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Book review

The Spatial Economy, Cities, Region and International Trade, Masahisa Fujita, Paul R. Krugman, and Anthony J. Venables (Eds.), MIT Press, 1999

The Spatial Economy is a lovely and important book that deserves close examination by all international economists. The issue that it addresses – the spatial distribution of economic activity – is of the first magnitude of importance. And the book is a testament to the fact that masters of the craft can derive an amazingly rich array of theoretical results from a remarkably small set of assumptions. Buy it, read it, mark it up.

Following some introductory material, the book has three major sections, covering regional, urban, and international economics. The part on regional economics contemplates a world in which there are immobile factors that provide a dispersed source of demand and mobile factors that potentially may serve to concentrate demand in one or a few locations. These locations are taken as identical *a priori* in all fundamentals. In this setting, they address two key questions. First, when will a symmetric equilibrium be stable? Second, when will asymmetric equilibria, once established, be sustainable? In their framework, the answers depend critically on a few key parameters, chiefly the level of trade costs, the share of spending on the good produced by the mobile factor, and the elasticity of substitution between varieties of a manufactured good.

The second section of the book, on urban economics, considers several key questions. The first builds on the classic work of von Thünen (1826), as interpreted by Alonso (1964), on land use and the structure of economic activity within a city. FKV move beyond this work to endogenize the existence of the city itself. They then go on to consider how a system of cities emerges as population grows. One chapter discusses an empirical regularity on the distribution of city sizes (of which more below), while another considers the role of ports and hub cities.

The third section of the book treats international trade proper. A distinguishing feature of this section is that factors are immobile, so cannot provide a source of demand that leads to concentration, as in the regional and urban economies.

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Instead, the source of mobile demand will be the location of manufacturing itself, via input-output linkages. They apply this to questions concerning the global structure of economic development, as well as to more applied questions such as the uneven spread of industry to developing countries. A variety of other applications are developed, including questions such as the influence of openness to trade on the distribution of economic activity *within* a country.

Since, until recently, the median international trade economist may have read no professional articles on regional and urban economics, it might be worth asking why these topics are developed in a single book and, more importantly, why trade economists should read more than the final section. A first answer is that the subjects are contained in a single volume because all of the models, with minor variation, draw on just three common analytic elements. The first is Dixit-Stiglitz preferences in the presence of monopolistic competition and increasing returns to scale. The second is Samuelsonian iceberg costs of trade. The third, and the only one that is slightly unconventional, is myopic Marshallian dynamics, in which mobile factors change locations in response to current wage differentials. One consequence of this methodological unity is that a number of key concepts, and at times key analytic expressions, turn out to be virtually the same across the various models. Thus the reader who invests in learning these central concepts can penetrate the remaining sections relatively quickly since the machinery is already in place. Moreover, this methodological unity is no accident. Once we allow for some factor mobility in our international models, the analytics of the regional and urban sections become immediately relevant.

The book is elegantly written. Still, no one will accuse it of being a light read. So what payoffs justify the effort? The most important payoff is a deeper understanding of one important explanation for the pattern of geographical economic concentration. The FKV approach focuses on a circular logic of concentration due to pecuniary externalities from demand in the presence of scale economies and imperfect competition. In the regional and urban sections of the book, these externalities come from the fact that in the presence of scale economies and costs of trade, producers want to locate near their major customers. But the mobile factors themselves are an important segment of that demand. Hence, a concentration of mobile factors may provide exactly the source of demand that makes that concentration sustainable. In the international section, factors are assumed to be immobile, so cannot themselves provide the required pecuniary externality. Instead, FKV posit that producers of differentiated manufactures also use differentiated manufactures as an intermediate input. That is, a producer of manufactures wants to be near other manufacturers both because it can sell intermediate inputs to them, but also because it can buy its own inputs more cheaply. This input-output nexus provides precisely the pecuniary externality required to again sustain concentration.

In much of the analysis, multiple equilibria are the norm rather than the exception. A first key question is whether a symmetric equilibrium will be stable.

FKV define a *break point* as a critical value in parameter space that separates cases in which the symmetric equilibrium is stable versus unstable. A second key question is whether a concentration of activity in one region, once established, will be a stable equilibrium. They define a *sustain point* as a critical value in parameter space that separates cases in which the concentrated equilibrium can versus cannot be sustained. A case in point is the analysis of the regional ''core-periphery'' model. Here a key parameter is the level of trade costs. They provide an example in which the break point is below the sustain point. In practice, what this means is that at very high levels of trade costs, only the symmetric equilibrium is sustainable. Concentrating production of the manufactured good and importing the constant returns agricultural good is simply too costly and so symmetry will prevail. When trade costs fall below the sustain point, multiple equilibria arise, and should the concentrated equilibrium ever arise, it will be stable. When the trade costs fall below the break point, the stability of the symmetric equilibrium vanishes and only concentrated equilibria are stable. While the nature of the equilibria varies with the specific application, the break and sustain points are among the unifying elements of the analysis.

The book covers a great deal more territory than can be dealt with in this review. So let me just mention a few highlights from the analysis. Surely the basic regional ''core-periphery'' analysis discussed above deserves to be in the toolkit of every international trade economist. I think that the same holds true of the outputs-as-inputs model of linkages that forms the foundation for the explicitly ''international'' segment of the book. Finally, I would recommend as a highlight the application to the problem of economic development included in Chapter 15. The basic problem considered is whether, when the rise of wages in a ''core'' region pushes some labor-intensive activities offshore, we should expect that all developing countries pursuing ''good'' policies will get some piece of the action or whether the logic of concentration requires that only a few enter the next stage of development. This seems to me to be an important insight worth further consideration.

Having already given away the fact that I think this is a very illuminating book, let me return to my duty as a reviewer to have something to gripe about. FKV spend two full chapters plus occasional comments discussing prior work. However, the exposition of prior work almost exclusively addresses the elements that they expect to mend in this volume. While such a narrow, anticipatory focus seems entirely reasonable for a journal article, this left me hungering to know what FKV think about important elements of the regional and urban literature that could not be squeezed into the paradigm of the present book.

A related problem is the paucity of facts about the world used to inform the analysis. The one exception is Chapter 12, apologetically titled ''An Empirical Digression: The Size of Cities.'' One wants to shake them by the arm and say, ''For goodness sakes – digress!'' It is a beautiful example for all theorists who plan to write books of what a great chapter can look like when you deviate from the norm of using facts only to explain how important your theory is and instead try to think hard about inconvenient facts in light of various theories. Bravo! But why, with so much talent assembled, did the authors not give us more?

What is so fascinating about Chapter 12? In it, FKV discuss an empirical regularity known as ‘‘Zipf’s law.’’ To understand what this is, start by ranking cities by population, and then take the log of the city rank. Now plot that against the log of the population itself. Astonishingly, for many countries and many time periods, the data falls neatly on a line with slope minus unity (in their calculation for the US, this wasn’t quite true – the actual figure was –1.004). To their credit, FKV are very frank about the fact that their model of city development has no tendency to produce Zipf’s law. Nor does another conventional model of relative city sizes, that of Henderson (1974), which determines size by the efficient scale of cities specialized to particular industries.

What models give rise to Zipf’s law? So far, we have only those that begin with Simon (1955) and extend through Gabaix (1999). What is distressing about these models is that while they produce Zipf’s law, they do so with virtually no economics. The key factors seem to be that growth in cities is random with a common mean and variance independent of city size. Can that be it? If this central fact is explained by a random growth model but not by the elegant models developed in the book, then what are we doing? Obviously, the Zipf relation is an inconvenient fact for their theory as well as the other theories that contain much economics. For now, it has to remain solely a puzzle. But let me applaud FKV for giving air to such an inconvenient fact. We need more inconvenient facts and the book would have been even richer if they had spent some time thinking about what actual facts we would like our models to explain.

In a book titled ‘‘The Spatial Economy’’ and devoted to economic geography, one might have expected to see more discussion of actual features of geography, physical and economic. The one concession in this direction is the chapter on ports and hub cities, which I found very insightful. Again, my chief request is ‘‘more!’’

Where does the literature go from here? FKV optimistically have a chapter titled ‘‘The Way Forward.’’ Unfortunately they offer little that is more than formulaic. After eighteen chapters of theory, they offer no more advice on the path for empirical work than that it would be nice to have some, especially if it is tied at least weakly to the theory. Perhaps such vagueness is inevitable: If they had great ideas about the path forward, then these items would have been in the book!

I will take this opportunity, then, to be more concrete about what I think should be priorities for work following on this book. For theoretical researchers, I think that we need to go beyond just mixing and matching assumptions to generate new models. We need to bring alternative hypotheses head to head with these new models to identify what is truly distinctive in the models FKV develop. For empirical researchers, breathtakingly heroic assumptions will be required to estimate structural versions of these models (cf. Davis and Weinstein (1998, 1999)). However, there is an alternative approach that will instead focus on some of the subsidiary hypotheses that arise in this literature. For example, the focus on multiple equilibria, break and sustain points, indicate the *possibility* (not a necessity) that the structure of economic activity may fail to be robust to strong shocks, even when they are only temporary. Is this a feature of actual economies? What role do features of physical geography play in the size distribution of cities? The random growth models that give rise to Zipf's law suggest the possibility of a great deal of hysteresis in city size. Is this a feature of the real world?

I am grateful to FKV for having delivered such a gem of a book. I wish that they had spent more time asking what features of the world their approach (or others) fails to explain. If the book doesn't read quite as easily as the classic volumes of Helpman and Krugman (1985) or Grossman and Helpman (1990), I am loathe to blame the authors. They have taken us a long way into difficult terrain and allowed us to see the world with new eyes.

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