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The renaissance in regional research

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1. Introduction

It is quite instructive to recall that less than a decade ago regional science was widely thought to be in some kind of "crisis." The first evidence of generalized concern with the field of regional science appears in Jensen's Presidential Address to the RSAI which was published in early 1991. This was followed by a specific diagnosis by Bailly and Coffey (1994) reported in a paper presented in Hawaii (of all places) at the 32nd annual meeting of the WRSA in 1993. This, in turn, stimulated commentary by a wide variety of natural scientists, geographers, economists, and planners in North America and Europe, decrying the existing state of regional science and suggesting a variety of collective actions that could be taken.

The principal charges levied by Bailly and Coffey against regional science in their fifteen-page rebuke to the profession were "a lack of relevance" and "a narrow perspective." The first of these was attributed to "a lack of emphasis on practical issues (p. 4)" and "the relative exclusion of social and human dimensions [of regional problems] (p. 6)." These sentiments were amplified in comments by Gibson (1994), modified in comments by Plane (1994) and rejected in comments by Anas (1994). The theme of crisis in regional science was further embellished in two volumes of the *International Regional Science Review* published in 1995 in which some 32 North American scholars debated the status and future of regional science. The introspection included a 48 page paper by Andrew Isserman on the status and future of regional science (1995).

A decade after Jensen's warning and a half decade after the soul searching stimulated by Bailly and Coffey, there are still occasional reactions to the presumed crisis in regional science (see, for example, the comments of John Rees, 1999). But no collective action has been taken to "reform" regional science – in the topics of analysis, in the methods used for analysis, or in its unrelenting

emphasis on narrow positivism in the modern (or perhaps post-modern) world of the new millennium.

And it's a good thing nothing has been done. And it will be a good thing if nothing is ever done to "fix" the problems of regional science. During the past decade several of the most exciting intellectual challenges in social science have emerged and been framed in exactly the terms familiar to students of regional science. These specific scientific problems have important policy implications, and they are also central to theoretical inquiry in several core disciplines – economics, political science, sociology, and statistics, for example.

It is my contention that, at no time since Von Thünen, have spatial relationships been so important in the social sciences and in the policy implications of social scientific inquiry. In the brief space of this paper, I will sketch two areas where this renaissance in regional research is clearly prominent. I will then indicate the driving forces behind these research areas and provide some links to institutions and individuals in regional science.

2. The renaissance by example

2.1. Urban space and economic growth

One of the most critical issues facing students of the modern economy has been the identification of the sources of economic growth and the factors that govern the rate at which the productivity of the economy improves. Robert Solow's theoretical work (1956) of four decades ago provided some clues, and Edward Dennison's analysis of national income data (1974) pointed to the importance of education and investment in human capital in explaining productivity improvements. But these analyses were framed in terms of exogenous factors leading to improvements in economic performance. Labor- or capital-augmenting technical change led to increased output, but the ultimate source of this technical change was exogenous to the economic system itself. In contrast, among students of the regional economy (e.g., Hoover 1963) there had long been an alternative story, loosely described. External economies of scale were enjoyed in urban areas, and these scale economies were produced by the co-location of firms and workers. This external economy provided a powerful description of the advantages of cities in the location of economic activities and provided a rationale for the higher incomes earned in urban areas. Indeed, an original puzzle in regional science – why would households and firms choose to locate in close proximity to one another, bidding up rents and land values? – required that there be some form of "economy" arising from the spatial pattern of economic activity. Why else would firms and households live close to each other on expensive land rather than dispersed on cheaper land?

Traditionally, this economy was modeled as a savings in transportation costs. A centrally located port facility provided cheap access so that locally produced output could be exported to world markets. Alternatively, a single market for agricultural products oriented farmers towards the central place and provided the rationale for rent differentials for otherwise identical land at different distances from the market.

As far back as Marshall there were explicit references to external economies that were produced by economic actors, for example "economies of

localized industry'' (David and Rosenbloom 1990) arising from shared inputs in production and consumption. More recently, Jane Jacobs (1969) speculated eloquently on the consumption economies arising from large urban agglomerations.

But it was only in the past decade that these external economies were formally modeled as endogenous outcomes, literally caused by the co-location of individuals and firms in urban areas. This powerful insight is, in part, an application of the new growth theory attributed to Paul Romer (1986). In any case, the perspective has opened up an enormously powerful range of theoretical and empirical research. The first papers incorporating this perspective, by Fujita (1988), Rivera-Batiz (1988), and by Abdel-Rahman (1988), appeared in a special issue of *Regional Science and Urban Economics* only a little more than a decade ago. These ideas were then extended by Abdel-Rahman and Fujita (1990) in a paper in the *Journal of Regional Science*, and they were popularized more broadly by Krugman in 1991. Since then, there has been an avalanche of theoretical research on the topic.

These new urban and regional models all share a common structure. This structure has a remarkable property. Each actor in the economy solves a well-defined economic problem in a competitive environment (and each earns zero profit), and yet there are increasing returns to the economy as a whole.

As an illustration, consider the following simplified structure. Let the output of a firm $\mathscr{O}$ be a function of the labor, L, capital, K, and intermediate producer services, V, employed in production according to a simple Cobb-Douglas production function

$$\mathscr{O} = L^{\alpha} K^{\beta} V^{\gamma}. \tag{1}$$

Now suppose that producer services, the aggregate of all intermediate goods in use in the firm's production process, varies with the set of individual services available in the local economy

$$V = \left(\sum_{i=1}^{n} S_i^{\delta} \right)^{1/\delta}, \quad \delta > 0, \tag{2}$$

where S_i is the amount of service i used in the production of output $\mathscr{O}$.

According to the form of Eq. (2), the quantity of V, the aggregate of these intermediate services, depends upon the number of distinct types of services available in the local economy, n. For example, the effective amount of intermediate services is greater if legal services are available separately in such finer categories as litigation, contract support, tax advice, and so forth. This specification recognizes that the division of labor improves economic efficiency. In this way, the greater variety of producer services available in larger metropolitan areas exerts an effect upon production conditions, which is independent of any single firm's choices of inputs.

Note the symmetry between the various producer services described in Eq. (2); if their costs are identical, then the amount of each service purchased will be equal for all producer services

$$S = \sum_{i=1}^{n} S_i = n S_i. \tag{3}$$

Equations (2) and (3) imply

$$V = Sn^{(1-\delta)/\delta},\tag{4}$$

and inserting this expression into the production function, Eq. (1), yields

$$\mathcal{O} = n^{\gamma(1-\delta)/\delta}L^{\alpha}K^{\beta}S^{\gamma}.\tag{5}$$

Equation (5) indicates that output increases with the number of distinct producer services available. The parameter δ measures the importance of heterogeneity of services in the production of output. As δ declines, the exponent of n increases and the diversity of services is more important in augmenting production. As δ approaches 1, aggregate producer services in Eq. (2) becomes a simple sum of the specialized services available, that is, services become perfect substitutes for each other. As δ approaches 1, the exponent in Eq. (5) approaches 0, and the external economy disappears.

An analogous role for diversity can be deduced on the consumption side of the market. Indeed, the analogy to Eqs. to (1) and (2) is merely

$$U = X_1^{\alpha}X_2^{\beta}X_3^{\gamma} = X_1^{\alpha}X_2^{\beta}\left(\left[\sum_{i=1}^{n}S_i^{\delta}\right]^{1/\delta}\right)^{\gamma}.\tag{6}$$

In this representation, final consumers have Cobb-Douglas preferences over goods X_1, X_2 and X_3. In turn, X_3, is an aggregate of distinct consumption alternatives, S_i. Consumers are better off with a larger number n of distinct consumption alternatives (i.e., Neopolitan, Sicilian, and Tuscan restaurants, not merely Italian restaurants). Consumers' tastes for variety, again measured by δ , may be more easily satisfied in larger cities. Utility is higher in more diverse environments:

$$U = n^{\gamma(1-\delta)/\delta}X_1^{\alpha}X_2^{\beta}S^{\gamma}\tag{7}$$

Of course, the benefits of these external economies in production and consumption do not come free. In larger agglomerations, transport costs for consumers and producers are higher, and site rents are bid up by competitors. In general equilibrium, these benefits are traded off against transport costs and rent payments, limiting the sizes of metropolitan areas and the benefits of agglomeration. Negative externalities (e.g., pollution) further reduce the external economies.

During the past decade, there has been a vast outpouring of theoretical and empirical papers extending and refining these ideas. This work has involved important empirical contributions by Henderson (1995, 1999), Glaeser and associates (1992, 1995), among many others, and has culminated in a recent book by Fujita et al. (1999). At the 1999 North American meetings of the Regional Science Association in Montreal, there were at least seven sessions devoted to these ideas (RSAI 1999), and the *Journal of Economic Perspectives* has recently published a symposium on the topic (Quigley 1998; Krugman 1998; Glaeser 1998).

The empirical evidence assembled to support and test these theoretical insights about the regional economy is potentially very valuable. Hitherto,

much of the discussion about the sources of economic growth had been framed at the national level, and most of the empirical evidence – time series data across a sample of countries – is inherently difficult to interpret. By framing these theoretical propositions at the level of the region, it is possible to investigate empirically the sources of endogenous economic growth using much richer bodies of data within a common set of national institutions. Geographical considerations of labor market matching and efficiency (Helsley and Strange 1990), of the concentration of human capital (Rauch 1993), and of patent activity (Jaffe et al. 1993) have all been studied at the metropolitan and regional level, and considerable effort is underway to use regional economic data to identify and measure more fully the sources of American economic growth. These are major research activities exploring externalities in regional economies throughout the developed world. This research program did not exist at all a decade ago.

2.2. Urban space and the social consequences of neighborhoods

Does the spatial arrangement of residence patterns affect the economic opportunities and outcomes of urban residents? Over thirty years ago, it was demonstrated that patterns of segregation in the housing market affect the employment prospects of minority residents. These findings, by Kain (1968) and many others (see O'Regan and Quigley 1999, for a review), were based upon a straightforward extension of the original Alonso model. Longer worktrips imposed by racial segregation reduced the net wages of minority workers and this reduced labor supply.

Only in the last decade, however, have the broader implications of space upon economic outcomes been analyzed in detail. William Julius Wilson's (1987) elegant but informal assessment of the plight of poor children growing up in disadvantaged neighborhoods spawned a large set of studies by sociologists and social psychologists attempting to measure these effects. An early and massive review by Jencks and Mayer (1990) examined the link between educational attainment (e.g., high school graduation, college attendance, cognitive skill development) and a variety of neighborhood influences. Jencks and Mayer also examined other outcomes (for example, criminal behavior and teenage sexual behavior) as well as labor market success.

The authors concluded that the evidence about the causal effects of neighborhoods was highly ambiguous, observing that, "the reason we don't know more is not that the questions are so hard to answer but that we have not invested much time or money in looking for answers (p. 178)."

In response to this challenge, sociologists, geographers, and economists developed the concept of the "geography of metropolitan opportunity" (Galster and Killen 1995), using the statistical tools familiar to regional scientists to analyze the importance of local externalities. Case and Katz (1991) used now-standard techniques of spatial autocorrelation to analyze the link between unemployment, criminal behavior, and bad neighborhoods. Galster and Mikelsons (1995) used extensive graphical methods to describe and measure the economic geography of neighborhoods in Washington D.C. Modern geographical information systems only make these measurements more powerful.

Recently, the formal methods used to identify the separate effects of neighborhood or geographic aggregates in these models have come into question.

Manski (1995) devoted a full chapter in his recent book on *Identification Problems in the Social Sciences* to this issue, which he named the "reflection problem." Evans et al. (1992) developed a maximum likelihood procedure to distinguish neighborhood effects in the context of schools. Even more recently, Brock and Durlauf (forthcoming) provided a synthesis of these complex models, which they define as "interactions – based models."

The issue recognized and addressed in these modern econometric treatments is nothing other that the classic problem originally pointed out by Charles Tiebout, arguably the most famous regional scientist of all. To illustrate this, consider the following representation of neighborhood or peer group effects

$$y_i = \boldsymbol{\beta}_1 X_{1i} + \overline{Y}_i \boldsymbol{\beta}_2 + \varepsilon_i. \tag{8}$$

In this model, Y_i is an individual outcome, say the probability of employment of individual i, X_{1i} is a vector of human capital and personal determinants of employment outcomes, and $\overline{Y}_i$ is the aggregate unemployment rate in the neighborhood in which individual i resides. In some formulations, the error structure specifies spatial autocorrelation (e.g., Case and Katz 1991),

$$\varepsilon_i = \sum_j f(d_{ij})\varepsilon_j + \xi_i, \tag{9}$$

Where d_{ij} is the distance between neighborhoods i and j and where $f(\ )$ is some functional aggregation of space, for example a gravity representation.

Tiebout's insight is that the average neighborhood composition is not exogenous to the outcome for any individual. Neighborhoods are chosen in a spatial market in which individuals and households vote with their feet, given prices and opportunities. In this way, individuals with weak labor force attachments are more likely to choose neighborhoods less accessible to workplaces, whose lower rents reflect this difference. Thus, in a cross section, it will be observed that unemployment rates are higher among those who live in inaccessible neighborhoods, but this association may say nothing at all about causation.

To address this problem, many researchers (e.g., O'Regan 1993; Raphael 1998) have concentrated on outcomes for at-home youth, arguing that the residences and neighborhoods in which youth reside are chosen by their parents. But since parents' attitudes and outcomes affect their children's behavior, this approach is not fully satisfactory.

A more complete model, and one more in the spirit of Tiebout, would postulate a second equation

$$\overline{Y}_i = \gamma_1 X_{1i} + \gamma_2 X_{2i} + \boldsymbol{\eta}_i \tag{10}$$

in which households choose the composition of their neighborhoods on the basis of vectors of observable characteristics X_{1i} and X_{2i}. The set of other exogenous variables X_{2i} identifies the two equations. Finally, an assumption about the co-variances between error terms permits the likelihood function describing Eqs. (8) and (10) to be defined, and the parameters can be estimated. Brock and Durlauf (forthcoming) survey many of these models and indicate the circumstances in which identification is possible.

The policy significance of this work is at least as important as the research on economic growth noted above. The concern arises from the substantial increase in the spatial concentration of poverty reported in the 1990 census and especially the increasing concentration of the minority poor (see Jagorsky 1997). The importance of policies to reverse these trends is heightened if the concentrations themselves have external effects upon social outcomes, that is, if in the aggregate social outcomes are worse under concentration than the outcomes experienced by the identical individuals and households under a different geographical arrangement.

As an alternative to these increasingly complex statistical models, other analysts have tried to learn from so-called "natural experiments" in which households are moved from one neighborhood to another. A series of studies of the Gatreuax program in Chicago by sociologists (e.g., Rosenbaum 1995) and of the Moving to Opportunity Program in Baltimore by economists (e.g., Ludwig et al. 1999) have assembled evidence on the importance of local economic geography in conditioning social outcomes.

As noted above, it is not easy to measure the importance of these neighborhood externalities. But there has been a substantial outpouring of high quality research during the past decade, experimental and non experimental, directed towards these measurements. The work has involved several core disciplines, from geography and sociology to statistics and social psychology. Some of this work was well underway during the recent questioning of the research agenda in regional science, but most has arisen during the decade of the 1990s.

3. Some speculations

In these few pages, I hope to have convinced you about the central importance of two areas of regional research which are thriving, indeed prospering, and which have developed only in the very recent past. I suspect other areas of regional research are equally "hot." Another example might be the growing interest in regional government finance and politics, and the issues in fiscal federalism arising from the permeability of regional boundaries. With the emergence of the European Union, the design of regional political constitutions and their incentives have become crucial policy issues – and ones in which "space matters in an important way." (see, for example, Ordershook and Svetsova 1997).

A second example, somewhat broader and more imperialistic, might include the whole topic of non market economic interactions. Glaeser (2000) has recently argued that "mainstream economists who focus on classic market transactions, have simply missed many of the most important pieces of the economy." He further argues that

> "these interactions are probably at least as important as standard market transactions in producing human happiness and possibly economic growth as well. Most of the non-market interactions that we care about have a profoundly important spatial component. The borrowing of information, the transfer of values and the formation of networks appear to take place primarily at short distances. The formation of cities both effects these non-market interactions and is affected by them."

At a minimum, the two fields of inquiry I have reviewed, ''urban agglomeration'' and ''neighborhood effects,'' have each attracted a great deal of attention. Together, they have engaged some of the best minds in the social sciences. The contribution of regional science ''infrastructure'' to this research enterprise has been substantial. This includes, for example, the facts: that many of the influential original papers on agglomeration were published in journals that have ''regional science'' in the title or are well known to regional scholars; that regional science meetings, at least in North America, have featured well attended sessions devoted to agglomeration and neighborhood effects for the better part of a decade; that at least two of the recent winners of the WRSA's Charles Tiebout competition were chosen for their contributions to the neighborhood effects literature.

I think the health of regional science has never been better. The research agenda has never been stronger, and the importance of these research issues to the core disciplines with which regional scientists identify -geography, economics, politics, statistics – has never been higher. The research agenda derives from policy issues within the larger society and from theoretical advances within the basic disciplines. With the increasing attention to space and to spatial interactions, particularly in a non-market context, the research outlook for regional science has never been more exciting.

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Comments on
The renaissance in regional research

John Quigley's paper is welcomed as the intellectual equivalent of why it is important not to be too anxious to discard old ideas or methodology; like neck-ties, they have significant potential for appearing again to be both useful, in style and in laying the grounds for new innovations. It was inevitable that regional science should go through a period of self-evaluation; periodic reassessment is good for the soul and often helps uncover impediments to progress. However, John has moved the debate from an intrinsic to an extrinsic focus and my remarks support the need to keep this latter focus in the years ahead.

I could not agree more with the sentiment of John's remark that "... it's a good thing that nothing has been done," (in response to the internal evaluations) but I think that he meant internally to regional science because it is both clear from his paper and the empirical evidence that there has been a remarkable sea change in the way the area commandeered by regional science is viewed by the body economic. We can argue over the degree to which this was the result of a small cadre of regional scientists proselytizing or some random set of events that caused economists to wake up and discover that the Anglo-Saxon basis noted by Isard forty years ago was very close to the truth.

In my remarks, I am going to elaborate a little on some of John's themes, drawing out some of his perspectives in complementary directions. One of the principal tenets of regional science, espoused in Isard's Location and Space-Economy was the duality between transportation and location decisions. Undergirding a great deal of the renaissance is the discovery of just how important this duality has become to understanding the changing spatial dynamics of the regional economies. I do find that there has been a certain degree of myopia in the application of the Romer et al. ideas to the urban qua spatial economy in the sense that the ideas seem to presuppose an existing spatial structure. What the duality has revealed, in the case of the US, is a liberalization of agglomeration economies from the metropolitan scale to much broader geographies. Firms are realizing scale economies in individual establishments where production is concentrated in a relatively small number of products; the diversity predicted by the models is being realized over many

establishments, often located in different states. In essence, economies of scope are still internal to the firm but external to any establishment. The evidence we have assembled from the Midwest clearly supports these findings; interstate trade is rapidly integrating clusters of state economies. Individual metropolitan areas, like Chicago, are hollowing out (the degree of intermediation in production is declining) and the spatial extent of the linkage structure is now most heavily concentrated in a radius of 200–500 miles. However, negative externalities are not the major cause – the realization of scale economies in combination with very low transportation costs allow for greater separation in production. Yet, at the macro level, these state economies are becoming very similar in structure (e.g. the percentage of activity accounted for by any two-digit sector) and interstate trade is dominated by intra – rather than inter-industry trade. Hence, the endogenous growth processes are now more regional in scope; perhaps we need to find new terms for agglomeration effects that we think of as spatially restrictive that still attract activity to geographies that extend well beyond the metropolitan scale.

Quigley's second focus, on the neighborhood scale is also welcomed. Here there are strong parallels between the regional and the intra-metropolitan scale. A great deal of the properties and findings of the new economic geography also apply within metropolitan areas but there is one important distinction – namely the daily flow of labor (commuting) and the associated flows of income. In this regard, neighborhood effects are far more difficult to contain; while Tiebout examined the location of residence choice process, there are equally important processes involving journey-to-work patterns for increasingly multiple-earner households and a further complexity associated with the expenditures on goods and services by those households. In our analysis of Chicago, we uncovered the fact that the south side (predominantly African-American) had earned income in excess of $10 billion; if business leaders were told of a new country with that level of purchasing power, they would have been falling all over each other to serve this country, locate plants there and consider a whole range of developments. The evidence – look at how excited they became over news that exports from Illinois to Mexico have increase from $1.1 to 1.2 billion (front page news in the business section of the Chicago Tribune). Yet, in their back yard, geographically speaking, is an opportunity eight times larger. We need to consider in our neighborhood-level models some parallels from trade theory in which exploration of non-tariff barriers is given higher priority.

Finally, I think it would be useful to collect further evidence of this renaissance, not just to make us feel good (a glass of Australian shiraz will do that more effectively – and faster!) but to gain a greater sense of the new opportunities that exist. For example, Jeffrey Frankel in the introduction to his new book, The Regionalization of the World Economy, notes:

> "Most international trade research in the past has ignored the geographic dimension. International trade models, whether empirical or theoretical, whether based on smallcountry or large-county assumptions, and whatever else their attributes, tended until recently to have one curious thing in common: they treater countries as disembodied entities, that lacked a physical location in geographic space. Many of the more interesting aspects of regional trading arrangements require the introduction of a geographic dimension."

In this same book, Alan Deardorf examines whether the gravity model works to help explain bilateral trade; however, he does so without ever referencing the use of the gravity model in similar applications (I think there have been a few (sic) applications in regional science!). If we have a problem, it is that we should be spending more time expanding the now opening minds of our receptive economics' colleagues rather than dwelling on introspection about our field. We have a hell of lot to offer and the sort of paper John has provided needs to be replicated and published in journals read by those who can be converted from what Rodney Jensen referred to as the great unwashed, to those deeply scented by the bouquet of regional science.

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