Well Isn't That Spatial?! Handbook of Regional and Urban Economics: A View From Economic Theory*

Marcus Berliant†

February 2005

*I thank Gilles Duranton, Sukkoo Kim, Fan-chin Kung and Ping Wang for comments, implicating them only for baiting me.

†Department of Economics, Washington University, Campus Box 1208, 1 Brookings Drive, St. Louis, MO 63130-4899 USA. Phone: (314) 935-8486, Fax: (314) 935-4156, e-mail: berliant@economics.wustl.edu

As a younger and more naïve reviewer of the first volume of the *Handbook* (along with Thijs ten Raa, 1994) more than a decade ago, it is natural to begin with a comparison for the purpose of evaluating the progress or lack thereof in the discipline.[1] Then I will discuss some drawbacks of the New Economic Geography, and finally explain where I think we should be heading. It is my intent here to be provocative[2], rather than to review specific chapters of the *Handbook*.

First, volume 4 cites Masahisa Fujita more than the one time he was cited in volume 1. Volume 4 cites Ed Glaeser more than the 4 times he was cited in volume 3. This is clear progress.

Second, since the first volume, much attention has been paid by economists to the simple question: "Why are there cities?" The invention of the New Economic Geography represents an important and creative attempt to answer this question, though it is not the unique set of models capable of addressing it. The narrow focus of this review will be on the New Economic Geography, namely applications of the Dixit-Stiglitz (1977) model to the urban context, as opposed to other models or market failures that are sometimes included in the definition. Though I will touch on other subjects, it's necessary to focus in order to bound the length of this note. The essays in the *Handbook* dealing primarily with the New Economic Geography are those by Ottaviano and Thisse, Baldwin and Martin, and Head and Mayer. Many of the other essays in volume 4 discuss this literature, for example in motivating their work (e.g. Holmes and Stevens). It also seems to comprise the main body of modern urban economic research.

For those entering the field, be they graduate students or faculty, I enthusiastically recommend the essays in this volume. It is a unique opportunity to learn, straight from some of the researchers who created it, the New Economic Geography and its implications.[3] The editors have done an excellent job of herding cats. It should be noted that the various authors are reluctant to cite the early, original work of Fujita (1988), Abdel-Rahman (1988, 1990) and Abdel-Rahman and Fujita (1990) in this area that pre-dates Paul Krugman's entry into the field. Their direct ancestor is Hesham Abdel-Rahman's dissertation, written under the direction of Masa Fujita at the University of

[1] Apparently a draft of the old review (prior to the removal of offensive language) had an impact on the current book editor of this journal, who made this suggestion.

[2] Though not as provocative as Larry Summers.

[3] First best would be to have seen the papers presented amidst the casino in San Juan at the RSAI meetings, so the *Handbook* is actually second best.

Pennsylvania Regional Science Department and completed in 1987.

Thijs ten Raa and I concluded our review with a series of complaints about the first volume and the regional science literature more generally. Regional science seemed to be focused on technique rather than on attacking interesting problems. It borrowed such techniques from fads in applied mathematics, and used them without giving serious consideration to whether the models are consistent with any sort of individual optimizing behavior or whether prices matter at all to agents. The latter, of course, is easily testable; as a reviewer, I continue to receive many papers that simply *assume* that land prices and wage rates are irrelevant to agents. I'm sure that these regional scientists pay no attention when they buy houses!

As most of the essays in the new volume are written by researchers with training in economics, most of these complaints don't apply. But the first does, and I have some further complaints directed specifically at the New Economic Geography. In my role as reviewer and reader of journals, I have seen many manuscripts whose motivation is not an economic question, but rather to extend a common paradigm such as the New Economic Geography in infinitely many minor variations. Although this might be a safe strategy for authors (particularly young authors), I am afraid that this phenomenon can crowd out riskier and more creative research by establishing certain models as central. Then papers using them are perceived as publishable. In sum, I am afraid that this new volume will further encourage less risk taking, though I hope not.

Since the major focus of modern economics is on market failures, and the New Economic Geography is the study of the consequences of a particular market failure in a particular model, the essays in the volume should be of interest to the general economist. What I fear most is that the generalist would get a one sided view of the New Economic Geography. Every class of models has its advantages and disadvantages in addressing economic questions. Naturally, the authors of this volume, who have nurtured the New Economic Geography from infancy, have an attachment to the paradigm, and thus have emphasized its advantages. For the general interest reader, I think that it will be useful to discuss here some of its disadvantages.

- Robustness. As a theorist, I'm not used to relying on particular functional forms for results. These are usually called "examples," not "theorems." If one talks with people working in general equilibrium theory (I do this as an antidote to geographers), they think that this type of model is pretty silly. How can we draw general conclusions about urban policy from these models if the conclusions change when the utility functions or functional form of transport cost change? Certainly, examples are a first step in a research program. But they are usually not the last.[4] And they are sparse in the parameter space, so they might not be representative.

- Indeterminacy. That is, the presence of equilibria that are not locally unique. I think that this is a problem when there is a continuum of locations - there appears to be a continuum of equilibria in that case. Without determinacy, comparative statics are pretty much a lost cause. It's also very hard to test a model with a continuum of equilibria, both because comparative statics can't be used, and because the equilibrium prediction consists of many possibilities. Fujita and Mori (1997) produce very original and interesting results on the implications of the New Economic Geography for central place theory, but use a complicated selection procedure to get the equilibrium they want; see section 5.2. Berliant and Kung (2004) show that generically in the specification of exogenous parameters, the set of equilibria with K cities contains a manifold of dimension $K-1$.

- Welfare. Paul Krugman seems to dislike any normative statements in the context of the New Economic Geography, though they are indispensable for policy analysis. Welfare implications of an equilibrium allocation seem to be dependent on functional form (the first point) and which equilibrium is selected (the second point).

- Commuting. Not in the model, but a large part of any urban economy. Some have argued that the New Economic Geography is a model of regions, but that doesn't mean that researchers use it as such.

- Bifurcations. This is a myth, an urban legend, perpetuated by some. A split bifurcation (as this term is used in urban economics) is the value of a parameter where the number of elements of the set of equilibria, that is dependent on a parameter value such as population or transportation cost, changes. In a more literal sense, one equilibrium splits into two. If you add enough parameters to the model, those used in the New Economic Geography are, in fact, sparse (their complement is generic). One

[4] My colleagues in political science may dispute this.

can only generate them by choosing the parameters of the model very carefully, and making sure that the dimension of the parameter space is very low (1 or 2). Kung (2004) proves this formally for the standard model.

- Tractability. Only the quasi-linear utility form of the model can be solved analytically (see Ottaviano *et al* (2002)); the other forms require computation. Of course, use of the quasi-linear form prohibits income effects, and while computations are trendy, they make life harder than analytical solutions. I'd like to see Masa Fujita and Jacques Thisse debate this point; probably they already have, in private. Better yet, I'd like to see their students debate this point.

- Empirical Evidence. From the outbreak of the epidemic, it was clear that the empirical evidence was weak. For example, my colleague Sukkoo Kim (1995) found that the model works well for the 1800's but not the 1900's. Dumais, Ellison and Glaeser (2002)[5] found that manufacturing as a whole dispersed at a rate of 3.2% every five years in the US, while textiles agglomerated. With the exception of textiles, this runs counter to the predictions of these types of models. Krugman (1991, pp. 59-61) chose textiles as his manufacturing example.[6] The chapters in the *Handbook* confirm weak support.[7]

- Empirical Connections. Some researchers look for evidence of one particular kind of agglomerative force in their data, say the one used in the New Economic Geography, but do not test this hypothesis against another, say natural advantages of location. Wouldn't competing models be of interest? Aside from issues of observational equivalence of models, to have a strong test of the model, one needs to provide a strong alternative hypothesis. Finding that regression coefficients are non-zero is rather weak, since the alternative is that they are zero. For example, the alternative could be that nothing affects agglomeration.

[5]The working paper was around for many years before publication.

[6]Gilles Duranton points out that even agglomerated industries tend to change location, contrary to a prediction of the New Economic Geography model. See Beardsell and Henderson (1999) for evidence in the computer industry.

[7]Taka Tabuchi has noted that the models of the New Economic Geography assume that skilled labor is freely mobile, while unskilled labor is completely immobile, and the latter is inconsistent with casual empiricism. See Steinbeck (1986) for evidence.

- Expanding the scope of criticism beyond the New Economic Geography, the field emphasizes steady state or balanced growth, with few analytical results on the dynamics of urban or regional development. Most dynamic analysis is again computational. See Berliant and Wang (2004) for more detail.

So where do we go from here? I want to provide a wish list, not a prediction. On the empirical side, satellite imaging promises to provide copious and precise data of interest. When combined with other data sources (e.g. Burchfield *et al* (2004)), the possibilities are exciting. On the theoretical side, there is much work to be done:

- The derivation of more testable implications to distinguish among the theories. This is emphasized in several chapters of the *Handbook*.

- The integration of theories. If the literature sticks to functional form assumptions, this is going to be hard.

- Working out the microfoundations of theories of agglomeration (my own preoccupation right now).

- Implications for other fields of economics, in particular public finance. Most of the theories of cities imply equilibrium allocations that are inefficient, due to whatever assumption is used to drive agglomeration, such as a form of imperfect competition. If we take this seriously, then all other urban phenomena must be observed thorough the lens of a pre-existing distortion. The theory of the second best comes into play. So, for example, both local and national tax policy analysis must adjust for such a distortion. This is a good test of whether urban economists actually believe their models.

- The role of information asymmetries in the urban economy, particularly adverse selection and moral hazard. This is the most important unexplored territory. It is relevant not only in the housing market, but for developers, city managers, zoning boards, and firms.

The *Handbook* provides an excellent overview of where we are. It should be used as a base from which future endeavors are launched.

References

[1] Abdel-Rahman, H., 1988, Product differentiation, monopolistic competition and city size, Regional Science and Urban Economics 18, 69-86.

[2] Abdel-Rahman, H., 1990, Sharable inputs, product variety and city sizes, Journal of Regional Science 30, 359-374.

[3] Abdel-Rahman, H. and M. Fujita, 1990, Product variety, Marshallian externalities, and city sizes, Journal of Regional Science 30, 165-183.

[4] Beardsell, M. and J.V. Henderson, 1999, Spatial evolution of the computer industry in the USA, European Economic Review 43, 431-456.

[5] Berliant, M. and F.-C. Kung, 2004, The indeterminacy of equilibrium city formation under monopolistic competition and increasing returns, Working Paper at http://econpapers.hhs.se/paper/wpawuwpur/0407011.htm

[6] Berliant, M. and T. ten Raa, 1994, Regional science: The state of the art, Regional Science and Urban Economics 24, 631-647.

[7] Berliant, M. and P. Wang, 2004, Dynamic urban models: Agglomeration and growth," in Urban Dynamics and Growth: Advances in Urban Economics, edited by Pieter Nijkamp and Roberta Capello (Elsevier).

[8] Burchfield, M., H.G. Overman, D. Puga, and M.A. Turner, 2004, The determinants of sprawl: A portrait from space, Working Paper presented at the 51st North American Regional Science Association Meetings in Seattle, WA, at http://dpuga.economics.utoronto.ca/papers/sprawl.pdf

[9] Dixit, A. and J. Stiglitz, 1977, Monopolistic competition and optimum product diversity, American Economic Review 67, 297-308.

[10] Dumais, G., G. Ellison and E.L. Glaeser, 2002, Geographic concentration as a dynamic process, Review of Economics and Statistics 84, 193-204.

[11] Fujita, M., 1988, A monopolistic competition model of spatial agglomeration: Differentiated product approach, Regional Science and Urban Economics 18, 87-124.

[12] Fujita, M. and T. Mori, 1997, Structural stability and evolution of urban systems, Regional Science and Urban Economics 27, 399-442.

[13] Kim, S., 1995, Expansion of markets and the geographic distribution of economic activities: The trends in U.S. regional manufacturing structure, 1860-1987, Quarterly Journal of Economics 110, 881-908.

[14] Krugman, P., 1991, Geography and Trade (MIT Press, Cambridge, MA).

[15] Kung, F.-C., 2004, Genericity analysis of split bifurcations, Working paper at http://econpapers.hhs.se/paper/wpawuwpge/0410008.htm

[16] Ottaviano, G., T. Tabuchi, and J.-F. Thisse, 2002, Agglomeration and trade revisited, International Economic Review 43, 409-436.

[17] Steinbeck, J., 1986, The Grapes of Wrath (Viking, NY).