Jacobs economies, we might attribute an output increase to changes in Jacobs/urbanization diversity measures, when in fact no spillovers are involved. Rather plants are outsourcing more. I will keep this issue in mind when interpreting results.

2.1. Estimation issues

In Eq. (1), time–industry fixed effects, $\delta_i(t)$, control for national shocks to productivity and for inflation. I use nominal measures of output, capital, and materials, avoiding issues about the accuracy of various national deflators and the extent of national productivity change. Those are topics beyond the scope of this paper. The f_{kj} represent time invariant plant and location fixed effects. Given high fixed effect plants (e.g., those run by talented entrepreneurs) may congregate in high fixed effect locations (e.g., those with strong regional amenities, resources, or institutions), I cannot disentangle plant and location fixed effects, but that does not affect the estimation. However, the f_{kj} will influence the $\ln E_j(t-s)$ and $\ln X_k(t)$, which means OLS estimates are biased (and indeed random effects estimates are rejected in favor of fixed effect ones by Hausman tests in all cases). Accordingly, as noted above, I estimate Eq. (1) for unbalanced panels of plants across

[4] I have two other comments on Ciccone and Hall's objections. First, their solution of using aggregate regional BEA income data may not solve the problem, since BEA has to estimate the service data used in calculating local value added and productivity. Second, they object to (1) for aggregate city-industry data, because of "doubling counting"—one plant's output is another's inputs in the same industry. Use of plant level data negates the issue. Moreover, even with the aggregate data, under the CRS assumption permitting aggregation, Eq. (1) remains valid. Double counting is obviously an issue for income accounting, but not in specifying production function forms.

counties and MSA's by standard fixed effects methods, where the f_{kj} appear as variables, or nuisance parameters. Doing so raises two key issues.

The first issue is that use of fixed effect methods requires sufficient time variation across plants in all variables, to be able to make inferences about effects of changes in the environment on productivity. Plant inputs and output display large variation, as do local industrial environment variables. The potential problem would lie with indices such as the specialization indices in (2). If we have annual data, the variation in specialization indices is very small. For the data here in five-year intervals, there is sufficient variation. In particular, for estimating samples, the *average* of the percentage change of absolute deviations for any specialization measure $(|S_j(t) - S_j(t-1)|/S_j(t))$ always exceeds 15% (with or without outliers) between any five-year time periods in all samples.

The critical issue is that, for unbiased estimates under fixed effects, we require that the plant inputs, $\ln X_k(t)$, and the industrial environment variables, $\ln E_j(t-s)$, are strictly exogenous for all t to the $\varepsilon_{kj}(t)$. That assumption begs the question of why $\ln E_j(t-s)$ measures, such as number of local own-industry plants, vary over time if not in response to $\varepsilon_{kj}(t)$. I assume the $\ln E_j(t)$ and $\ln X_k(t)$ vary in response to, say, changes in local wages, rents, and taxes. Such changes make location j a better or worse place in which to locate, or one factor cheaper than another; but these unobserved changes have no direct effect on plant productivity. Also in Eq. (1), in terms of $\ln X_k(t)$, capital stock is beginning of year so it and arguably labor and materials (chosen in t before revelation of $\varepsilon_{kj}(t)$) are exogenous to the $\varepsilon_{kj}(t)$.[5] I have strong priors that, after controlling for plant/location fixed effects and national time–industry fixed effects, such shocks are contemporaneous idiosyncratic plant output shocks.

Whatever my priors, there may be local shocks, such as provision of MSA infrastructure and upgrading in quality of the MSA labor force, that may affect both plant productivity and the local (county) industrial environment. Second in Eq. (1), output and materials are measured in monetary terms. One can assume that these goods are traded on a national basis and relative spatial prices are determined by national transport networks that vary little over time; then fixed effects would take care of these time invariant relative price differences. But one could be concerned that markets are more localized. Changes in relative output prices across locations over time would affect both the nominal output measure and choices of inputs, as well as local own industry scale. On the input side, changes in material input prices affect both measures of material inputs and outsourcing decisions (relative to in-house production) and hence plant efficiency and output (Ono [31]). I conducted different sets of experiments in considering these possibilities. All reinforce results presented later.

First I tried adding in MSA-time fixed effects in addition to plant/location fixed effects to directly control for contemporaneous MSA labor force, infrastructure, and local input and output price shocks. While results are similar to those obtained with plant/location fixed effects, the procedure is suffers from efficiency problems. First it completely eliminates consideration of MSA-wide industrial environment variables that are relevant for Jacobs-

[5] However, if annual data were used it would be less clear that the $\ln X_k(t)$ are also exogenous to the $\varepsilon_{kj}(t-1)$ as required—that last period's shock does not affect this period's inputs. My data are spaced five years apart, so, in fact, it seems reasonable that there is no effective impact of a shock from five years ago on inputs today.

urbanization economies. Second, it eliminates single-county MSA's, sharply cutting some of the samples. Third, for county variables on localization economies, identification is now based only on time variation of contemporaneous county differences in environments within an MSA. For MSA's with dominant counties, variation is limited.

In a second set of experiments, to directly control for local variations in input prices and outsourcing effects on productivity, I use the non-diversity measures for manufacturing, high tech, machinery, and all economic activity in Eq. (2). These measures were discussed earlier as measuring urbanization/Jacobs economies, such as information spillovers.[6] As such these measures could be doing double duty, controlling for externalities and for effects of material input price variations, which will make their interpretation difficult (see later).

In a final set of experiments, to more generally deal with endogeneity of all RHS variables to the $\varepsilon_{kj}(t)$, I tried instrumentation. For 2SLS in a panel, instrumentation requires all instruments be strictly exogenous to all $\varepsilon_{kj}(t)$. Such instruments that I have are little correlated with plant inputs; and the problem of weak instruments dominates (Bound et al. [8]). Instruments such as market potential of the MSA and county air quality attainment status are somewhat correlated with $\ln E_j(t-s)$, but they are still weak instruments in general. In almost all these 2SLS experiments, externality results tend to rise to unbelievably high levels. So I turned to IV estimation of the production function in (1) by GMM, as a set of equation years. I first difference, to obtain a set of first differenced estimating equations (e.g., 92–87, 87–82, etc.). I impose equal slope coefficients across years, but can now instrument with *predetermined* variables such as lagged plant inputs and lagged industrial environment variables, under the presumption that local market and internal firm frictions indicate a dynamic adjustment process such that predetermined values of variables are correlated with future changes. While this helps with the weak instrument problem, instruments for early equation years remain weak (see later). A further drawback is that estimation requires plants to remain in the sample for a considerable period of time, drastically reducing sample size. In the estimation I test for exogeneity assumptions on instruments, as well as assumptions in Eq. (1) on absence of serial correlation in the $\varepsilon_{kj}(t)$.

There is one final issue concerning the $\varepsilon_{kj}(t)$, which affects standard error calculations for coefficients. Once fixed effects are controlled for, in a given year are the contemporaneous shocks affecting plants in the same locality correlated? As noted earlier, I believe that, after controlling for plant/location fixed effects and national time-industry fixed effects, such shocks are idiosyncratic plant output shocks that are locally uncorrelated. This is consistent with my reading of Davis et al. [10]. But Moulton's [29] issues of incorrect standard errors in a context with more plant observations than geographic areas (given geographic covariates) cannot be ignored. In this context though, as we will see the number of MSA's and counties in the sample is enormous; but it is the case that there are typically multiple plants per MSA in any estimation. Results on standard errors with contemporaneous errors terms clustered by MSA-year versus results with unclustered errors are almost the same, with standard errors moving up *or* down typically by 5–10%. Breusch–Pagan

[6] Urbanization economies could be Dixit–Stiglitz [11] diversity effects in local intermediate input markets (Fujita [15]) as well, which do not directly imply local price effects in input markets.

test for clustering cannot reject the hypothesis of unclustered errors. Therefore we report robust (White corrected) standard errors without clustering.

3. Results

I estimated many different models for different industries, by a variety of statistical techniques. The results presented are the key, robust findings for the four industry groups: corporate high-tech plants, high-tech single-plant firms called "non-affiliate" plants, corporate machinery plants, and machinery non-affiliate plants. The presentation starts with the key summary results on the nature and magnitude of localization economies. I then discuss various other formulations for localization economies. Then I turn to lagged own industry external effects and finally to urbanization-Jacobs economies.

Table 2 presents results for two statistical formulations in columns 1 and 2 for each of the four samples: OLS (i.e., just industry–time dummies) and then primary results under plant/location fixed effects. For plant inputs of labor, materials and capital, coefficients are of expected magnitudes and generally highly significant. Several comments are relevant. First, I do not divide labor into production and non-production workers, in part because the skill distinction between the two has blurred in recent decades. Making the distinction does not affect other results and coefficients for non-production workers are not that robust. Second, under OLS, input coefficients generally sum to something close to one, consistent with CRS. However, with fixed effects, coefficients generally sum to less than one, indicating either decreasing returns or omission of a factor such as entrepreneurship (in the plant fixed effects) consistent with the Lucas [27] model. Third, the result of imposing fixed effects differs by input, with coefficients rising modestly for labor typically, falling for materials, and falling considerably for capital. The last is not an unusual result of imposing fixed effects. A typical interpretation is that this is attenuation bias accentuated under fixed effects because capital is poorly measured by book value. However, under the time differencing involved in fixed effects, one is correlating investment changes with output changes where changes in book value more accurately measure investments.[7] An alternative interpretation is that capital stocks are highly correlated with unobserved entrepreneurial talent (represented by the fixed effect), so that OLS results overstate the capital coefficient.

Finally, in the fixed effect results, the technology for corporate and non-affiliate plants differs. While their use of labor and capital is similar, their use of materials differs. The difference for high tech is not statistically significant; but machinery corporate plants do a lot more outsourcing, or have a significantly higher materials input coefficient (0.437 vs. 0.327), than non-affiliates. This is consistent with evidence in Ono [31] for outsourcing of service inputs. While in-house production is often viewed as having high fixed costs, implying that small plants are more likely to outsource, in fact, the evidence is consistent with a model where the fixed costs arise in the outsourcing decision. Small plants do not

[7] Of course if investments are delayed in being brought into full usage in production, changes in capital will have reduced effects in output. However, if I use lagged values of capital in estimation, coefficients are even smaller.

Table 2
Basic results on localization economies

	Census sample		Non-affiliate sample	
	(1)	(2)	(1)	(2)
		A. High tech		
ln(hours worked)	0.436[a]	0.501[a]	0.445[a]	0.491[a]
	(0.015)	(0.024)	(0.023)	(0.052)
ln(materials)	0.499[a]	0.393[a]	0.436[a]	0.338[a]
	(0.012)	(0.020)	(0.0203)	(0.037)
ln(capital)	0.082[a]	0.055[a]	0.101[a]	0.053[a]
	(0.0089)	(0.014)	(0.014)	(0.021)
ln(no. of own industry plants in county)	0.012[a]	0.080[a]	0.021[a]	0.081[b]
	(0.0041)	(0.021)	(0.0063)	(0.044)
Industry–time fixed effects	yes	yes	yes	yes
Plant/location fixed effects	no	yes	no	yes
adj R^2	0.944	0.961	0.941	0.964
Sample size	4046	4046	1266	1266
Plants	1419		554	
MSA's	207		110	
Counties	312		157	
		B. Machinery		
ln(hours worked	0.445[a]	0.508[a]	0.499[a]	0.490[a]
	(0.014)	(0.022)	(0.012)	(0.025)
ln(materials)	0.485[a]	0.437[a]	0.425[a]	0.327[a]
	(0.0087)	(0.016)	(0.0094)	(0.017)
ln(capital)	0.062[a]	0.027[a]	0.067[a]	0.029[a]
	(0.0078)	(0.012)	(0.0058)	(0.0088)
ln(no. of own industry plants in county)	0.019[a]	0.023	0.020[a]	−0.016
	(0.0031)	(0.015)	(0.0033)	(0.023)
Industry–time fixed effects	yes	yes	yes	yes
Plant/location fixed effects	no	yes	no	yes
adj R^2	0.946	0.959	0.932	0.957
Sample size	6781	6781	5027	5027
Plants	2304		2151	
MSA's	250		217	
Counties	432		352	

[a] Significant at 5% level.
[b] Significant at 10% level.

have enough volume of business to develop outsourcing relationships (with specialized orders) and in-house more, while larger, corporate firms develop outsourcing relationships. For machinery we will return to these points when discussing urbanization economies later on.

The focus in Table 2 is on localization economies, which are measured in the table by the number of plants in the own subindustry in the county (not MSA). So, for example within high tech, for a computer plant, localization economies are measured by the count of computer plants in the same county. Later in Table 3, I will consider a variety of alternative measures of localization economies.

Under OLS estimation in column (1), significant localization economies exist in small magnitudes in all four samples, with elasticities ranging from 0.012 to 0.021, indicating that a 1% increase in the number of own subindustry local plants increases plant output by 0.021% or less. Plant fixed effects to control for both plant time invariant special features (entrepreneurial ability) and location amenities (local regulatory and business culture and basic urban infrastructure) change the results dramatically. In high tech, in column (2) compared to column (1), the magnitudes of localization economies rise four-fold, to around 0.08 in both samples. These are significant localization economies, indicating that, for example increasing the number of own industry plants locally from 10 to 100 increases plant output by over 20% for the same own plant inputs. That is a strong basis for clustering of like economic activity. The magnitudes are similar to those in other productivity studies such as Henderson [20], Nakaruma [30], and Sveikauskas [37], for similar industries. Scale externalities when they exist typically take elasticities up to about 0.10.

One may be puzzled as to why the coefficient rises under fixed effect results, although with multiple affected coefficients the direction of bias for any one coefficient involves complex relationships. But as a partial view, to the extent fixed effects represent county amenities, one would expect plants to gravitate to locations with better amenities. Similarly to the extent fixed effects represent better entrepreneurial talents we might believe better entrepreneurs would migrate to larger clusters (given, for example, they might better afford the higher rents in those clusters (i.e., compete for spots in such clusters)). In either case from this partial view, there would be a positive correlation between local industry scale and fixed effects, suggesting that introducing fixed effects should lower the scale economy coefficients. Apart from the fact that this is just a partial view of bias, as a practical matter in my sample, estimated fixed effects in both high-tech samples are slightly negatively correlated with the local industry scale, perhaps hinting that where clusters occur are “accidents of history” (Henderson [22]).

In machinery, in column (2) with fixed effects, the magnitude of the external scale coefficient is less than under OLS (for machinery samples, local scale and estimated fixed effects are positively correlated). In both samples, the coefficients under fixed effects are insignificant. In regressions where individual machinery industries are distinguished (Henderson [21]), such coefficients are also insignificant for all industries.

These basic results from fixed effect estimation suggest localization economies are strong in high-tech industries and non-existent in machinery. For high tech, magnitudes for non-affiliates appear no different than for corporate plants. This would suggest that corporate plants benefit as much from the external environment (despite intra-firm networks) as do non-affiliates. However, when I investigate other statistical formulations as well as lagged external effects, this conclusion will be altered.

The use of a log linear production function does not drive results on externalities. The results for a translog specification are almost identical. I stick with the conventional log linear production function because translog functions result in poorly behaved global technology specifications. (To get well-behaved ones generally requires incorporating a full system of factor demand equations in order to anchor coefficients.) TFP results in various specifications are typically similar to those for Eq. (1) (Henderson [22]). I stick

Table 3
Issues for localization economies

	Census sample			Non-affiliate sample		
	(1)	(2)	(3)	(1)	(2)	(3)
			A. High tech			
ln(hours worked)	0.506[a]	0.478[a]	0.501[a]	0.504[a]	0.428[a]	0.492[a]
	(0.026)	(0.034)	(0.024)	(0.054)	(0.080)	(0.052)
ln(materials)	0.386[a]	0.411[a]	0.394[a]	0.334[a]	0.413[a]	0.337[a]
	(0.021)	(0.024)	(0.020)	(0.038)	(0.051)	(0.037)
ln(capital)	0.055[a]	0.061[a]	0.055[a]	0.049[a]	0.038	0.053[a]
	(0.015)	(0.021)	(0.014)	(0.021)	(0.031)	(0.021)
ln(no. of county own	0.102[a]	0.103[a]		0.101[a]	0.135[a]	
ind. plants)	(0.027)	(0.035)		(0.048)	(0.064)	
ln(avg. own ind.	−0.026[a]			−0.00068		
Plant employ in county,	(0.0098)			(0.023)		
outside own plant)						
ln(no. of own ind. plants		−0.059[b]			0.043	
in rest of MSA)		(0.032)			(0.055)	
ln(no. of county own–ind.			0.067[a]			0.092[b]
non-affiliate plants)			(0.021)			(0.047)
ln(no. of county			0.025			0.027
own ind. corp. plants)			(0.030)			(0.033)
Ind.–time fixed effects	yes	yes	yes	yes	yes	yes
and plant/location fixed effects						
N	3690	2029	4046	1181	701	1266
adj. R^2	0.960	0.965	0.961	0.964	0.960	0.964
			B. Machinery			
ln(hours worked)	0.497[a]	0.497[a]	0.508[a]	0.489[a]	0.472[a]	0.490[a]
	(0.023)	(0.027)	(0.022)	(0.025)	(0.029)	(0.025)
ln(materials)	0.436[a]	0.437[a]	0.437[a]	0.327[a]	0.330[a]	0.328[a]
	(0.016)	(0.019)	(0.016)	(0.017)	(0.020)	(0.017)
ln(capital)	0.028[a]	0.033[a]	0.027[a]	0.036[a]	0.039[a]	0.029[b]
	(0.013)	(0.016)	(0.012)	(0.0090)	(0.011)	(0.0088)
ln(no. of county own	0.020	0.026		−0.0054	−0.037	
ind. plants)	(0.017)	(0.019)		(0.026)	(0.0274)	
ln(avg. own ind. employ in	−0.0075			0.0012		
county, outside own plant)	(0.0078)			(0.017)		
ln(no. of own ind. plants		0.019			−0.010	
in rest of MSA)		(0.020)			(0.030)	
ln(no. of county own ind.			0.014			−0.021
non-affiliate plants)			(0.014)			(0.025)
ln(no. of county own ind. corp.			0.0012			−0.0048
plants)			(0.016)			(0.018)
Ind.–time and plant	yes	yes	yes	yes	yes	yes
location fixed effects						
N	6360	4729	6781	4770	3697	5027
adj R^2	0.962	0.957	0.959	0.956	0.956	0.957

[a] Significant at 5% level.
[b] Significant at 10% level.

with a production function specification, which avoids the presumption that inputs at each instant are chosen to minimize total contemporaneous costs.

There are a variety of other simple, important experiments pertaining to the results in Table 2. First scale economy magnitudes do not vary over time. Specifically, adding in a slope differential term for local scale for 1972–1982 (vs. the base case of 1987 and 1992) results in coefficients of zero.[8] Also scale economy effects do not vary significantly with plant age or with plant vintage within the samples, so younger and older non-affiliates, for example, benefit equally from the local industrial environment. Scale elasticities do not change with local scale, so no diminution of effects is indicated by either a quadratic specification or a specification allowing for a differential slope if the plant is in a county that ranks in the top eight employment centers over time for that subindustry. Finally, our coefficients represent "average" effects and one might wonder whether their variance differs with local own industry scale. For example, scale effects might operate in a narrower band as local scale rises. Examination of plots of plant residuals against local own industry scale (and comparison of variances of residuals for small and large employment centers) indicates no change in the variance with local industry scale.

3.1. Endogeneity issues

To investigate endogeneity of RHS variables to the $\varepsilon_{kj}(t)$, as explained above, I tried three experiments. I report on the ones that use MSA-time fixed effects and instrumentation here. The one using non-diversity measures to control for variations in relative local input prices is discussed later. Here I first add in MSA-time fixed effects (in addition to plant/location and subindustry time fixed effects) to control for contemporaneous shocks which might influence RHS variables, as well as output. To have variation in localization measures, I restrict estimation to multi-county MSA's. Results are similar to those in column (2) of Table 2. The coefficients (standard errors) on number of county own subindustry plants for high tech corporate, high tech non-affiliates, machinery corporate, and machinery non-affiliates are 0.085 (0.035), 0.346 (0.114), 0.023 (0.020), and −0.0090 (0.037) for sample sizes of 2343, 769, 5140, and 3880, respectively. The only real difference compared to column (2) is that the high-tech non-affiliate coefficient rises four-fold. For high-tech non-affiliates, while the sample size is now quite small, this would be the first evidence that non-affiliates benefit more from externalities than corporate plants. But given the loss in sample size and loss of efficiency (variation in the data), I rely on the formulation in column (2), with just plant/location fixed effects.

The other experiment involves instrumentation. As detailed in Henderson [22], instrumenting to control for non-orthogonality of the $\ln X_k(t)$ and $\ln E_j(t)$ to the $\varepsilon_{kj}(t)$ suffers from weak instruments. With IV estimation of a multi-year system of equations by GMM for (1) in differences, predetermined values of $\ln X_k(t)$ and $\ln E_j(t)$ may be used as instruments (Arellano and Bond [2]). In a balanced panel for corporate plants, equivalent first stage regressions for changes in plant inputs and $\ln E_j(t)$ have R^2's with a range of

[8] For example, for column (2) results for the four respective industry groups, the differential slopes are −0.0043 (0.0084), 0.00064 (0.019), 0.012 (0.0085), and −0.0023 (0.011).

0.15–0.23 and a typical value of 0.20 for high tech, with two periods of predetermined values. For machinery, the range is 0.096 to 0.20, but with a typical value of only 0.10. Neither is great, but it seemed that at least for high tech it was worth proceeding. For corporate high tech, with a balanced panel (and hence at least two values of predetermined variables as instruments for all but the first equation year), the scale elasticity is 0.164 (0.078) and other coefficients are similar to those in Table 2. But the sample size is small (about 1/10 of that in Table 2), given each plant must appear from 1972 to 1992. Using unbalanced panels doubles the sample size, but adds in plants with only a short instrument list (one period of predetermined values, with first stage R^2's of around 0.05, raising standard errors). The coefficient (and standard error) for high tech is then 0.129 (0.091). For non-affiliates, sample sizes are too small to reasonably draw conclusions from estimates (and no estimation was carried out). I do note that IV estimation in corporate high tech strongly supports the absence of serial correlation of the $\varepsilon_{kj}(t)$ and Sargan tests suggest that use of contemporaneous values of variables as instruments in addition to predetermined ones is valid. In summary, instrumenting simply suffers from weak instruments and limited sample sizes, does not yield contradictory results, and does not indicate that correlation of $\varepsilon_{kj}(t)$ with $\ln X_k(t)$ is important. I believe that the fixed effect controls in column (2) are sufficient.

3.2. Other localization specifications

This section focuses on high-tech industries, since it is only for these industries that localization economies are significant. However, the results for machinery are given as well, and all conclusions derived here apply to machinery as well. The first question is why are localization economies measured by a count of the own industry plants in the county. Why not the MSA? Why not a count of own industry employment? Table 3 provides some basic statistical answers to these questions. Column (1) of Table 3 examines the issue of why I use a count of plants as the scale measure. Initially I used employment measures but these yielded weaker results. Decomposing own industry employment in the county into the number of plants and the average employment in those plants (excluding the own plant), as in column (1), reveals the problem.[9] In all cases, average employment per plant does not positively contribute to productivity. This suggests that localization externalities derive from the existence of enterprises per se. This may tell us something about the nature of scale effects. Enterprises could be interpreted as separate sources of information spillovers as in Fujita and Ogawa [16], so externalities are related to the count of such sources, with employment size of the sources (and hence labor market externalities?) being unimportant.

In column (2), I examine the issue of how localized effects are. Experiments with adding the scale of own-industry activity outside the county in the MSA suggest scale outside the own county does not matter.[10] I ran the regressions for all MSA's, for multi-county MSA's,

[9] Sample size falls relative to the usual because of eliminating observations where the plant is the sole subindustry plant in the county.

[10] I note there are lots of plants outside the own county. For example, for high-tech corporate, the average number of plants in a county is 67 and the number outside the own county is 18, rising to 34 in multi-county MSA's.

and for multi-county MSA's where there is a positive count of plants outside the own county for all observations. Results are almost the same in all cases, and I report results in column 2 for the last, most clearly defined situation.[11] Again, in no case is there a positive significant effect of own industry plants outside the own county affecting productivity of a plant. These externality effects seem to be confined to the own county, consistent with other evidence in Rosenthal and Strange [35]. If we find plants in the same industry in separate clusters in two different counties in the same MSA, we would conclude that plants in one cluster do not benefit from direct externalities from plants in the other cluster. The clusters might be in the same MSA to take advantage of common input suppliers, or an accessible output market (controlled for variously by fixed effects, time-MSA fixed effects, and later non-diversity measures).

Another critical issue is whether different types of plants contribute differently to externalities. While at the moment, it appears corporate and non-affiliate plants may benefit equally from externalities, it seems from Table 3 that non-affiliate plants generate greater externalities.[12] In column (3) for the corporate and non-affiliate samples in high tech, the coefficients for the count of non-affiliate plants are 0.067 and 0.092, respectively, while those for the count of corporate plants are insignificant and 0.025 and 0.027, respectively.[13] While the differences are not quite statistically significant, the gaps are large and suggestive. [14] And it is interesting to note in these results that corporate plants also benefit more from surrounding non-affiliates than from other corporate plants. These results accord with Saxenian's [36] case study of Route 128 vs. Silicone Valley for high-tech development.

I also examined (not reported in Table 3) whether new plants (births) contribute more or less to information flows than existing plants. For machinery, in the two samples, coefficients on births and numbers of pre-existing plants entered separately are all insignificant. In high tech, in both samples, coefficients of pre-existing plants noticeably exceed those of births.[15] This result is even more compelling since births are overwhelming non-affiliate plants, which more generally seem to generate greater externalities. While births potentially could be a source of new ideas, they may initially contribute less to externalities because they are less integrated into local networks.

[11] In all work in the paper, adding in a dummy for zero level observations yields a zero coefficient. So, for example, here that would apply in the first or second samples, with a dummy variable if the numbers of plants outside the own county in the MSA are zero.

[12] The average (and standard deviation) of non-affiliate and corporate plants in a county in high-tech Census, for example, are 51 (82) and 16 (22).

[13] The test here is whether corporate and non-affiliate plants entered as separate scale variables in the production function have the same elasticity. An alternative is to assume one scale variable, but to decompose it and do a Taylor series expansion so the scale terms in the production function are $\varepsilon_1 \ln(\text{non-affiliate plants}) + \varepsilon_2$ corporate/non-affiliate plants, where by construction ε_1 should equal ε_2. The actual values of ε_1 and ε_2 are almost identical to the respective coefficients reported in column (3).

[14] As in column (1) adding in the average size of non-affiliate plants results insignificant coefficients (generally negative).

[15] For plant fixed effects, the elasticities for births and pre-existing plants for corporate plants are 0.0431 (0.016) and 0.078 (0.025), while for non-affiliates they are 0.0359 (0.025) and 0.115 (0.041). Adding in dummy variables for cases where a measure is zero results in insignificant coefficients for the dummies.

A last issue concerns whether my assumption of Hicks' neutrality in Eq. (1) is justified. Coefficients on the ln(no. own industry plants in the county) interacted with labor, materials and capital are small and completely insignificant for both the corporate and non-affiliate samples for high-tech industries. Hicks' neutrality is a reasonable assumption. For machinery, there is weak evidence of some interaction with one significant coefficient out of the six possible—for capital for non-affiliates (but the non-interactive capital term becomes negative and insignificant). Later we will see for machinery that any non-neutrality has more to do with urbanization economies.

3.3. Dynamic externalities

Do past environments affect current productivity? As discussed earlier, past environments could contribute, for example, to a "stock of local trade secrets," or local depreciable knowledge accumulation, which enhances productivity of plants in the present. Or past environments might represent a lag structure to, say, information flows or other static externalities. In either case, localities with less past activity in an industry offer less in the way of lagged effects, diminishing current productivity.

I devoted considerable effort to finding lagged effects, given how they are stressed in the growth literature and recent urban literature (e.g., Glaeser et al. [17]). There is absolutely no evidence of lagged effects in either machinery samples or in any individual machinery sector (see Henderson [21]), for localization economies, as well as any of our level or non-diversity measures of urbanization/Jacobs economies noted earlier. So again, I focus just on high-tech for the moment. I examined whether improvements in local industry scale from 5 ($t-1$) or from 10 ($t-2$) years ago affect productivity today and whether urbanization/Jacobs measures also had any impact. For corporate plants there is no evidence of these lagged effects in any form. However, for non-affiliates in Table 4, there is strong evidence of lagged effects for own industry activity externalities from 5 years ago, but not from 10 years ago and not from urbanization/Jacobs measures. Once I allow for

Table 4
Dynamic externalities in high tech

	Census	Non-affiliates
	(1)	(1)
ln(hours worked)	0.501[a] (0.024)	0.494[a] (0.0523)
ln(materials)	0.393[a] (0.020)	0.333[a] (0.037)
ln(capital)	0.054[a] (0.014)	0.051[a] (0.021)
ln(no. of county own ind. plants)		
t	0.073[a] (0.022)	0.089[a] (0.045)
$t-1$	0.021 (0.022)	0.070[a] (0.036)
Ind.–time and plant/location fixed effects	yes	yes
N	4046	1266
adjR^2	0.961	0.964

[a] Significant at 5% level.

lagged effects, it appears localization/MAR effects are much larger for non-affiliates than for corporate plants. This accords with the intuition that non-affiliates are more reliant on external environments, than corporate plants with their intra-firm networks.

3.4. Jacobs-urbanization economies

There is a significant literature advocating the existence and importance of Jacobs-urbanization economies. Diverse and/or large economic bases are thought to promote cross-fertilization among industries, through information spillovers, labor market networks and search, and other sources of externalities. Evidence of this in the literature for manufacturing based on productivity analysis is weak and the results of this study are consistent with that.

To try to isolate Jacobs economies, I examine the effect of lack of local diversification, or the degree of local specialization of the industrial base, on productivity as given in Eq. (1). Sample sizes differ from the usual, for variables for which I did not have 1972 data from County Business Patterns. Controlling for plant inputs, local own industry scale, industry–time dummies, and plant/location fixed effects, for high-tech industries, for both the corporate and non-affiliate samples, variables for non-diversity in MSA manufacturing employment (twenty two-digit industries) and for non-diversity within the MSA high tech sector (nine three-digit industries noted earlier) produce positive, rather than expected negative signs. For high tech, diversity in total MSA employment (eighty two-digit industries) produces negative signs but is completely insignificant. Experimenting with a variable for non-diversity in modern services did not fare any better. Lagged measures have zero effect. Turning to machinery, non-diversity in manufacturing or within the machinery sector produces again completely insignificant coefficients, but non-diversity overall in the corporate sector produces a negative coefficient, significant at the 10% level. We return to this momentarily.

Non-diversity measures have no significant effect on productivity in any circumstance. Moreover these measures have virtually no effect on other coefficients, in particular the materials measure, where some of the diversity measures would relate to local availability, diversity, or pricing of materials. This can be seen, for example, in Table 5, parts A and B, by comparing coefficients from Table 2 with the column (3) coefficients in Table 5 for the case with non-diversity of overall MSA employment represented.

Given this overall rejection of a Jacobs-diversity story, I turned to the more general formulation of urbanization economies, which are represented just by general scale measures. No measure of scale—employment in all manufacturing, employment in all industries, employment in high-tech industries, total plants in manufacturing, total plants in all industries in the MSA, employment in service industries—had an effect on productivity in the high-tech corporate or non-affiliate samples and in any individual high-tech industries in either sample (Henderson [21]). Lagged specifications are similarly insignificant. But machinery is a different story, in terms of static externalities. It is the machinery results that we focus on in Table 5, part A. Corresponding results for high tech are summarized in Part B, to illustrate the statements just made about the lack of urbanization-Jacobs economies in high tech.

Table 5
Urbanization economies

	Census sample			Non-affiliate sample		
	(1)	(2)	(3)	(1)	(2)	(3)
			A. Machinery			
ln(hours worked)	0.506[a]	0.514[a]	0.514[a]	0.489[a]	0.494[a]	0.494[a]
	(0.022)	(0.025)	(0.025)	(0.024)	(0.0276)	(0.027)
ln(materials)	0.436[a]	0.436[a]	0.439[a]	0.327[a]	0.317[a]	0.318[a]
	(0.016)	(0.019)	(0.019)	(0.017)	(0.019)	(0.019)
ln(capital)	0.025[a]	0.016	0.019	0.029[a]	0.032[a]	0.033[a]
	(0.011)	(0.013)	(0.013)	(0.0087)	(0.0094)	(0.00937)
ln(no. of own industry plants in county)	0.015	0.014	0.021	−0.021	−0.033	−0.028
	(0.015)	(0.0195)	(0.018)	(0.024)	(0.026)	(0.026)
ln(all other manu. employ. in MSA)	0.116[a]			0.065		
	(0.034)			(0.048)		
ln(all other employ. in MSA)		0.189[a]			0.087	
		(0.055)			(0.077)	
Non-diversity (two-digit) of MSA employ.			−1.84[b]			−0.717
			(1.01)			(1.10)
Industry–time fixed effects and plant fixed effects	yes	yes	yes	yes	yes	yes
adj R^2	0.959	0.952	0.952	0.957	0.975	0.954
N	6781	4803	4803	5027	4376	4376
			B. High tech			
ln(all other manu. employ. in MSA)	−0.011			−0.139		
	(0.046)			(0.101)		
ln(all other employ. in MSA)		−0.052[a]			−0.127	
		(0.077)			(0.158)	
Non-diversity (two-digit) of MSA employ.			−0.674			−2.05
			(1.78)			(3.93)
ln(hours worked), ln(capital), ln(materials) ln(no. own ind. plants in county), Ind.–time fixed effects, plant/location fixed effects	yes	yes	yes	yes	yes	yes
N	4046	3283	3283	1266	1190	1190

[a] Significant at 5% level.
[b] Significant at 10% level.

In machinery in the corporate sample, overall scale measures such as total employment, manufacturing employment, and total plants for either manufacturing or overall affect productively significantly and very strongly. Results for manufacturing and overall employment are reported in columns (1) and (2) of Table 5A; scale elasticities exceed 0.10 and hence are very large. In column (3), the effect of non-diversity in overall employment is also reported. For machinery non-affiliates, while coefficients have the same signs as for the corporate plants, the coefficients are smaller and insignificant.

Does this finding indicate that there really are urbanization economies in the corporate sector of machinery, which are not found in the non-affiliate machinery sector or in high tech? The suspicion from Ciccone and Hall [9] as we will indicate below is that, at least in part, the result arises from unmeasured business service inputs, rather than externalities. However, it might not be surprising to find urbanization economies per se for these capital goods industries. Much of machinery, or capital goods production is special order. In bigger cities there may be cross-fertilization, where the influence of different industries and producers around a plant feeds into an inventive design and production process for local special order machinery. A problem is that we might expect such effects to be more important for non-affiliates than for corporate plants, and they are not. However, a possibility as to why these effects operate for corporate machinery plants and not for others might have to do with materials. Machinery corporate plants outsource more materials than non-affiliates as noted earlier, and more than high-tech plants. Urbanization economies could arise from Dixit–Stiglitz local scale effects from the overall MSA scale, and hence diversity of locally traded intermediate inputs[16] (as modeled in Fujita [15] or Venables [38]), affecting productivity of the industry sector with more materials intensive production. There are several problems with this explanation as to why these would reflect externalities. First the more relevant externality measures—the (non-)diversity measures for Jacobs economies—are never significant at the 5% level. Second, the materials coefficients are unaffected by the introduction of these urbanization measures. Third, there is some evidence of a Hicks' biased form to these urbanization scale economies in the corporate machinery sector, where they seem to be possibly capital using and materials saving.[17] For example, interacting overall MSA manufacturing scale with inputs produces a negative significant coefficient on the materials term. But that seems at odds with the idea that outsourcing will be more efficient and outsourcing expenditures will increase for firms with greater urban scale.

As suggested in Ciccone and Hall [9], measured urbanization economies could in part capture the effects of omitted inputs—greater outsourcing of **business service** inputs with greater urban scale, or greater use of purchased service inputs, which are not reported in the LDR data. The increased usage would be due to lower prices and availability of such inputs in bigger markets. In that case, part of the perceived rise in productivity would be illusionary, representing increased omitted inputs, not urbanization economies, as local scale increases. This possibility is reinforced by the fact that the so-called urbanization effects operate in the corporate sector which is much more likely to be involved in service input outsourcing (Ono [31]).

[16] That is machinery producers have as material inputs, a CES Dixit–Stiglitz specification where greater varieties of purchased inputs enhance machinery productivity.

[17] The coefficients and standard errors for (all variables in natural logarithms) labor, materials, capital, count of own industry plants, total MSA manufacturing employment, and labor, capital, and materials interacted with MSA manufacturing employment are 0.709 (0.168), 0.651 (0.106), −0.091 (0.081), 0.016 (0.016), 0.291 (0.090), −0.018 (0.014), 0.010 (0.0071), and −0.019 (0.0089).

3.4.1. The urban growth literature

The negative findings on Jacobs-diversity economies are at odds with findings in the literature (Glaeser et al. [17] and Henderson et al. [23]) examining city-industry employment growth equations. There, in the usual OLS formulation decreasing specialization (increasing metro diversity) in both high-tech and machinery facilitates employment growth. Similarly, metro area scale may enhance employment growth. Why do productivity findings differ from employment growth findings? Overall scale and diversity may positively affect location decisions through, for example, local transport cost savings from improved local upstream and downstream linkages, thus affecting local industry growth. However, that is very different from the direct productivity effects of scale and diversity externalities, which arise from, say, information spillovers.

4. Conclusions and extensions

In terms of conclusions, localization/MAR scale externalities arise from the number of local own industry plants. High-tech industries experience significant localization economies, while machinery industries do not. Externalities are quite localized, within the own county, so that there are not external benefits from plants in other counties in the MSA. It appears in the basic formulation that corporate and non-affiliate plants benefit equally from static externalities, even though corporate plants can rely on intra-firm networks across sister plants. But once we consider dynamic externalities, the result accords with intuition—non-affiliate plants benefit more from external accumulated local knowledge (or other benefits) than do corporate plants, with their reserves of firm experience. Finally, it appears that non-affiliate plants generate greater externalities than corporate plants. Corporate plants simply seem to be more walled-off from the local environment, than non-affiliates, which is the Saxenian [36] story.

Evidence of static Jacobs-diversity economies of any type does not exist for any industry. Evidence of static urbanization-scale economies appears for corporate machinery plants. However, oddly, they then do not appear for non-affiliate machinery plants where they ought to be more important. There is the concern that urbanization effects are parading as effects of omitted outsourced service inputs. Finally there is no evidence of dynamic Jacobs or urbanization economies of any type for any industry.

The results bear on two other issues in the literature. Is the degree of agglomeration of an industry related to its degree of scale economies? In Table 1 high-tech industries are more agglomerated than machinery industries. In this paper they have higher localization economies also, suggesting agglomeration and economies are related. However, the deconcentration of industries which occurred in recent years in Table 1 is not explained by changes in the degree of localization economies, which are the same over the sample period.

The literature, especially Arthur [3] and Rauch [32], suggests that mobility of industries should also be linked to the degree of scale economies and sizes of agglomerations. As scale economies rise, new locations are at an increasing disadvantage in attracting plants (and hence becoming production sites), since they offer no scale advantages, and with dynamic externalities, no accumulated localized knowledge. It seems however that other factors may dominate the determinants of the rate at which industries move across locations.

While high-tech industries have greater scale economies and a greater degree of agglomeration, they are more mobile than machinery industries. In Henderson [22], I look at industry mobility. I divide the distribution of industry shares of national employment across MSA's into 5 cells and calculate mean first passage times. Mean first passage times of moving from the lowest cell (typically a zero share), to the top two cells with the top 5 and then the next 10 percentiles of highest ranked industry-employer cities, are on average almost twice as fast in high tech. Similarly the mean first passage times of moving down from the top cell to bottom cells are much faster in high tech.

Rather than being based on magnitudes of scale economies and agglomeration sizes, the differential in mobility between high tech and machinery may be explained by aspects of machinery production, where backward and forward linkages are important. The five machinery industries relatively intensively use heavy inputs—primary iron and steel and primary non-ferrous metals, where the former is based on raw materials heavily concentrated around the Great Lakes. For the machinery industries, the ratio of these heavy inputs to output averages 0.125 (with a range for individual industries from 0.097 to 0.153); and the ratio of heavy inputs to all inputs averages 0.234 (range 0.177–0.279). For high tech, the corresponding numbers are 0.049 (range 0.016–0.071) and 0.089 (range 0.026–-0.120). Apart from agglomerating near material sources to save on transport costs with input linkages, the machinery industries may be relatively immobile because these sources are geographically fixed.

Acknowledgments

Support of the National Science Foundation (Grant No. SBR-9730142) is gratefully acknowledged. I thank Joyce Cooper for her help and Tim Dunne for advice on the use of the LRD. I thank Duncan Black, Areendam Chanda, and Yukako Ono for excellent assistance. I thank Will Strange for helpful comments on an earlier version of the paper entitled "Evidence on Scale Economies and Agglomeration," as well as participants in seminars at Washington, Harvard, Penn State, and Clark Universities. Comments by Ed Glaeser spurred me to look at endogeneity issues more carefully. I also benefited from discussions with Tom Holmes. The research in this paper was conducted while the author was a Census Bureau research associate at the Boston Research Data Center. Research results and conclusions expressed in this paper are those of the author and do not necessarily indicate a concurrence by the Bureau of the Census. This paper has been screened to insure that no confidential data are revealed.

Appendix A

A basic description of the choice of industries and the construction of estimating samples is given in the text. Here I add some details. Then I give variable definitions.

A.1. Industry choice

I use the main four high tech and five machinery good industries (where computers are classified as high tech, not machinery), in the USA. Omitted from machinery is SIC 359 which is an ill-defined residual category. Also omitted are three-digit machinery industries with small sample sizes; the largest excluded one (SIC 352) was less than 40% of the smallest included subindustry. Since I originally looked at individual industries, small samples often occupied too few locations to be useful. Second small sample sizes for any industry can present disclosure problems at the Census Bureau. The excluded high-tech industries—communications (SIC 366), missiles and space vehicles (376), search and navigation equipment (381), and measuring devices (382)—all had very small sample sizes (even more so in the non-affiliate sample).

A.2. Estimating samples

In the text, there is a fairly long description of the construction of the two basic estimating samples. Here I add a few details. I eliminate all plant-years for "administrative records," when all data other than employment and wages are imputed. I eliminate all non-administrative records, where an impute flag has been assigned by the Center for Economic Studies of the Census Bureau, based on a record-by-record assessment of when most relevant non-labor data has been imputed (due to non-reporting or reporting errors).

In general, I utilize data for a plant only for the sample years for which it remains assigned to the same industry. So if a plant appears in two Censuses but in different industries (at least one of which is one of my nine sample industries), it is excluded. An exception to this exclusion rule is for the non-affiliate sample, where in each ASM wave SIC codes are not updated from the Census in which the ASM wave is drawn. So if a plant switches industry between 1984 and 1988, it remains by default in the estimating sample for non-affiliates.

A.3. Variables

Plant output is annual production (sales adjusted for beginning and ending year inventories of finished products, work-in-progress, and resales). Inputs are total hours worked (production workers hours plus 1800 times the number of non-production workers), materials used in annual production, and beginning of year book value of machines, equipment, and buildings (where for 1987 and 1992, buildings cannot be separated out). Beginning of year book value may not be the best measure of capital stock; but using perpetual inventory methods would require plants to be surveyed in all years 1972–1992, which would reduce the sample sizes to tiny levels. Moreover, with fixed effects, changes in book values should fairly accurately measure changes in capital stock.

In the non-affiliate sample, capital stock numbers are not available in the ASM for 1988, 1989, 1993, so I assign the end of year numbers for 1987 to plants in 1988 and to those in 1989 (a different wave than for 1988); I assign end of year numbers for 1992 to plants in 1993. The 1988 and 1993 numbers are thus accurate (ignoring minor typical reporting differences between end of year t numbers and beginning of year $t+1$ numbers).

Industrial environment variables are generally as described in the text. For scale variables such as average plant employment in the industry, I exclude the own plant from the calculation. Similarly for total MSA high tech, machinery, manufacturing, and all employment, I always subtract out the own industry total for the county. Due to the complexity of repeated calculations, diversity indices are not so adjusted (but the effect on the calculated values of indices of the own industry (three-digit) in an MSA diversity index for 80 two-digit industries is essentially zero).

The count of plants includes the own plant generally (since to adjust would simply involve subtracting a constant (1)). However, in distinguishing non-affiliate and corporate plant counts, I subtract one from the non-affiliate count if the own plant is a non-affiliate and similarly for corporate plants (since we want the relevant count outside the own plant). For variables where counts could be zero (e.g., births), I add a constant (1) to all counts, so the natural logarithm is defined for the zero case. (Experiments to have separate slope and dummy were not fruitful.)

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