NBER WORKING PAPER SERIES

MARSHALL'S SCALE ECONOMIES

Vernon Henderson

Working Paper 7358
http://www.nber.org/papers/w7358

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 1999

Marshall's Economies
Vernon Henderson
NBER Working Paper No. 7358
September 1999
JEL No. R000, O300, L600, S620

ABSTRACT

This paper estimates the nature and magnitude of the local externalities from own industry scale, as envisioned by Marshall. Census panel data on individual plants in high-tech and machinery industries across up to 487 countries are utilized, to quantify the direct effects of local external environment on plant productivity. Careful attention is paid to endogeneity issues in estimation. Magnitudes of scale externalities for corporate versus single plant firms are estimated and the sources of externalities (employment, numbers of plants, numbers of births, etc.) and extent (within the county versus extending to the rest of the MSA) are investigated. The paper asks in addition whether externalities are static or dynamic, a key issue in thinking about urban growth and industrial mobility; and whether they are dependent just on local own industry activity or also on overall local urban scale and/or diversity, a key issue in analyzing industrial composition and development of cities. The paper relates the findings on externalities for different industries to the extent of agglomeration and the degree of mobility of those industries across cities.

Vernon Henderson
Department of Economics
Brown University
Box B
Providence, RI 02912
and NBER
j_henderson@brown.edu

Marshall's Scale Economies[1]

Vernon Henderson

Brown University

Draft: September, 1999

This paper estimates the extent and nature of local external economies of scale for key high tech and traditional machinery industries. Unresolved issues in the literature concern (1) whether urban scale economies derive primarily from local own industry activity, as envisioned by Marshall (1890), or from overall scale and diversity of all local economic activities, (2) whether scale economies are primarily static or dynamic, (3) what precise attributes of the local environment generate externalities (which may relate to the micro foundations of scale externalities) and (4) what exactly are the magnitudes involved. We expect the answers to vary by the type of industry and its stage of development. As emphasized in the literature (Lucas 1988, Glaeser, Kallal, Scheinkman, and Shleifer 1992, Black and Henderson, 1999), answers to these issues are critical to understanding the nature of urban development – sources of urban growth, extent of spatial agglomeration of different industries, product cycles, industrial mobility across cities, and industrial composition of different cities. Using plant level data in a panel framework, the intention is to help resolve some of these issues, by examining how changes in aspects of the local industrial environment induce changes in plant productivity, for different industries.

The scale economy estimates for high tech and machinery industries are also related

[1]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 research 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 University, Harvard and Clark University. 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 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.

to two issues concerning urban development, both involving the extent of agglomeration of economic activity. Almost all manufacturing industries are agglomerated, with many cities having absolutely no employment in any specific industry and a few having high concentrations. The first issue is whether industries which have greater degrees of own industry scale externalities are more agglomerated than others. Alternatively, for example, agglomeration could be greater for industries which are drawn to large employment centers either to exploit backward and forward transport linkages with local final or intermediate good buyers and sellers (Krugman, Fujita and Venables, 1999) or to enjoy generalized urbanization economies (Jacobs, 1969). The second issue is whether the most agglomerated industries are the least mobile, or whether other factors seem to drive mobility. To examine these issues for high tech and machinery industries in addition to estimating scale economies, I will need to characterize the extent of agglomeration, the extent of industrial mobility, and changes in both over time.

Issues and the Literature

In considering the dynamics of agglomeration, the literature asks to what extent existing agglomerations are immutable, locked-in by own industry scale externalities. The question itself presupposes own industry scale economies are the basis for agglomeration, a persumption in the urban literature (Henderson, 1974) which this paper will examine, with findings contradicting some of my own priors. Using a firm-location matching model of the evolution of agglomerations, Arthur (1990) predicts that as an industry grows nationally, local relative employment fluctuations for the industry will dampen, and locational patterns as measured by local shares of national employment will become fixed. Locations without an industry can't attract new plants because they offer no scale benefits. In opposition to this notion is empirical work of Davis, Haltiwanger and Schuh (1996) who postulate that locations experience on-going allocative shocks, which effectively maintain turbulence in the system and induce shifts in locational patterns. I test whether fluctuations in city-industry employment shares tend to dampen over time, by examining transition processes. I estimate mobility rates for different industries to see if those subject to greater scale externalities are slower to shift locations. I examine patterns of agglomeration to see if more agglomerated industries have greater scale economies and to see in what types of locations industries agglomerate.

These examinations also allow us to determine whether agglomeration tendencies have changed over the last thirty years. Are industries deconcentrating and spatially spreading with declines in transport and telecommunication costs; or are they further concentrating with heightened scale effects and role of localized spillovers through face-to-face interaction? In fact, have the magnitudes of scale externalities changed over time? Answers to these questions will help us understand the changing USA economic geography.

Turning to estimation of scale externalities, a number of productivity studies (Ciccone and Hall (1996), Henderson (1986), Nakamura (1985), and Sveikauskas (1975)) have attempted to sort out the nature of externalities. The conceptual issue concerns whom plants learn from, when externalities involve information spillovers across plants and within labor markets, facilitated by socialization, business interaction with suppliers and the exchange of employees in local labor markets. Do plants learn primarily from other local plants in the same industry? Such externalities of Marshall are called localization economies, or sometimes MAR [Marshall, Arrow, Romer] externalities in a dynamic context. Do plants learn instead from local plants outside their own industry through cross fertilization? These externalities are called urbanization economies, or sometimes in a dynamic form, Jacobs (1969) externalities. If the latter, is overall diversity important, or are specific inter-industry networks important? So, for example, do high-tech industries benefit from being in large cities per se, rather than environments with a diversity of other high-tech industries.

The form of externalities underlies aspects of 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 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, 1998). 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 manufacturers and financial, business, research and development and management services tend to be found disproportionately in larger metro areas. If the nature of externalities changes over time with product development, we may have a product cycle where activity is initially found in large diverse metro areas but then decentralizes to smaller more specialized metro areas.

The data allow me to analyze key details concerning these issues, never examined before. For example, do externalities apply more to single-plant firms who get information from external sources, than to corporate multi-plant firms who may exploit an internal-firm information network; or do corporate plants benefit equally from improvements in the local environment? Do plants learn from existing more mature plants; or does learning depend on an infusion of newborns, bringing new ideas and experimentation? Does the external learning, or absorption of spillovers by plants decline with plant age? As a final example, are externalities very localized, say, emanating just from plants in the own county, or also from nearby counties in the same metro area?

Another key issue concerns whether externalities are static or dynamic. Dynamic externalities are the underpinnings of endogenous growth models (Romer 1993), including those in urban settings, (Eaton and Eckstein (1997), Black and Henderson (1999)). In an urban context, each locality builds up a stock of local "trade secrets" dependent on past industrial activity, a local public good accessed by locating in the city (Glaeser, Klallal, Scheinkman, and Shleifer (1992)). Dynamic externalities have strong implications for industrial mobility (Rauch 1993). New locations have trouble attracting industries subject to dynamic externalities, because they can't offer a built-up stock of trade secrets and because accumulating an attractive stock involves costly efficiency loses for initial locators. Location patterns may be subject to strong histories.

So far, no productivity studies have investigated dynamic externalities. Studies investigating their existence (Glaeser et al (1992) and Henderson, Kuncoro and Turner (1995)) examine employment growth patterns between two time periods, asserting that, if the level of employment in an industry today is correlated with local own industry employment 15 or 30 years ago, that is evidence of dynamic externalities. There are two problems with this inference. First is conceptual. The typical estimating equation contains two key measures — base period own industry employment to control for "mean reversion," induced for example, by Davis et al (1996) allocative shocks, and a base period own industry concentration measure to represent localization externalities. The mean reversion control and the concentration measure are so closely related, it is hard to distinguish effects. Moreover, the mean reversion process and how, say, externalities inhibit mean reversion or perhaps dampen allocative shocks have never been explicitly modeled. That makes the specification and interpretation of employment growth equations, at best, tentative.

Abstracting from the first problem, the second concerns whether a partial correlation between present employment levels and past concentration implies externalities. Rather the correlation can arise from a "fixed effect" in estimation, representing unmeasured time invariant locational attributes such as resource endowments, local culture affecting the legal, tax and institutional environment, and access to national and international markets. Current industrial location patterns may be related to historical ones, not because of dynamic externalities, but because of persistent local comparative advantage. The final section of the paper will also show what happens when fixed effects methods are applied to the Glaeser et al. and Henderson et al. formulations. In this paper, we avoid the mean reversion and fixed effect problems, by directly examining the effects on changes in plant productivity of changes in past local industrial environments.

The paper utilizes plant and city-industry level data from the Census of Manufacturers for 1963-1992, and information from the Annual Survey of Manufacturers for certain non-Census years. The paper is organized as follows. For four high-tech and five standard machinery industries, for 1963-1992, I first examine the extent of agglomeration and the evolution and mobility of these industries across metropolitan areas. Then I examine determinants of productivity for these industries from 1972 on, for plants located in 742 urban counties and 317 metropolitan areas. The effects of various contemporaneous and historical attributes of scale and diversity of the local industrial environment at the county and metro level on plant productivity are measured. Finally I link patterns of agglomeration and mobility to the scale economy results; and I examine aspects of location patterns.

Preview of Key Findings

To aid the reader, I preview key findings of the paper before going into detailed analysis. For manufacturing activities, the paper presents evidence that scale externalities derive from own industry (localization-MAR) externalities, and are very local. Specifically they derive from the numbers of own industry plants in the own county, as opposed to, say, an industry total employment measure or to activity in surrounding counties in the same MSA. I will argue that the result is consistent with the micro foundations of scale externalities being localized information spillovers across plants, rather than scale economies in labor markets. Single plant firms and corporate plants benefit equally from static externalities. However, in industries where dynamic externalities exist, single plant firms seem to derive greater benefits from dynamic externalities. Corporate plants may be able to use their own internal information networks to substitute to some extent for the stocks of local knowledge spillovers that single plant firms rely on. Finally, in some cases, dynamic externalities may derive more from past births of plants, as opposed numbers of pre-existing plants. The idea that externalities might derive from births of plants has implications for mobility and agglomeration of industries that have never been modelled.

Manufacturing activities don't seem to benefit from Jacobs-urbanization externalities. A suggestion is that the search for such externalities might more productively focus on the service and R&D sectors, which are the activities found disproportionately in large diverse metro areas.

The extent of spatial agglomeration of individual industries seems to be closely related to the extent of scale economies for the industries. However, the decline in industrial geographic concentrations over time seems unrelated to scale externality magnitudes which have not changed. I also find that even industries without scale economies agglomerate to some considerable extent, perhaps to trade (backward and forward linkages) with those industries which do experience scale economies. The hierarchy of agglomeration – first the industries with scale economies and then, to a lesser extent, those that serve them – has yet to be modelled in the literature.

While agglomeration and scale economies are linked, the degree of mobility of industries seems to be dominated by factors other than scale economy magnitudes. Such factors include on-going access to raw materials.

1. INDUSTRIAL AGGLOMERATION

For this paper, I assembled data on all three-digit machinery industries (except the ill-defined SIC 359) and eight three-digit high-tech industries. Industries with small sample sizes are excluded from analyses. The estimating sample of the largest excluded industry (SIC 352) was less than 40% of the smallest included industry and for some specifications was too small to utilize; excluded high-tech industries have tiny samples. The four high-tech industries that have large national employment are computers (SIC 357), electronic components (367), aircraft (372) and medical instruments (384). As a comparison group, I use the five large employment machinery industries – construction (353), metal working (354), special industrial (355), general industrial (356) and refrigeration (358). Industries are defined consistently over time. The data in this section are from the 1963, 67, 72, 77, 82, 87, and 92 Census of Manufacturers, based on plant level data in the Longitudinal Research Data [LRD] base of the Census Bureau. These data are aggregated up to the metropolitan and national level to examine evolving patterns of industrial agglomeration across 317 metropolitan areas for 1963-92 in five-year time periods.

This section will show that high-tech industries are distinctly more agglomerated than machinery industries. By some measures, machinery industries also deconcentrated further during the past thirty years. However, surprisingly, high-tech industries are the most mobile.

Evolving Extent of Agglomeration

Measures of the extent of agglomeration of an industry typically focus on the upper tail of the distribution — the extent to which national employment is concentrated in the very largest employer-cities. I start with these measures, but will also show that these measures miss a key aspect of changing patterns of agglomeration. Table 1 describes high-end industrial agglomeration and its change from 1963 to 1992. Part (A) is for the high-tech industries and part (B) for machinery industries. While the table compares 1963 to 1992, the deconcentration and reconcentration tendencies enumerated in the table occur throughout the time period 1963-92. The table suggests high-tech industries are more concentrated than machinery industries, with the difference increasing over time.

Column 1 of parts (A) and (B) measures primacy — the share of the largest city employer in national industry employment. Also indicated are the absolute city-industry employment and the identity of the city. In 1992, average primacy in high-tech is 12%, compared to 5.5% for machinery. Average primacy declines in machinery from 1963 to 1992 but is unchanged in high tech. In machinery, only metal working has a high degree of primacy;

Table 1. Agglomeration Patterns

Industry	(1) Primacy: share of largest center in nat. employ. (Abs. size, 1000's) 1963	1992	(2) Concentration: g(t) 1963	1992	(3) National employ (1000's) 1963	1992
A) High Tech						
Average	**12 (31)**	**12 (51)**	**.026**	**.028**	**239**	**399**
computers (357)	12% (15) Dayton	17% (44) San Jose	.031	.038	130	251
electronic components (367)	7.1 (20) L.A.	11 (55) San Jose	.010	.016	287	531
aircraft (372)	18 (84) L.A.	16 (91) Seattle	.047	.050	480	548
instruments (384)	9.9 (5.6) Chicago	5.0 (13) Boston	.015	.0063	57	264

Table 1, continued

B) Machinery

Average	**8.1 (18)**	**6.2 (14)**	**.013**	**.0071**	**200**	**203**
construction (385)	15 (17) Peoria	6.6 (12) Houston	.016	.0095	210	179
metal working (354)	8.1 (41) Detroit	12 (31) Detroit	.025	.015	269	255
special industrial (355)	6.6 (13) Chicago	3.8 (6.2) Boston	.0069	.0036	191	161
gen. industrial (356)	5.9 (12) Chicago	5.0 (12) Chicago	.0071	.0024	208	244
refrigeration (357)	4.8 (5.9) Chicago	3.5 (6.3) Niagara Falls	.0082	.0050	123	178

and it, alone among machinery industries, could be classified as high tech (Markusen, Hall, and Glasmeier, 1980).

Column 2 of parts (A) and (B) gives an adjusted Ellison-Glaeser (1997), or normalized Hirschman-Herfindahl index of concentration for 1963 and for 1992. The index $g_i(t)$ for industry i in time t is

$$g_i(t) = \sum_{j=1}^{317} \left(\frac{E_{ij}(t)}{E_i(t)} - \frac{E_j(t)}{E_n(t)} \right)^2 \tag{1}$$

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 g 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.

For the concentration measure, high-tech industries in 1992 average .028; while the machinery ones average only .0071. Moreover in high-tech industries, except instruments, primacy or concentration increases from 1963 to 1992; while in all machinery industries primacy and concentration declines. Note the primacy and concentration results generally correspond. Because deviations in (1) are squared, the three-four largest cities for an industry drive the concentration index and changes in it. A question for the paper is whether the greater high end concentration of high-tech industries is associated with greater scale externalities, compared to machinery.

These usual measures of concentration only tell us about the extreme right of the employment distribution. What the primacy or Ellison-Glaeser indices do not tell us is the thickening in all industries of the middle portions of the employment distribution that has occurred over the last 30 years and the decline in number of zero employment cities. In Figures 1a and 1b, I plot the truncated distributions of the logarithm of shares of national industry employment across cities for each industry for 1963 versus 1992. Each city's employment is normalized by national employment to get its share of national employment for that year, so the focus is on the shape of the employment distribution (not absolute shifts left and right with changes in national employment). The truncated distribution is for the logarithm of shares.[1]

In all industries, there has been a shift into middle employment cities over the last thirty years (which occurs almost continuously over the time period). In machinery the distributions of employment shares have become more peaked in the middle with the highest point at about a share of .0025 (log share = -6), which is typically 500 local employees. In high-tech, between 1963 and 1992, we go from having no middle peak in 1963 to having one in 1992, typically around 1000 local employees. As can be seen for most industries, having more middle (as opposed to minimal) employment **share** cities means two things here. First is that there are fewer zero (and minimal) employment cities. In Table 2 the number of zero employment cities in high-tech falls from an average of 173 to 90 and in machinery from 106 to 51, out of 317 metro areas. The 67% growth in national employment in high-tech (see column 3, Table 1) could explain the 58% increase in positive employment high-tech cities. However in machinery, national employment is unchanged, and the number of positive employment cities still increases by 26%.

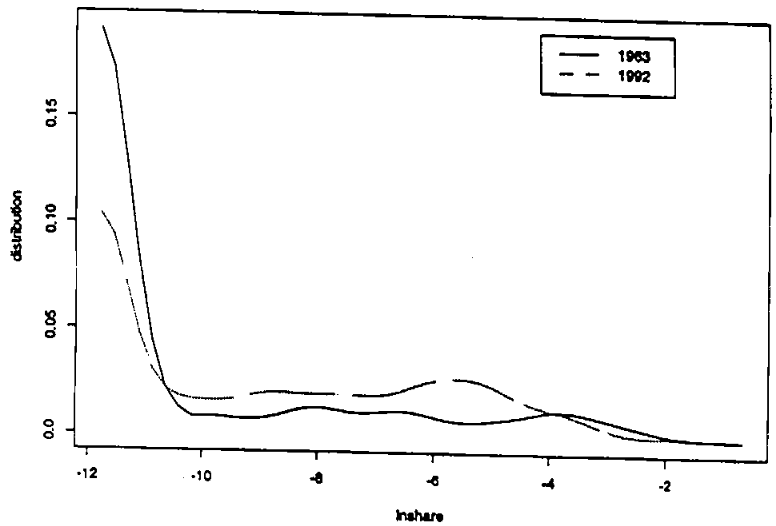
For the zero employment MSA's in 1963 to gain employment some shrinkage in the outer tail of high (but not necessarily highest) employment cities (so as to populate these middle employment cities) is required. Table 2 illustrates this. In <u>all</u> industries, the national shares of employment in cities ranked 4-32 (the top 10 percentiles of city-industry employers excluding the top 3) fall from 1963-1992, while the national shares of cities below the top 10

[1]Zero employment cities are assigned employments of one or log shares of about -12. The estimation is done in S-plus, adjusted for trunction, using a kernel estimator.

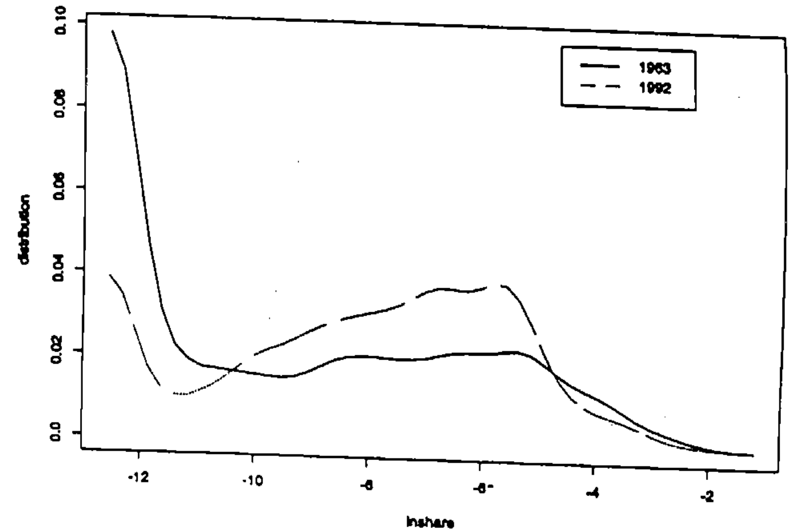
Figure 1. Distribution of MSA Shares of National Industry Employment

(a) High-Tech

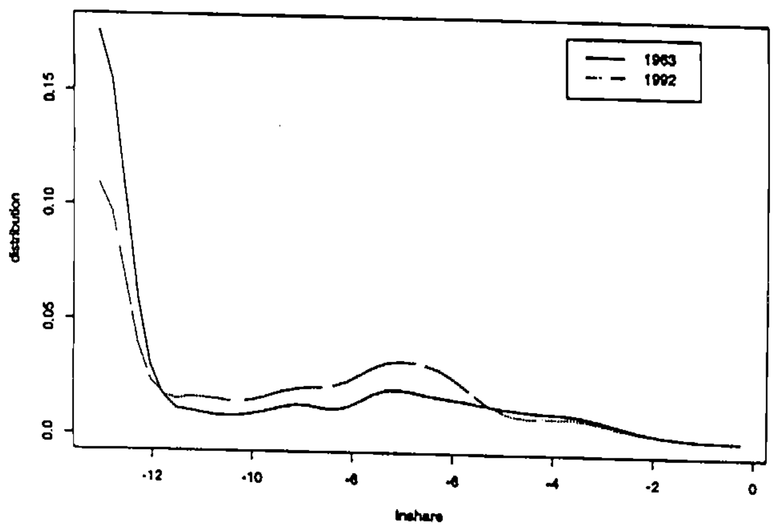
MSA Share of Employment: Industry 357



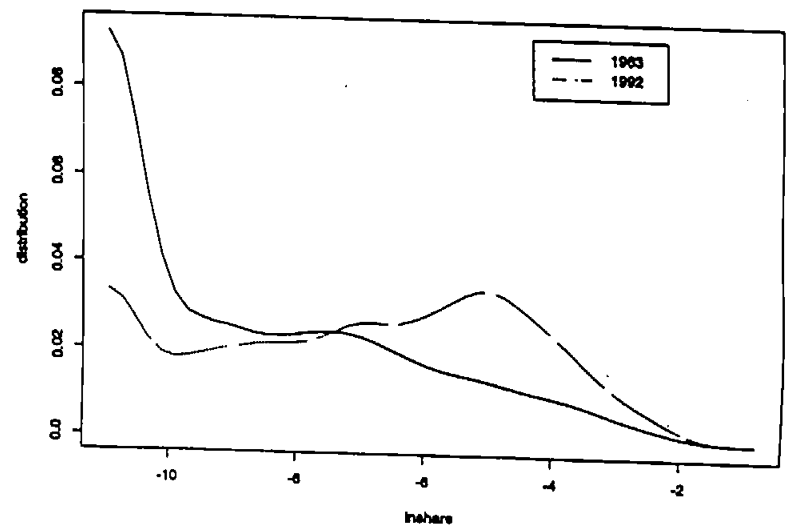
MSA Share of Employment: Industry 367



MSA Share of Employment: Industry 372

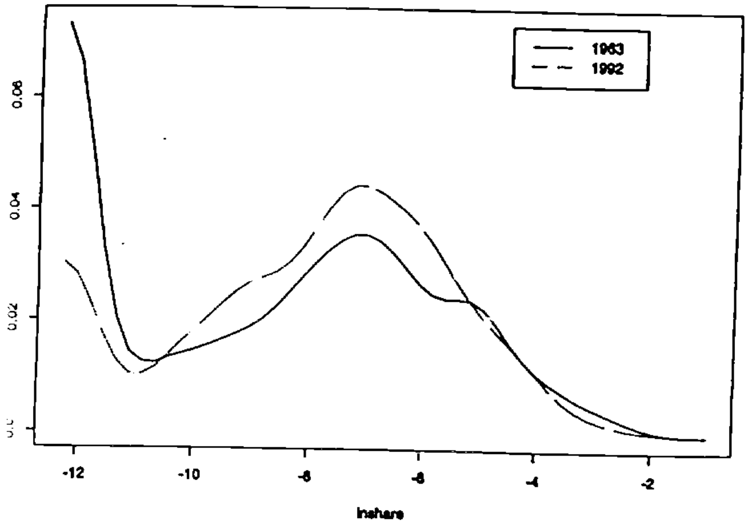


MSA Share of Employment: Industry 384

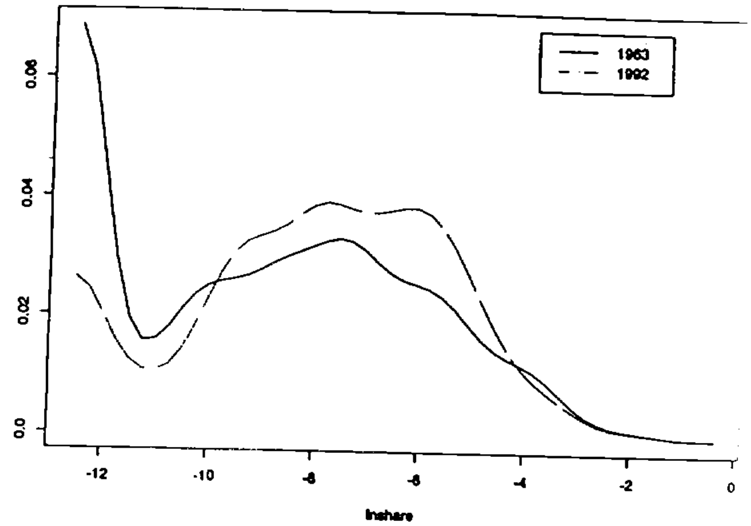


(b) Machinery

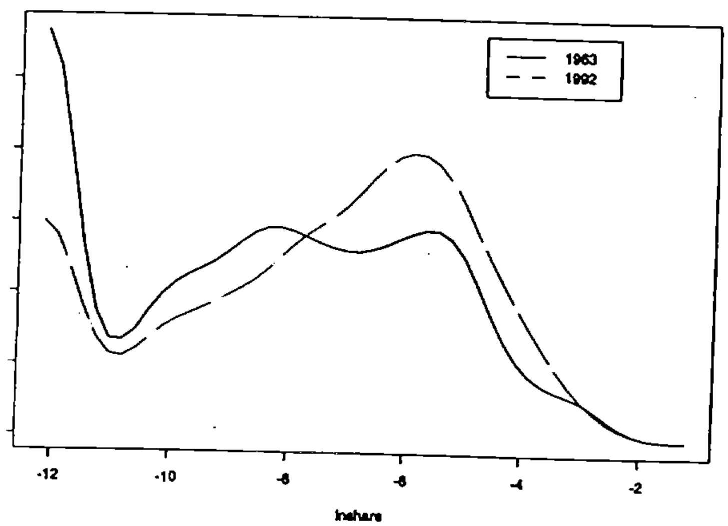
MSA Share of Employment: Industry 353



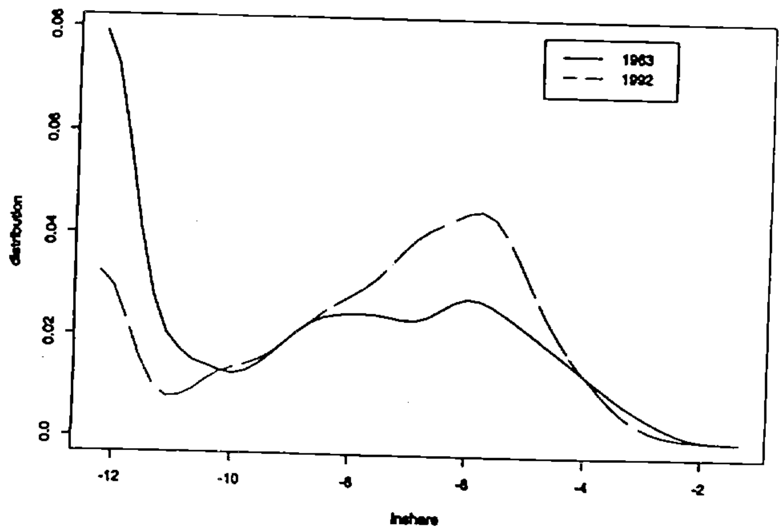
MSA Share of Employment: Industry 354



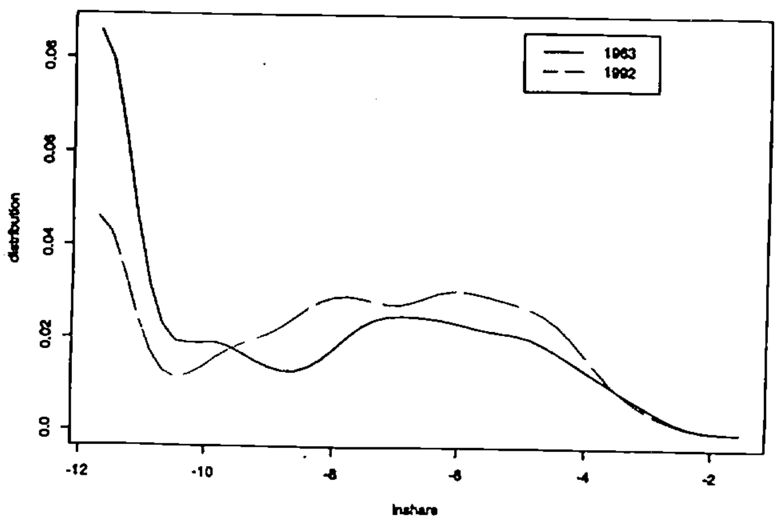
MSA Share of Employment: Industry 355



MSA Share of Employment: Industry 356



MSA Share of Employment: Industry 358



percentiles ("The Rest") increase. However in some industries, the shares of the largest city-employer (computers, electronic components, and metal working in Table 1) or the largest three city-employers (computers and aircraft in Table 2) increase. These 1-3 top end cities drive the change in the Ellison-Glaeser index in Table 1.

In summary, while high-tech industries have become somewhat more concentrated at the extreme high end of the distributions, in all industries employment has spread out into middle employment centers. That is accompanied by a decline in the number of small or zero employment centers, as well as decline in employees from somewhat higher employment centers. A question is whether these changes are somehow related to changes in scale externalities. An alternative is that transport costs have declined. Declines in transport costs can have opposing effects. Agglomeration sizes (of bigger centers) can increase since producers don't need to spread out to save on transport costs of serving regional markets. But producers can also locate remotely in low cost towns (and off-shore) and more cheaply ship to markets.

Mobility.

There are various ways one could look at mobility of industries across locations. Here I use mean first passage times. I estimate how fast cities transit across cells of a discrete size distribution for each industry of city shares of national industry employment. I characterize distributions using five cells, with relative upper cut-off points chosen so cell sizes are 55, 15, 15, 10, and 5 percent of all cities. Upper cut-off points in 1963 average .00014, .00056, .0029, .015, and open. So 55% of all cities in 1963 each have .014% or less of national employment of a typical industry, while 16 cities, or 5% of cities in 1963 each have over 1.5% of national employment. Results are not qualitatively different for other reasonable cell divisions. For computers and aircraft, too many cities have zero employment to distinguish the bottom two cells; so I combine them to have cell sizes of 70, 15, 10, and 5 for a four-cell discrete distribution. This idea is to compare mobility in the rankings of cities – for example how quickly does a city in the bottom 55 percentiles of city employments move to the top 5 percentiles, for different industries.

I assume distributions evolve according to a homogeneous stationary first-order Markov process, testing for stationarity. The Markov process captures the Davis et al. (1996) notion of on-going turbulence in allocative processes. Sources of non-homogeneity, such as geographic and historical features are estimated in Beardsell and Henderson (1999) for computers; and are beyond the scope of this paper. Based on transitions for 1963-67, 67-72, ..., 87-92, I calculate an overall transition matrix, M, where the maximum likelihood estimates of the transition probabilities, p_{ij}, are the total number of transitions to cell j from the total number of entries in cell i over all years. To calculate how fast cities move across cells, or states of the distribution, I calculate mean first passage times. If ϕ_{jk}^t is the probability that a city in state cell j for an industry next visits state k at a time t (1/2 decades) later, then the mean first passage time (in 1/2 decades in the data), τ_{jk}, from j to k is

$$\tau_{jk} = \sum_{t=1}^{\infty} t\phi_{jk}^t. \tag{2}$$

The ϕ_{jk}^t are calculated recursively from the transition matrix coefficients.[2]

In studying mobility, what are we looking for? First according to Arthur (1990), for at least growing high-tech industries, mobility rates should dampen over time. For all industries,

[2] If $[M^t]_{jk}$ is the j,k element of transition matrix raised to the power t, Markov chain theory tells us that

$$[M^t]_{jk} = \sum_{s=0}^{t} \phi_{jk}^s [M^{t-s}]_{kk} \;\forall\; t \geq 1.$$

Given $\phi_{jk}' = [M_{jk}]$ and $\phi_{jk}^0 = 0$, we can recursively define ϕ_{jk}^t as (Karlin and Taylor (1975))

$$\phi_{jk}^t = [M^t]_{jk} - \sum_{s=0}^{t-1} \phi_{jk}^s [M^{t-s}]_{kk} \;\forall t \geq 1. \tag{3}$$

This allows us to calculate the τ_{jk}. At $t = 1000$, the calculation converges for all industries, (or $\sum_{t=0}^{1000} \phi_{jk}^t \rightarrow 1$ and $t\phi_{jk}^t \rightarrow 0$ at $t = 1000$).

the stationarity of the transition matrices is never close to being rejected.[3] There seems to be no consistent change in the transition process for 1963-1992, regardless of whether industries grow, decline or stagnate in the 1963-92 time period. Note also in Table 1, for most industries, including machinery ones, even the identity of the primate city-industry changes from 1963 to 1992.

Given a stationary transition process for all industries, how do relative mobility rates compare? In Table 3(a), we report the average of mean first passage times for machinery industries. The off-diagonal elements are mean first passage times; the diagonals are mean first return times (including staying in the own state). For a "typical" machinery industry, for a city starting in state/cell 1, the expected time for it to first visit state 5 is 211 half decades; going in reverse it is 30 years. The slow times to move up and much quicker times to move down simply reflect the asymmetry in cell sizes, starting with 55% for the bottom cell and declining to 5% for the top. By construction, cities are slow to join the top 5%, given newcomers are drawn from a large group; but cities are quick to leave the top cell, given exiters are drawn from a small group.

What is of interest are the inter-industry comparisons. In part (b) of Table 3, I present the numbers for instruments and electronic components which are similar to each other; and in part (c) I give the four state distribution numbers for computers and aircraft. Excluding aircraft, high-tech industries have much quicker times to move up and down. The times to move from states 1 or 2 to 4 or 5 for electronic components and instruments are much quicker than the fastest machinery industry. The same statement applies for the reverse – going from state 4 or 5 to 1 or 2. In comparing computers to other industries, one could

---

[3]The χ^2 statistic for the test is

$$-2\ log\ [\Pi_t \Pi_j \Pi_k\ \left\{\frac{\hat{p}_{jk}}{\tilde{p}_{jk}(t)}\right\}^{m_{jk}(t)}]$$

with $(T-1)K(K-1)$ degrees of freedom. $\hat{p}_{jk}$ is the stationary estimate, $\tilde{p}_{jk}(t)$ the decade by decade estimate, $m_{jk}(t)$ the number of cities moving from j to k in t, T the total number of years and K the number of cells.

Table 3. Mean First Passage Times

(a) Averaged Across Machinery Industries
[Lowest and highest values in brackets. Unless noted otherwise in parentheses, SIC for the lowest, is always construction (SIC 353); highest is always metal working (SIC 354))]

		cell k				
		1	2	3	4	5
cell j	1	1.8	13 [12-16]	31 [24-46]	74 [56-113]	211 [167 (358)-277]
	2	8.2 [7-11]	6.7	22 [16-34]	66 [50-100]	203 [159 (358)-267]
	3	16 [12-25]	13 [11-18]	6.6	46 [37-67]	185 [148 (358)-240]
	4	24 [17-37]	21 [17-29]	11 [8.5 (355)-15]	9.9	145 [105 (358)-185]
	5	30 [27-48]	29 [25 (358)-41]	19 [15 (358)-27]	9.0 [5.6 (358)-12]	18.6

(b) Electronic Components (and Instruments)

	1	2	3	4	5
1	1.8 (1.8)	11 (13)	24 (22)	44 (43)	123 (158)
2	5.4 (6.8)	6.7 (6.7)	19 (15)	40 (38)	119 (153)
3	12 (11)	11 (9.4)	6.6 (6.6)	25 (29)	104 (143)
4	16 (15)	15 (14)	6.8 (7.4)	9.9 (9.9)	83 (117)
5	20 (21)	19 (20)	10 (13)	8.2 (8.3)	20 (20)

(c) Times for Computers (and Aircraft)

	1	2	3	4
1	1.4 (1.4)	15 (17)	32 (43)	108 (257)
2	5.4 (5.9)	6.6 (6.6)	23 (33)	99 (248)
3	8.2 (11)	7.4 (8.7)	9.9 (9.9)	83 (220)
4	12 (25)	12 (22)	8.7 (14)	20 (17)

compare the time for moving from state 1 to 4 of 108 1/2 decades for computers to the time to move from state 2 to 5 for machinery (where the quickest industry takes 159 1/2 decades). Aircraft is the only relatively slow moving high-tech industry. It is closely linked to government with about 35% of sales to government, unlike any of the other industries (all under 10%), which may help explain its limited mobility.

In summary, we have the following. High-tech industries are more concentrated than machinery industries. Over time high-tech industries have retained their highest end concentration, while machinery industries have spread. Given high-tech industries are more concentrated relatively and absolutely, we might expect them to have higher degrees of scale economies and to be less mobile. However excluding aircraft, high-tech industries seem more mobile, or less anchored than machinery industries. The question we will return to in section 3 concerns why? High-tech industries have grown quickly compared to the stagnant machinery industries, which in itself suggests mobility. However, given mobility is over **relative** size distributions and given transition processes are stationary, national employment growth differences shouldn't affect mobility calculations. Also, high-tech industries on average have larger plant sizes, another potential source of immobility.

2. MEASURING EXTERNALITIES

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 industrial environment. Based on a first-order Taylor series expansion (in logs) of a general production function, output of plant k in MSA/county j at time t, $y_k(t)$, is hypothesized to be

$$log\ y_k(t) = \alpha\ log\ X_k(t) + \sum_{\ell=0}^{2} B_\ell\ log\ E_j(t-\ell) + \delta(t) + f_{kj} + \epsilon_{kj}(t) \tag{4}$$

I will also look at results for second-order (or translog) specifications and for TFP specifications of plant internal technology. In (4), $log\ X_k(t)$ is a vector of plant inputs, $log\ E_j(t-\ell)$

a vector of industrial environment variables in $t - \ell$, $\delta(t)$ a time fixed effect, f_{kj} a plant/location fixed effect, and $\epsilon_{kj}(t)$ the contemporaneous error term. Equation (4) will be estimated by panel methods, so inferences about industrial environment variables will be based on how **changes** in a plant's environment affect productivity. Also the issue of exogeneity of RHS variables to the $\epsilon_{kj}(t)$ will receive considerable attention.

The plant's own inputs are labor, capital, and materials. Among Ciccone and Hall's (1996) objections to a form such as (4) is that plant purchases of service (versus material) inputs are not recorded in Census data.[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. Then we might attribute the output increase to changes in Jacobs/urbanization diversity measures of externalities, when in fact no spillovers are involved. I will keep this in mind when interpreting results.

In equation (4), the $log\ E_{kj}(t-\ell)$ 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 $\ell = 0$, or $log\ E_{kj}(t)$; and I examine dynamic externalities for $\ell = 1$ and 2, or $log\ E_{kj}(t-1)$, and $log\ E_{kj}(t-2)$, where time intervals are five years. So I am asking if the local industrial environments from five or ten years ago affect productivity today.

[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 service data to the service input problem. Second, they object to (4) 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 aggregate data, under the CRS assumptions permitting aggregation, equation (4) remains valid. Double counting is obviously an issue for income accounting, but not in specifying production function forms.

In terms of urbanization/Jacobs economies, I experiment with both level and diversity measures, at the MSA level (consistent with Jacobs' (1969) notions). The log level measures describe local scale outside the own industry of total private employment, total manufacturing employment and employment in related industrial activities as described momentarily. The diversity measures cover the same activities, but focus on local diversity rather than scale of such activities. The general diversity measure is related to the Ellison-Glaeser (1997) index in (1), but covers a different dimension. The index measures lack of diversity or specialization. Specifically, for MSA_j in time t specialization/lack of diversity in an industrial activity is

$$d_j(t) = \sum_i \left(\frac{E_{ij}(t)}{E_j(t)} - \frac{E_i(t)}{E(t)} \right)^2 . \tag{5}$$

E_{ij} is employment in industry i in city j, $E_j(t) \equiv \sum_i E_{ij}(t)$ is total employment in city j 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. $d_j(t)$ 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 city j's shares over all industries mimic national shares it is perfectly diverse; and $d_j(t) = 0$. As city j's shares start to deviate from national shares $d_j(t)$ starts to rise. At the limit $d_j(t) \to 2$, 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. The Jacobs hypothesis is that as $d_j(t)$ rises, plant productivity declines.

In defining the relevant i, I experiment with five measures: (1) overall manufacturing employment for 20 two-digit manufacturing industries (the relevant i); (2) overall private employment (80 two-digit industries); (3) for machinery industries, three-digit level employment within SIC 3500; and (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 (376), search and navigation equipment

(381), measuring devices (382), and medical instrument (384) and (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).

Estimation Issues

In equation (4), the time fixed effects, $\delta(t)$, 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 deflaters and the extent of national productivity change. That's a topic beyond the scope of this paper. The f_{kj} represent time invariant plant and/or locational 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 can't disentangle plant and location fixed effects, although I discuss the issue more in section 3. The f_{kj} will influence the $log\ E_j(t-\ell)$ and $log\ X_k(t)$, biasing OLS estimates. Accordingly I estimate equation (5) for unbalanced panels of plants across counties and MSA's by standard fixed effects methods. Doing so raises three key issues.

First concerns the sample of plants, where I require each plant to appear in at least two Censuses. Until 1987, plants in those Census years must also be in the ASM for the same years to have non-imputed data on key variables. Moreover beyond 1987, most plants which survive a general filter for imputed data in a Census year (see below) are in the ASM for that year anyway. The ASM plants in one Census are in a different ASM wave from those in another Census (where each five-year wave of an ASM runs from a Census year plus two to the next Census year plus one). In the construction of ASM samples there is weighting where large corporate plants generally appear in each wave and small single plant-firms generally are not chosen in two consecutive waves. Thus my sample is weighted towards corporate plants, a sample for which externalities could be less relevant. To test for this, I also draw a sample of single-plant firms in the ASM in non-Census years, picking plants from the first and last year of each wave and linking their productivity to industrial environments in the immediately prior Census. Details are given in the data subsection below.

The second issue is that use of fixed effect methods requires sufficient variation in industrial environment variables, to be able to make inferences about effects of changes in the environment on productivity. If we have annual data, the variation in diversity 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 $(|d_j(t) - d_j(t-1)|/d_j(t))$ for any diversity measure always exceeds 15% between any five-year time periods in all samples.

The final and critical issue concerns the fixed effects assumption that the $log\ X_k(t)$ and $log\ E_j(t-\ell)$ are strictly exogenous to the $\epsilon_{kj}(t)$. That assumption begs the question of why $log\ E_j(t)$ measures, such as number of local own-industry plants, vary over time (if not in response to $\epsilon_{kj}(t)$). I assume both the $log\ E_j(t)$ and $X_k(t)$ vary in response to changes in local factor prices or regional market sizes, making location j a better or worse place in which to locate. I assume the contemporaneous shocks affecting plant productivity are independent of these general price and market size changes, which derive from regional and national general equilibrium adjustments to macro shocks and changes in incomes and demographics. Also in equation (4), in terms of $X_k(t)$, capital stock is beginning of year so it and arguably labor and materials (chosen in t before revelation of $\epsilon_{kj}(t)$) are exogenous to the $\epsilon_{kj}(t)$.[5]

The potential problem is that there may be local shocks, such as provision of MSA infrastructure or upgrading in quality of the local labor force, that may affect both plant productivity and the local (county) industrial environment. I conduct three experiments to

[5]However, if annual data were used, it would be less clear that the $X_k(t)$ are also exogenous to the $\epsilon_{kj}(t-1)$ 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 to assume that there is no effective impact of a shock from five years ago on inputs today.

test this possibility. None of them suggest a weakening of results on localization economies. First I re-estimated the model limiting the sample to multi-county MSA's and add in MSA-time fixed effects. This controls for contemporaneous MSA (but not county) shocks. It seriously impinges on efficiency since identification is now based only on time variation of within MSA county differences in environments. Results are footnoted. To more generally deal with endogeneity of all RHS variables to the $\epsilon_{kj}(t)$, I try 2SLS estimation. For 2SLS in a panel, instrumentation requires all instruments be **strictly exogenous** to all $\epsilon_{kj}(t)$. The exogenous instruments I had were sufficiently uncorrelated with plant inputs to be useful. A result from such 2SLS work is to raise externality results to truly unbelievable magnitudes. So for 2SLS, I restrict the examination to just TFP equations (to remove the log $X_k(t)$ as RHS variables). Instruments such as market potential of the MSA and county air quality attainment status are used to deal with possible endogeneity of $logE_j(t-\ell)$ variables to $\epsilon_{kj}(t)$. Again the strictly exogenous instruments generally are weakly correlated with the $logE_j(t-\ell)$ and externality results tend again to rise to unbelievable levels.

Finally, I turn to GMM estimation of the production function in (4). As detailed below, I first difference the equations, 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, greatly increasing efficiency. The drawback is that estimation requires plants to remain in the sample for a considerable period of time, significantly reducing sample size. The GMM estimation also allows us to test for exogeneity assumptions on instruments.

Data

The data consist of three sets of information, based on the Longitudinal Research Data [LRD] base, containing the Census of Manufacturers from 1963-92 and the Annual Survey of Manufacturers [ASM] from 1972-92. For the first set of information for each Census year, for each county and MSA we can calculate the various industrial environment variables mentioned earlier, for 1963, 72, 77, 82, 87 and 92. The second set of information is plant level data for 1972, 77, 82, 87, and 92. I eliminate all plant-years for "administrative records," where all data other than employment and wages (gotten from Social Security records) are imputed for certain. Also, 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 an assessment that most relevant nonlabor data has been imputed anyways. Generally only plants in the ASM of a Census year survive and even many of those are eliminated because of imputations. The result for any industry is that the data cover 15-20% of national urban plants in the Census. Finally, I impose the requirements that, for these remaining plant-years, a plant appear in at least two Censuses (so a fixed effect can be identified), and that recorded values of sales and inputs be nonzero. These requirements further reduce the sample by 50%, typically eliminating smaller plants not in two consecutive ASM's of Census years, as well as deaths (noting nearly 50% of plants overall die every five years). So in estimation, my sample covers about 8% of producing plants, across the nine industries. Still the sample sizes are large in absolute terms, with wide geographic coverage.

The third data set picks plants at the beginning and end of each ASM wave: (1974, 78), (79, 83), (84-88) and (89-93). For these plants analysis is restricted to non-affiliate plants: single-plant firms. The sample has some problems. First, the assignment of environmental variables is from the prior Census year, not the data year. Different plant years can be assigned the same externality measure. For example, 1978 and 1979 both are assigned the same "contemporaneous" environmental variables from the 1977 Census (although generally plants do not appear in successive waves, so as to appear in both 78 and 79). Second, capital stock variables are not available for 1988, 89 and 93, so I assign the end of year numbers for 1987 to 1988 and to 89 and for 1992 to 1993. Third, SIC classification must be defined for the Census prior to the wave (e.g., from 1977 for 1979 and 1983 plants), because non-Census year records in the LRD are not updated for changes in SIC definitions. So if a plant switches industry (composition of output), from, say, 1977 to 1983 we won't know to exclude it. Despite these problems, I believe the results will suffice to tell us if externality results for non-affiliate plants differ markedly from the Census year sample dominated by corporate plants.

In estimation, output is annual production (sales adjusted for beginning and ending year inventories of finished products and work-in-progress and for resales). Inputs are total hours worked (production workers hours plus 1800 times the number of nonproduction workers), materials used in annual production, and beginning of year book value of machines, equipment, and buildings (where for 1987 and 1992, buildings can't 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 1972-92, which would reduce the sample sizes to tiny levels. Moreover, with fixed effects, changes in book values pretty accurately measure changes in capital stock.

Overview Results

For this paper, I estimated many different models for different industries, by a variety of statistical techniques. In this section I present overview results on the key issues, comparing four industry groups: Census year high-tech plants (mostly plants of multi-plant, or "corporate" firms), ASM high-tech single-plant firms called "non-affiliate" plants, Census year machinery plants, and ASM machinery non-affiliate plants. Within each group, the individual own industries remain the three-digit ones. For example, within high-tech, for a computer plant, localization/MAR economies is measured by a count of, say, computer plants or computer employment in the county or in the MSA. Within each group, individual industries are pooled in estimation of equation (4), constraining the α and β_ℓ to be the same within the group, but allowing separate time-industry dummy variables ($\delta_i(t)$, for industry i). It turns out that, within each of the four industry groups, coefficients for the individual industries are reasonably similar. I will report when there are important deviations of individual industries from the group results; and, later, I will break out some specific results on individual high-tech industries.

In this section, I start with basic fixed effect results, detailing my primary findings on localization economies — the key results in the paper. Then I present the basic findings on urbanization economies. Table 4 contains the first set of results. Plant-MSA fixed effects and individual-industry time fixed effects are not reported for these unbalanced panels. Sample sizes, counts of plants, and number of geographic areas — counties and MSA's are given. County coverage for the key industrial environment externality variable ranges from 157 to 487. In terms of plants' own technologies, coefficients for plant inputs are pretty much as expected, including the low capital coefficients which occur with fixed effect estimation (see discussion below on functional forms in Table 4b). Coefficients on inputs sum to less than 1, in the range .83 - .95, indicating decreasing returns to scale (given unobserved fixed plant inputs such as "entrepreneurship").

The focus for results in this and in all other tables is on the external industrial environment measures. For reasons which will become apparent, I measure localization economies by the count of own-industry plants in the own county — in essence a count of different nearby sources of information spillovers. Significant localization economies exist in the Census high-tech and machinery groups, as well as (at a 8% level) in high-tech non-affiliates. Moving from OLS (not reported) to fixed effect estimates increases standard errors dramatically, raises coefficients in high-tech and lowers them in machinery.

Primary Results. In Table 4, high-tech industries have scale elasticities of .08, so that an increase in the number of plants in a county from, say, 5 to 50 raises plant output by 18.5%, ceteris paribus, a very strong benefit from local own industry agglomeration. For Census plants, scale effects in high-tech are significantly larger than in machinery, a key finding of the paper. In fact, for individual machinery industries no significant localization

Table 4. Basic Results

(white-corrected standard errors in parentheses)

[Determinants of log (value of plant production) with time-individual industry and plant fixed effects]

	log (hours worked)	log (materials)	log (capital)	log (no. of own industry-county plants)	Adj R^2	Sample size (plants/MSA's/ no. of counties)
High-tech, Census-year ("corporate") plants	.512** (.021)	.358** (.018)	.056** (.011)	.079** (.019)	.964	5160 (1890/218/343)
High-tech, ASM non-affiliate plants	.539** (.053)	.269** (.040)	.032 (.021)	.081* (.046)	.962	1272 (557/110/157)
Machinery, Census year plants	.543** (.016)	.367** (.013)	.038** (.0076)	.023* (.012)	.964	10882 (3970/267/487)
Machinery, ASM non-affiliates plants	.507** (.023)	.280** (.015)	.037** (.0089)	-.016 (.024)	.954	5040 (2157/218/353)

**Significant at 5% level; *Significant at 10% level.

economies are found.[6] Having greater localization economies in high-tech is consistent with high-tech industries being more agglomerated. For corporate plants versus non-affiliates in high-tech, localization economies are the same magnitude, in contrast to priors, where it seemed non-affiliates lacking internal firm cross-plant information networks, would rely more on the external environment. However, this is not the final word on this comparison — differences will emerge when I turn below to dynamic externalities and look in a later section at individual high-tech industries.

Specification Issues. Why do I measure localization economies by the count of own industry plants in the county? An alternative is to use own industry employment, which yielded much weaker results. The reason is apparent in columns (i) and (ii) of part (a) of Table 5. There, for each county, I factor total own industry employment into the number of plants and average plant employment. As column (ii) reveals, average employment in other plants does not contribute to own plant productivity, while numbers of other plants do. This suggests scale externalities derive more from very local information spillovers generated by numbers of plants, rather than externalities in labor markets, which would be represented by total employment (perhaps at the MSA level). Another issue is that we can test whether births and pre-existing plants in the county and births and pre-existing plants outside the county but within the MSA affect productivity equally. This breakdown is given in columns (iii) - (vi) of part (a) of Table 5. For effects outside the county there is no pattern to results.[7] Within the county, effects of births and pre-existing plants are not statistically different, so we lump the two together, to obtain the measure in Table 4.

A second issue concerns whether younger (more dynamic?) non-affiliate plants could provide more spillovers than corporate plants to other plants (either corporate or non-affiliate); or the opposite could be the case — the mature corporate plants could provide

[6]For Census plants coefficients (and standard errors) for SIC 353-358 are .017 (.030), .018 (.028), -.016 (.039), .025 (.023) and .013 (.028).

[7]Pre-existing plants in an MSA, who are competitors, may reduce the value of shipments, an effect opposing externalities. For plants outside the own county this negative effect could dominate.

Table 5. Specifications of Localization Economies and Plant Technology

Part (a) County Own Industry Plants[1]

	log (own ind. county plants) (i)	log (avg. employ in other own ind. plants in county) (ii)	log (own ind. county births) (iii)	log (own ind. county pre-existing plants) (iv)	log (own ind. births in rest of MSA) (v)	log (own ind. pre-existing plants in rest of MSA) (vi)
High-tech Census Year	.102** (.024)	-.021** ((.0094)	.042** (.014)	.071** (.022)	.0006 (.017)	-.039** (.019)
High-tech ASM non-affiliates	.101* (.052)	.012 (.023)	.033 (.026)	.131** (.045)	.031 (.037)	-.060 (.049)
Machinery Census Year	.021 (.014)	-.0036 (.0065)	.013* (.0092)	.012 (.012)	.021 (.014)	-.0036 (.0065)
Machinery ASM non-affiliates	-.020 (.026)	-.0022 (.015)	-.0077 (.014)	-.0021 (.021)	-.023 (.015)	.029 (.025)

[1]Where the minimum value may be zero (e.g., births), the base case is set at one.

Part (b) Other Specifications

	log (own ind. county plants)	Ratio: non-affl./ corporate own ind. plants	Total-factor productivity: log (county own ind. plants)	Trans-log prod. function log (county own ind. plants)
	(i)	(ii)	(iii)	(iv)
High-Tech Census Year	.076** (.021)	.020 (.046)	.121** (.021)	.068** (.019)
High-Tech ASM non-affiliates	.079* (.046)	.053 (.120)	.081 (.052)	.073* (.042)
Machinery Census Year	.023* (.012)	-.0031 (.032)	.036** (.012)	.014 (.012)
Machinery ASM non-affiliates	-.016 (.024)	-.035 (.060)	-.0074 (.022)	-.017 (.023)

more spillovers. Columns (i) and (ii) of part (b) of Table 5 test for this. The ratio of non-affiliate to corporate plants has inconsistent effects and completely insignificant coefficients. All plants seem to contribute equally to spillovers. I also experimented with whether externalities diminished with local scale. I tried a quadratic form to county own industry plants and I also experimented with "stagnation" possibilities, where effects diminish in counties who are in the top 8 ranked own industry employment counties nationally for longer periods of time. The various experiments suggested no diminishing of effects in any industry group.

Finally, there is the specification of own plant technology. One way to handle the problem of possible endogeneity of the $\log X_k(t)$ to $\epsilon_{kj}(t)$ is to look just at the productivity residual, TFP, as a function of the industrial environment. Then the LHS of the estimating equation becomes $\log y_k(t) - \hat{\alpha}(t) log X_k(t)$ where $\hat{\alpha}(t)$ are the national shares of output for factors in year t.[8] The results are given in column (ii) of part (b) of Table 5. Coefficients on county own industry plants are not significantly different from those in Table 4. The main objection to the TFP form is that it presumes that (a) the production technology is an exact Cobb-Douglas and (b) cost-minimizing levels of each input (including capital)[9] are used each period.

The objection to the exact Cobb-Douglas form underlying TFP equations is explored by estimating a trans-log production function (just in $log X_k$'s), or second-order Taylor series expansion in logarithms. In general, the linear, quadratic and interactive terms are all significant suggesting a strict Cobb-Douglas may be inappropriate. However, the results do not always have plants operating in well-behaved regions technology space. Given the high degree of multicollinearity, better estimators would require factor share equations (for, say labor and materials) to anchor the functional form (as well as, potentially, constraints to

[8]Given negative outcomes of Hausman specification tests of using plant random effects estimation with just MSA fixed effects, estimation includes plant/MSA fixed effects. This suggests MSA's with higher externalities — more plants — may attract better (high fixed effect) plants. We explore this issue further in Part 3 of the paper.

[9]For capital usage, I use a rental ratio of 0.15 to be applied to book value — a plausible value (Becker and Henderson, 1999).

ensure plants stay in well-behaved regions). That is beyond the scope of this paper. Rather we simply note that the externality results in column (iv) of part (b) of Table 5 are similar to those in Table 4. While in the version in Table 5, effects are smaller than in Table 4, in other versions (using price deflators) they are larger.[10] So we rely on the Table 4 specification, with its first order approximation.

Changes in Externalities Over Time. It is possible that the magnitudes of externalities have changed over time. For example, improvements in information and communication technologies could make information spillovers more or less important, depending on whether they are complements or substitutes (Gasper and Glaeser, 1996). For the first three industry groups, in columns (i) and (ii) of Table 6, the differential between earlier and later years is zero. Only for machinery non-affiliates does it appear earlier effects could be more negative than the later (zero); but for this industry group the bottom line is simply that there are no significant externalities.

Dynamic Externalities. The focus in Table 4 is on static externalities — the impact on current productivity of changes in the current industrial environment. What about the effect of past environments? Do past environments affect current productivity, reflecting, say, their contribution to a stock of local trade secrets. In Table 7 I test for the effect of changes in the local industrial environment from 5 (t-1) and 10 (t-2) years ago. There are no effects from 10 years ago, but for non-affiliate high-tech industries strong effects from 5 years ago appear. In fact, those effects are stronger, with an elasticity of .11, than any of the static (t) externalities. This could be an anomaly, but results presented below suggest it is not.

Urbanization Economies. Finally, there are urbanization economies. For individual industries, the scale and diversity of all other manufacturing, of general high-tech industries, of

[10] With a trans-log, use of time dummies to control for inflation doesn't strictly do the trick, given interactive and quadratic terms. We also re-estimated the equation deflating output and materials by the CPI, getting similar but somewhat larger coefficients to those in Table 4.

Table 6. Changes in Externalities Over Time

	log (own county plants)	early year (72, 77, 82) differential log (county plants)
	(i)	(ii)
High-Tech Census Year	.084**	-.0060
	(.020)	(.0074)
High-Tech ASM non-affiliates	.082*	-.0031
	(.047)	(.019)
Machinery Census Year	.016	.0070
	(.014)	(.0058)
Machinery ASM non-affiliates	-.016	-.0001
	(.025)	(.011)

Table 7. Dynamic Externalities

log (own industry plants in the county)

	t	t-1	t-2
High-Tech Census plants	.068** (.020)	.013 (.020)	.023 (.016)
High-Tech ASM non-affiliates	.090* (.047)	.108** (.039)	.011 (.032)
Machinery Census plants	.028** (.014)	-.016 (.014)	.0048 (.012)
Machinery ASM non-affiliates	-.018 (.025)	.015 (.020)	-.0032 (.025)

three-digit machinery industries, and of modern service activity generally had no impact on productivity.[11] Columns (i) - (iv) of Table 8 present a selection of results for the four industry groups, focusing on the key urbanization measures — all other manufacturing and all other high-tech. Only all other manufacturing scale (but not diversity) had a significant impact on machinery Census plants (although not for any individual machinery industry). Lagged values (dynamic externalities) show no effects anywhere.

Given this limited outcome, I went to County Business Patterns data for 1977-92 and constructed a measure more in line with Jacobs (1969) — overall diversity of total (excluding the own industry) MSA economic activity. Diversity is over 80 two-digit industries. The estimating equations drop 1972. Diversity of the overall MSA environment has consistently negative signs in column (vi), but is never statistically significant. Moreover on its own, the coefficients in square brackets, it is completely insignificant in all formulations.

Finally, I turned to the most primitive measure, <u>scale</u> of the overall urban environment. It has strong positive effects in machinery, Census plants. There is no positive effect in high-tech where the literature expects such externalities, whether for the group or for individual industries and whether for Census or non-affiliate plants. For Census machinery, the elasticity is very large, around 0.15. A breakdown of this into individual machinery industries shows specific industries drive the results. Coefficients (and standard errors) of .334 (.121), .064 (.111), .095 (.097), .061 (.095), and .234 (.127) for construction, metal working, special industrial, general industrial and refrigeration are obtained for Census plants, so effects are only reasonably significant in two industries. However I note for non-affiliate plants the breakdown is -.147 (.188), .208 (.121), .100 (.180), .491 (.249), and -.227 (.212). The significant and positive effects in construction and refrigeration for Census plants become negative for non-affiliates, an unsettling result. A concern in interpretation is that the results do not reflect urbanization economies, but the greater plant use of (unmeasured) purchased service inputs that occurs in larger scale metro areas (Ciccone and Hall, 1996).

[11]The only exception is that the diversity of high-tech employment affects instruments significantly.

Table 8. Urbanization Economies

	log (all other manu. employ. MSA) (i)	Diversity of manu. employ. MSA (ii)	log (all other high-tech. employ. MSA) (iii)	Diversity of high-tech. employ. (iv)	log (all other private employ. MSA) (v)	Diversity all other employ. MSA [separately entered] (vi)
High-Tech Census plants	-.0023 (.042)	.421 (.331)	-.011 (.014)	-.074 (.085)	-.026 (.072)	-.672 (1.68)
					[-.032 (.071)	-.795 (1.65)]
High-Tech ASM non-affiliates	-.050 (.106)	.242 (.760)	-.039 (.038)	.204 (.338)	-.108 (.169)	-5.27 (4.71)
					[-.139 (.168)	-5.75 (4.70)]
Machinery Census plants	.076** (.027)	-.111 (.175)	N.A.	N.A.	.174** (.049)	-1.65* (.910)
					[.151** (.045)	-.949 (.859)]
Machinery ASM non-affiliates	.055 (.047)	-.0066 (.337)	N.A.	N.A.	.138 (.139)	-2.40 (2.12)
					[.093 (.080)	-.907 (1.17)]

Sample sizes for 77-92 (excludes 72) are respectively 4223, 1170, 7968, and 4182.

There is also the issue of endogeneity of overall MSA scale to the $\epsilon_{kj}(t)$ affecting plant productivity. As we will see next, I do not have time varying exogenous variables with which to instrument for overall MSA scale in 2SLS. For GMM, predetermined (lagged) variables can be used as instruments, but GMM yields completely insignificant coefficients for urbanization economies.[12] For these reasons, the high urbanization economies in Table 8 for machinery are not a highlighted result. But they are suggestive.

The Exogeneity Issue.

Despite controlling for fixed effects, are there local shocks $(\epsilon_{kj}(t))$ which affect the $logX_k(t)$ and $logE_j(t)$, as well as productivity, in the estimating equation? To study this issue, I conducted four experiments. The first allowing for MSA (but not county) time fixed effects (shocks) as well as plant fixed effects yields results similar to Table 4.[13] The next two instrument for RHS variables. In the first I attempted 2SLS, where with fixed effects, instruments in any year must be exogenous to the $\epsilon_{kj}(t)$ for all years. I had insufficient exogenous variables to instrument for plant inputs;[14] so I focused on the TFP equation where the only RHS variable is own industry county plants (where with fixed effects, variables and instruments are demeaned). Instruments are market potential of the MSA overall and for high-tech, and county non-attainment status in ozone regulation, where an attainment status designation goes back to 1972. Becker and Henderson (1999) show location decisions of polluting plants are sensitive to attainment status designation. Market

[12]Estimation (see Table 9 below) by GMM (or Census high-tech and for Census machinery plants yields coefficients (and standard errors) on urbanization scale economies of -.109 (.521) and .354 (.356).

[13]For the sample of multi-county MSA's, I added in (individual industry) MSA-time fixed effects to the equations in Table 4, to control for MSA-wide shocks, as they might affect plant inputs or the number of plants in county. Identification comes solely from time variation of within-MSA county differences in environments and inputs and county shocks are not controlled for . Localization effects are lower in the high-tech (.037) and machinery (.0056) Census samples, although much larger in the high-tech non-affiliate sample (.164). Within the Census high-tech group, the weaker result is driven by just one industry — instruments. For computers, electronic components, aircraft and instruments, the coefficients on own industry county plants are for ordinary fixed effects (see Table 10 below) .100, .102, .026, .062 and for MSA-time fixed effects added are .146, .079, .050, -.109.

[14]The only time (and county) varying instrument for the three plant inputs is manufacturing wages.

potential for MSA_j is the sum of total employment in <u>other</u> MSA's deflated by the distance from j to each MSA. Market potential for high-tech replaces total employment by high-tech employment (SIC 357, 366, 367, 372, 376, 381-384).[15] For these exogenous instruments, 2SLS coefficients for county own industry plants for high-tech Census, high-tech non-affiliate, machinery Census, and machinery non-affiliate are 1.87, .335, -.646, and -3.65, all significant. However, the explanatory power of the first stage — (time variation in county own industry plants as explained by time variation in the instruments) is only about .05 in machinery and .20 in high tech. To help increase this I added in as an instrument (time variation in) county all other industry employment in manufacturing from two time periods ago (lagged to try to enhance exogeneity). For the four respective industry groups, coefficients are 1.36, .110, .302 and -.606, all significant. First stage explantory powers are respectively .24, .43, .07 and .07. Only in high-tech non-affiliates is (a) the coefficient of .110 believable (and consistently with results in Tables 4 and 5 not overstating localization economies) and (b) efficiency reasonable. Given the difficulty with 2SLS work, I turned to GMM, which is more flexible in the choice of instruments and which accounts for heterogeneity.

For GMM, I differentiate equation (4) across adjoining years to get $\Delta log y_k(t) = \alpha \Delta log X_k(t) + \sum_{\ell=0}^{2} B_\ell \Delta log E_j(t-\ell) + \Delta\delta(t) + \Delta\epsilon_{kj}(t)$, where for example $\Delta log E_j(t-2) = log E_j(t-2) - log E_j(t-3)$. In estimation each year (92-87, 87-82, 82-77) is treated as a separate equation, with coefficients (other than $\Delta\delta(t)$) constrained to be equal across years. Instrumenting (lagged 1972 inputs) loses us a year (77-72) in estimation. I treat predetermined variables as exogenous and the length of the instrument list increases from year to year. (E.g., for plant inputs, only 1972 values are exogenous to the 77-82 equation, but 1972, 1977 and 1982 are exogenous to the 92-87 equation.) Instruments for each plant-year include predetermined values of plant inputs, MSA manufacturing employment, county

[15]Even these instruments are difficult to assert as being strictly exogenous, especially across equations within a year (i.e., across MSA's).

non-attainment status, MSA manufacturing wages and county own industry plants.[16] The model is estimated by GMM using DPD (1998 version, Arellano and Bond 1991), accounting for heterogeneity and serial corrlation.

In estimation I work with two samples. First is a balanced panel which requires plants to be in the sample from 1972-92. The advantage of balancing the panel is that there are more instruments available — by 1992 instruments include three sets of predetermined variables (1972, 77 and 82). Given the problem of instrumenting in 2SLS, having a good set of instruments seems critical. The disadvantage is the great reduction in sample size, to about 1/10 of that in earlier tables. The second sample is an unbalanced panel, which greatly expands sample size relative to the balanced panel by adding in a variety of plants that only appear in three Censuses (e.g., plants appearing in 1972-82, 77-87, or 82-92). The disadvantage is that these additional plants have very limited instruments — one year of predetermined values. The basic model in Table 4 is estimated by GMM for the Census samples only. For non-affiliates, which tend to appear in the ASM sample only 2-3 times in a row, sample sizes for GMM are too small.

Results for high-tech and machinery Census plants are given in Table 9. For the balanced panels, high-tech localization economies are higher than in Table 4 at 0.164 (vs. 0.079) and are significant. For machinery localization economies are also higher than in Table 4 but are insignificant. For the unbalanced panels, coefficient magnitudes are similar; but, with the limited instrumenting, standard errors are relatively large.

As a fourth experiment, I tested in the GMM estimation for strict exogeneity of the $logX_k(t)$ and $logE_j(t)$ (to $\epsilon_{kj}(t)$ in all years), compared to just assuming predetermined values are exogenous. Hausman tests could not reject strict exogeneity of the $logE_j(t)$ (county own industry plants) in either sample nor strict exogeneity of the $logX_k(t)$ in high-tech. These Hausman tests, as well as Sargan tests on over-identifying restrictions, indicate that the strict exogeneity requirements in Table 4 are not a major problem. Regardless

[16]Note since equations are differenced and fixed effects eliminated, level values can be used as instruments.

Table 9. Exogeneity Issues

log (number of county own industry plants)

(a) GMM with balanced panels

	coefficient	sample size
High-tech Census sample	.164** (.078)	N = 147 T = 3
Machinery Census sample	.096 (.060)	N = 336 T = 3

(b) GMM with unbalanced panels

High-tech Census sample	.129 (.091)	1615 (581 plants)
Machinery Census sample	.086 (.066)	3186 (1104 plants)

of whether contemporaneous versus predetermined variables are treated as exogenous in the current year, all estimations passed Sargan tests on over-identifying restrictions. Serial correlation tests in all estimations also indicated that errors in the levels equations (the $\epsilon_{kj}(t)$ in equation (4)) are serially uncorrelated.

Given the efficiency problems in instrumenting, the sample size reduction with GMM, and the Hausman and Sargan test results supporting strict exogeneity, I strongly prefer the fixed effect results. And the evidence from instrumenting certainly suggests fixed effect results are not overstating the extent of scale externalities.

Births, Dynamic Externalities, and Non-Affiliate High-Tech Plants

The results obtained in Tables 4 and 5 suggest that non-affiliates do not benefit more from static externalities than corporate plants, or that newborn plants do not generate greater spillovers than pre-existing plants. However, when we looked at dynamic externalities in Table 7 for high-tech plants, evidence suggests greater external benefits for non-affiliated than corporate plants. Second, while, in Table 5 for static externalities, there was no evidence of greater externalities generated by births than existing plants, the investigation did not deal with dynamic externalities. In this section I re-examine differential responses between non-affiliate and corporate plants and I also look at birth (flow) effects, separate from plant (stock) effects, in terms of externalities generated. There remains no evidence of dynamic externalities for machinery in any of formulations below, so results are only reported for high-tech industries.

The first step is to extend the formulation in columns (iii) to (vi) in Table 5, so as to distinguish birth versus existing plant effects and own county versus surrounding county effects, with dynamic externalities (one lag). There is no evidence of dynamic externalities from activity in surrounding counties. For high-tech Census plants I find lagged own county births and pre-existing plants contribute equally (coefficients of .017 and .019). For non-affiliates while the difference in coefficients is not quite statistically significant, the (significant) coefficient for lagged births is .087 while that for pre-existing plants is .0096. Note it is difficult to sort out effects of births from pre-existing plants. Levels and changes in these variables are strongly correlated, given most births are "replacement" births which replace the 50% of plants that die out on average every five years. Given these features, I decided to look at results separately for own county births, as well as plants.

Results are given in Table 10. The birth results have smaller sample sizes because 1972 is dropped as an estimating year since we don't know births in $t-2$ for that year. For Census plants, dynamic birth effects are larger than plant effects and coefficients are statistically significant; but, for non-affiliates, birth and plant effects are very similar. Thus there seems to be, at best, modest evidence that birth effects can be more important than plant effects.

However, we continue to conclude that non-affiliates benefit more than corporate plants from externalities, at least dynamic ones. The difference in coefficients for plant effects is significant at $t-1$. This accords with the intuition that non-affiliates are more reliant on the external environment, utilizing the accumulated stock of local trade secrets. I also examined whether younger plants, per se, benefit more from externalities than older plants, but found no decline in externality benefits with age.

Table 10 also examines individual high-tech industries, for the Census sample. I don't report results for non-affiliates for individual industries because of problems with limited sample sizes. The individual industry results are interesting. Aircraft does not seem to experience positive externalities. Birth effects in instruments and perhaps electronic components appear stronger than plant effects, while for computers the opposite is the case. The general conclusion is that specific industries respond differentially to static versus dynamic externalities and to externality sources — births versus plants.

3. INDUSTRIAL AGGLOMERATION AND SCALE ECONOMIES

Combining results in sections 1 and 2, there are conclusions and two additional issues.

Table 10. Dynamic Localization/MAR Economies:

	log (no. of plants in own industry in county)				log (no. of own industry births in county)			
	t	t-1	t-2	Sample Size	t	t-1	t-2	Sample Size
Census plants	**.068** (.020)**	**.013 (.020)**	**.023 (.016)**	**5160**	**.042** (.163)**	**.035** (.017)**	**.040** (.015)**	**4223**
Non-Affiliate plants	**.090* (.047)**	**.108** (.039)**	**.011 (.032)**	**1272**	**.058* (.031)**	**.107** (.035)**	**.037 (.028)**	**1193**
Individual Industries Census Sample								
Computers	.094** (.053)	-.0013 (.052)	.039 (.035)	921	-.026 (.048)	-.062 (.041)	.046 (.027)	759
Electronic components	.075** (.024)	.031 (.033)	.051** (.023)	1987	.053** (.027)	.074** (.031)	.072** (.024)	1701
Aircraft	.051 (.036)	-.032 (.024)	-.063** (.030)	1043	.017 (.028)	.020 (.028)	-.017 (.027)	795
Instruments	.043 (.045)	.044 (.043)	.012 (.038)	1107	.098** (.038)	.075* (.043)	.022 (.031)	881

**Significant at 5% level; *Significant at 10% level.

In terms of conclusions, localization/MAR scale externalities arise from the number of local own industry plants, or points of information spillovers. Overall static externalities seem to affect older corporate and younger non-affiliates plants equally; and both seem to offer the same externality benefits to others. However, once we allow for dynamic externalities, overall and industry by industry in high-tech, non-affiliates seem to benefit from externalities more than corporate plants, which have their own firm information networks. High-tech and most individual machinery industries do not benefit from urbanization-Jacobs economies from manufacturing and related industry diversity and scale, nor overall urban scale and diversity. The high-tech industries experience greater local external scale economies than machinery and, as such, are also more agglomerated, than the machinery industries as would be expected. However, the on-going spread of industries, especially machinery ones, is not related to changes in scale economies over time. Instead they may be related to declines in transport costs/weights of inputs, allowing producers to spread out and move nearer customers.

A particularly surprising conclusion is that the high-tech industries subject to large-scale economies are more mobile (except for aircraft) than the machinery industries. In fact, electronic components which is arguably the most mobile industry is also an industry experiencing major dynamic externalities. Why is this? I offer two reasons. First, greater mobility in high-tech might arise from the tentative finding that for some individual high-tech industries new births are a source of positive spillovers — new blood injecting new life into localities. That suggests that local plant-turnover is important to sustaining productivity. Also new locations may not be at such a distinct disadvantage in attracting plants compared to existing agglomerations; new locations can generate spillovers by births, creating externalities as they grow. While this process has never been formally modelled, it suggests locational mobility is much easier than Arthur (1990) envisioned.

Of course, 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 .125 (with a range for individual industries from .097 to .153); and the ratio of heavy inputs to all inputs averages .234 (with a range from .177 to .279). For high-tech, the corresponding numbers are .049 (range .016 to .071) and .089 (range .026 - .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 locationally fixed (i.e., the Markov transition process is not homogeneous, but varies with resource endowment conditions).

The results in the paper shed light on two additional issues. First, in contrast to Glaeser et al. (1992) and Henderson et al. (1995) we find no evidence of urbanization-Jacobs economies in high-tech industries from overall MSA scale or diversity. Given these high-tech industries are subject to on-going rapid technological developments, some have argued that greater productivity requires an infusion of ideas from outside the own industry, which would be enhanced in larger, more diverse urban environments. I find little evidence of this. For the own industry employment growth rate regressed against base period values of log of all other industry employment, all other industry diversity, log of own industry employment (with mean reversion and localization/MAR economies not disentangled), and time-industry dummy variables, Table 11 give us OLS and MSA fixed effect results for growth from 77-82, 82-87, and 87-92 for high tech. Similar results occur if we replace the own industry scale measure of employment by numbers of plants.

In Table 11, while MSA scale is important to industry growth in OLS, with fixed effects it is not. For diversity, fixed effect results are supportive of urbanization-Jacobs economies, in contrast to my productivity findings. This suggests diversity is important in location

Table 11. Urbanization/Jacobs Economies from Employment Growth Equations

	log (MSA own ind. employ.)	log (all other MSA employ.)	Diversity of all MSA employ.	N (no. of MSA's)	adj. R^2
	t-1	t-1	t-1		
High-tech OLS	.764** (.012)	.222** (.026)	-1.08 (1.63)	2229 (296)	.759
fixed effects	.660** (.021)	-.134 (.244)	-8.80* (4.78)	2229 (296)	.771
Machinery OLS	.791** (.0092)	.188** (.017)	-1.52* (.801)	3616 (312)	.791
fixed effects	.730** (.015)	-.452 (.172)	4.04 (2.59)	3616 (312)	.798

decisions (diversity of suppliers of inputs) but not in providing actual externalities. I also report results for machinery, where fixed effects results are not supportive of urbanization-Jacobs economies at all (in contrast to Table 10 findings). Even if MSA scale does improve productivity in machinery, that doesn't mean recent growth of MSA's is correlated positively with machinery employment growth. Machinery growth may not be competitive in faster growing MSA's. In short I believe productivity formulations yield quite different results from growth ones because the former examines productivity effects of externalities, while the latter examines spatial allocation, or location processes in a general equilibrium context with many determinants.

The second issue concerns whether firms in an industry go to urban sites with better industry-specific city amenities. The alternative is that industry agglomerations are spread randomly across potential sites, with resulting patterns being "accidents of history." I have a measure of MSA specific time invariant amenity benefits for each industry. From the estimation of the productivity relationship (I use the specification in Table 4 for all industries), I get a location-plant fixed effect, $\hat{f}_{kj}$. I average these fixed effects across plants in each industry in each MSA to get an MSA fixed effect for each industry, $\hat{f}_j$.[17] The question is whether MSA's with higher f_j's have higher employment in an industry? The answer bears on two questions. Are site amenities a basis for agglomeration and/or do industries which agglomerate for other reasons go to the best urban sites?

[17]There is an issue if we decompose f_{kj} into f_k and f_j, whether f_k's are correlated with f_j's — better plants go to better amenity locations. Without scale effects and absent locational amenities, plants of differing abilities would locate randomly — they would have no need (1) to cluster together or (2) to cluster so highly ability plants are segmented from lower ability ones (Black, 1998). Absent scale effects but with locational amenities, with or without differences in f_k's, plants would tend to cluster in locations with higher f_j's. These locations offer higher inherent productivity and thus draw in plants. However, not all plants go to one location with the highest f_j since locations because "congested" — with agglomeration, wages, land and environmental costs rise. If the f_k vary as well, one can envision segmentation, where higher f_k plants go to higher f_j locations, because they can better afford the higher congestion costs — or they benefit more from higher f_j's than lower ability plants. Regardless we can say, if the $\hat{f}_j$ and own industry employment levels are positively correlated, then absent scale economies, this implies locational amenity differences exist. Such amenities are then a basis of agglomeration.

In Table 12 for the four industry groups, I show the correlations between the $\hat{f}_j$'s and both 1992 MSA own industry employment levels and growth in own industry employment from 1972 to 1992. We are looking for positive correlations with employment levels; but correlations with growth may be zero or negative. Even if industry stocks are in high amenity locations, growth may be at the margins everywhere or at inferior sites, given better ones are congested. For machinery industries the pattern holds; correlations with levels are positive, and larger than with employment growth. For high-tech all correlations are low, although employment growth is positively completed with MSA-fixed effects. With regional shifts in high skill labor and population, the identity of the best high-tech employment centers may have changed with time, with the best plants focusing on high growth areas. However, I would again cite the result that births may generate high-tech productivity improvements. Maybe the best plants seek the fastest growing locations with the most births in high-tech industries.

Table 12. Do Plants Go to the "Best" Locations

Simple Correlation Coefficients: MSA-Industry Fixed Effects With:

Samples	Log (1992) MSA own ind. employment)	Growth rate 92-72 of own ind. MSA employment
High-tech Census	-.036	.086
High-tech, non-affiliates	-.035	.075
Machinery, Census	.140	-.048
Machinery, non-affiliates	.271	.113

REFERENCES

Arellano, M. and S. Bond (1991), "Some Tests of Specifications for Panel Data: Monte Carlo Evidence and an Application to Employment Equations," *Review of Economic Studies,* 58, 277-297.

Arthur, B. (1990), "Silicone Valley Locational Clusters: When Do Increasing Returns Imply Monopoly," *Mathematical Social Sciences,* 235-251.

Beardsell, M. and V. Henderson (1999), "Spatial Evolution of the Computer Industry in the USA," *European Economic Review,* 43, 431-456.

Becker, R. and V. Henderson (1991), "Costs of Air Quality Regulation," in *Distributional and Behavioral Effects of Environmental Policy,* C. Carraro and G. Metcalf (eds.), University of Chicago Press, forthcoming.

Black, D. (1998), "Essays in Growth and Inequality in an Urbanized Economy," Brown University unpublished Ph.D. dissertation.

Black, D. and J.V. Henderson (1998), "Urban Evolution in the USA," Brown University, Working Paper # 98-21.

Black, D. and J.V. Henderson (1999), "A Theory of Urban Growth," *Journal of Political Economy,* 107, 252-284.

Ciccone, A. and R.E. Hall (1996), "Productivity and the Density of Economic Activity," *American Economic Review,* 86, 54-70.

Davis, S., J. Haltiwanger, and S. Schuh (1996), *Job Creation and Destruction,* MIT Press.

Eaton, J. and Z. Eckstein (1997), "Cities and Growth, Theory and Evidence from France and Japan," *Regional Science and Urban Economics,* 27, 443-474.

Ellison, G. and E. Glaeser (1997), "Geographic Concentration in U.S. Manufacturing Industries: A Dartboard Approach," *Journal of Political Economy,* 105, 889-927.

Fujita, M., P. Krugman, and A. Venables (1999), *The Spatial Economy,* MIT Press.

Gasper, J. and E. Glaeser (1996), "Information Technology and the Future of Cities," Stanford University, mimeo.

Glaeser, E., H. Kallal, J. Scheinkman and A. Schleifer (1992), "Growth in Cities," *Journal of Political Economy,* 100, 1126-1152.

Henderson, J.V. (1986), "Efficiency of Resource Usage and City Size," *Journal of Urban Economics,* 18, 47-70.

Henderson, J.V. (1998), "Evidence on Scale Economies and Agglomeration," Brown University Working Paper No. 98-22.

Henderson, J.V., A. Kuncoro, and M. Turner (1995), "Industrial Development of Cities," *Journal of Political Economy,* 103, 1067-1090.

Holmes, T. (1998), "The Effect of State Policies on the Location of Manufacturing: Evidence from Border States," *Journal of Political Economy,* 106, 667-705.

Jacobs, J. (1969), *The Economy of Cities,* Random House.

Karlin, S. and H. Taylor (1975), *A First Course in Stochastic Processes,* San Diego: Academic Press.

Lucas, R.E., Jr. (1988), "On the Mechanics of Economic Development," *Journal of Monetary Economics,* 22, 3-42.

Markusen, A., P. Hall, and A. Glasmeier (1986), *High-Tech America: The What, Where and Why of the Sunrise Industries,* Boston: Allen and Unwin.

Marshall, A. (1890), *Principles of Economics,* London: Macmillan.

Nakamura, R. (1985), "Agglomeration Economies in Manufacturing," *Journal of Urban Economics,* 17, 108-124.

Rauch, J. (1993), "Does History Matter When It Only Matters a Little," *Quarterly Journal of Economics,* 108, 813-867.

Romer, J. (1993), "Increasing Returns and Long-Run Growth," *Journal of Political Economy,* 94, 1002-1037.

Sveikauskas, L. (1975), "Productivity of Cities," *Quarterly Journal of Economics,* 89, 393-413.

</ref_section>