

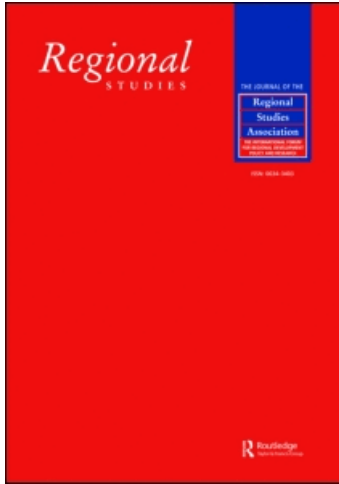
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### Of Economics and Geography: Unity in Diversity?

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## Review Essay

Edited by PÄIVI OINAS

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# Of Economics and Geography: Unity in Diversity?

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**The New Introduction to Geographical Economics**, STEVEN BRAKMAN, HARRY GARRETSEN and CHARLES VAN MARREWIKJ, Cambridge University Press, Cambridge (2009), xxvii + 568 pp. £37.00 (pbk). ISBN 978-0-521-69803-0.

**Economic Geography. The Integration of Regions and Nations**, PIERRE-PHILIPPE COMBES, THIERRY MAYER and JACQUES-FRANÇOIS THISSE, Princeton University Press, Princeton, NJ (2008), xxiv + 399 pp. US\$57.50 (pbk). ISBN 978-0-691-13942-5.

**Economic Geography. Places, Networks and Flows**, ANDREW WOOD and SUSAN ROBERTS, Routledge, London (2011), xii + 179 pp. US\$45.95 (pbk). ISBN 978-0-415-40182-1.

‘Customers who bought this item also bought *Economic Geography* by Combes, Mayer & Thisse’, it said when I intended to buy *The New Introduction to Geographical Economics* by Steven Brakman, Harry Garretsen and Charles van Marrewijk online at Amazon.com. It did not mention the recent new volume on *Economic Geography. Places, Networks and Flows* by Andrew Wood and Susan Roberts. Reviewing (and buying) these three books together thus is not per se customary – but it might be fruitful in an attempt to build an eclectic approach on space and the economy. The first two books are embedded in the geographical economics community, the third in the economic geography community.

The three books provide insights on the simultaneous usefulness of two scientific approaches that both focus on geography and economy: ‘old’ economic geography and ‘new’ geographical economics (in the literature often called the New Economic Geography). Both approaches at first sight appear potentially complementary to each other (LAMBOOY and VAN OORT,

2005). It is often argued that agglomeration economies in relation to economic growth are part of a story concerning old wine in new bottles. Indeed, economists and geographers theorizing economic growth used conceptualizations of agglomeration economies from the early beginnings of both disciplines onward, although with different timing and intensity of use. From 1991 onwards (with the seminal paper by KRUGMAN, 1991), localized productivity gains – the core determinant of agglomeration economies – that were observed for a long time already, became integrated and explained in a common micro-economic theory that since then has triggered numerous empirical extensions. Examples are the transformation from static to dynamic accounts of agglomeration, the integration of institutional and social theory in economics as well as in geography, the incorporation and explanation of knowledge and information spillovers as stylized facts in theory and empirical research, and the growing acknowledging of disequilibria in economic modelling due to endogenous growth processes (MCCANN and VAN OORT, 2009). Is there a common future for the two disciplines? KRUGMAN (2011, p. 1) recently argues that ‘no fruitful exchange between the two is expected because of the use of different methodologies’. STORPER (2011, p. 9) in a reaction argues that if the New Economic Geography improves in addressing innovation-driven agglomeration and urbanization and carefully distinguishing dynamic processes and causal forces in spatial economics, a better balance between economists’ love for parsimony and geographers’ thirst for complexity will be achieved. Similar notions can be noted in the books reviewed here. Brakman *et al.* devote a final chapter on a thorough and open discussion on criticisms and value added of geographical economics, concluding that the way forward would

be distinguishing causal processes of agglomeration from selection issues, and using micro-data, market relations and conceptualizations common in the discipline of urban economics (GLAESER, 2011). They refer for good practice to *Economic Geography* by Combes *et al.* that indeed has five chapters devoted to the empirical breadth and determinants of spatial concentration. Brakman *et al.* also see promising development paths together with institutional and evolutionary economic geography (BOSCHMA and MARTIN, 2010). It is indeed interesting to see how some of the proposed models on path dependency, selection and varying growth patterns of sectors in that line of research overlap or stem from the same core material as some of the research on geographical economics discussed by Brakman *et al.* Both books by Brakman *et al.* and Combes *et al.* devote quite some space to the policy implications of the economic theories presented. In that respect, it is interesting to notice the discussion on place-based development, where the recent WORLD BANK (2009) report building on geographical economic theory suggests, put sketchily, ‘that “place-based policies” are basically bad for development, while “spatially indifferent policies” are good for it’ (STORPER, 2011, p. 15). The European Union, in contrast, focuses on exactly place-based development as a means of economic, social and sustainable growth (BARCA, 2009). The book by Wood and Roberts stresses the plurality that has been and is represented in economic geography, treating new economic geography on only one page (p. 47), stating rather uncompromisingly that it is not new after all but that it served to revise interest in some quite traditional questions.

The two disciplines do still not appear to be happily married. Instead, they seem only to tolerate each other. Reading the three books and related literature on economic geography, geographical economics and urban economics, I have the impression that geographical economics is thinking out loud for further development and refinement to bring economics and geography more together, while economic geographers ‘proper’ focus on ever more plurality and complexity, but this book gives the impression that they actually put relatively little effort in bridging the divide towards geographical economists. Maybe the divide does not have to be bridged – but that puts an awkward light on two disciplines with the same central theme.

The recent literature on regional economic development is dominated by insights from geographical economics and the related empirical literature on agglomeration and urban economics. The main difference between geographical economics and related urban economics and agglomeration theories is that the first describes a distribution of economic activity and population resulting in different welfare effects while the latter concerns the implications of different spatial distributions of people and activity for productivity and gross domestic product levels or growth

(GLAESER, 2010). The book by Brakman *et al.* by now is an established work for introducing geographical economics, while at the same time also providing insights to and relations of geographical economics with urban and agglomeration economics. It is an important, authoritative and easily accessible textbook that, despite most other textbooks on the subject, refrains from extensive formal modelling and mathematics. Instead, besides presenting the basic geographical economics model, it focuses on applications and empirical testing. The book convincingly presents that geographical economics is based on the insights and analytical approaches that are common to (the pre-dating) new growth theory and new trade theory. In both of these strands of literature the dominant analytical approach is the modelling of imperfect competition and increasing returns to scale within the monopolistic competition framework of Dixit and Stiglitz, in which utility is a function of variety. New trade theories now allowed for the modelling of inter- as well as intra-industry trade flows within a general equilibrium framework in which the structure of demand and supply is endogenously determined. KRUGMAN (1991) first applied this modelling framework to the question of geography under conditions of economies of scale and labour mobility, and reinterpreted Marshall’s principles of externalities as stemming from the benefits of the pooling of the local labour supply and the demand for specialized non-tradable inputs. In these models, spatial concentration and dispersion were seen to emerge as a natural consequence of market interactions involving economies of scale at the level of the individual firm, with many of the results generated by these models being reminiscent of the results of central place theory and the rank-size rule (VAN BERGEIJK and BRAKMAN, 2010). This spatial version of the Dixit–Stiglitz monopolistic competition theory has since become a crucial element of spatial economists’ models on the location of economic activities. The book by Brakman *et al.* presents this model in four related chapters. After this, it shows that this framework is related to empirical research on many stylized facts: on urban systems, international business relations, institutions, dynamics and growth. Many examples and cases illustrate the material presented, and make the book a joy to read.

Besides this thorough and accessible introduction, a valuable contribution is delivered by its final two chapters on policy, evaluation, criticisms and added value of geographical economics. Several criticisms of this monopolistic modelling logic underpinning geographical economics have come from economic geography schools of thought as well as both orthodox and heterodox schools of economics. These critiques focus variously on the immeasurability of some of the notions of increasing returns inherent in geographical economics frameworks, the static nature of some of its assumptions, the specific focus on the representative firm, the presence only of pecuniary economies, and

the absence of either human capital or technological spillovers as externalities. On the other hand, advocates of geographical economics approaches argue that their analyses do provide insights into spatial economic phenomena which were previously unattainable under the existing analytical frameworks and toolkits. The authors do not spare their own field of research, and in a transparent way present which criticisms (still) hold, and which, to their opinion, are probably not so valid (anymore). They see, as mentioned, fruitful common development paths with institutional and evolutionary economic and geographical frameworks, and the empirical richness of urban economics (of which also main criticisms stem originally). As they state, one's assessment of geographical economics depends very much on what the aim of geographical economics is considered to be. KRUGMAN (2011, p. 7) remarks on this that a generation ago mainstream economists hardly thought at all about the location of production within countries; they hardly looked at local and regional data for evidence on such matters as the strength and nature of external economies. This fusion of new and old ideas also raises new questions, poses new challenges, and opens up new directions for future research.

The book by Combes *et al.* fully complies with this conclusion. It introduces geographical economics in a more formal way (not less complete than by Brakman *et al.*, but more formally presented), and from the start aims to familiarize the reader with geographical economic theories and their empirical validations. Combes *et al.* argue that although, as always in economics, everything depends on everything else, it is key to determine causality, and space and proximity are central in many theories and hence causalities. The book identifies the microeconomic mechanisms that explain regional disparities in developed countries, putting large emphasis on empirical issues that are central in urban economics and spatial econometrics, such as the unit of analysis, spatial dependence, causality, endogeneity and international network relations. The book makes clear that both (physical) infrastructure and relational firm networks play an important role in several of the recent regional economic development theories. These networks determine the shape of the space in which agglomerations develop and thereby determine the size of agglomeration externalities at a specific location. Geographical economics models are based on trade relations, travel time and road infrastructure, which determine the costs of trade between different agglomerations and regions. Inter-regional infrastructure that reduces trade costs between regions may produce different effects. It may reduce income inequality between regions when trade costs are already low, but it will increase them when the trade costs are high (BALDWIN *et al.*, 2003). Non-physical networks are potentially important as channels for knowledge spillovers and due to their possible effect on innovation. Innovation is

central to the Lisbon Strategy of the European Union. It is argued that the spatial diffusion of knowledge and its effect on innovation is of major importance for ensuring productivity growth and increasing the welfare of nations. As knowledge is hard to appropriate it generates benefits to other agents through several spillover mechanisms, like research collaborations, labour mobility, and spin-offs (MCCANN and VAN OORT, 2009).

Due to its explicit empirical and econometric focus, especially in Chapters 10–13, the book by Combes *et al.* gives an excellent overview of identification, estimation, omitted variables, reverse causality, sorting and evaluation issues related to geographical economics modelling outcomes as well as urban economics and interaction modelling outcomes. Empirical researchers will appreciate the overviews given here. The confrontation between facts and theory in geographical economics is still in its infancy. The seemingly simple issue of measuring concentration and specialization presented in Chapter 10 actually tops a large discussion on measurement, conditions, and properties of measures and indicators (cf. HENDERSON and THISSE, 2004). Chapter 10 interestingly also hints at continuous space conceptualizations as recently suggested by researchers such as ARBIA (2001) and DURANTON and OVERMAN (2005). This approach does not need an a priori regionalized dataset, and hence avoids many of the estimation problems associated with regional analyses. This kind of overviews clearly adds complementary value to the book by Brakman *et al.* The one rather strange issue of the book is its title, that according to the content should read as 'Geographical Economics and Agglomeration – Between Theory and Empirics'. The current title refers too literally to the 'old' economic geography.

An 'old' economic geography textbook is the one written by Wood and Roberts. It is of a different character than the other two books discussed so far. I was intrigued by its subtitle concerning places, networks and flows. Physical and non-physical networks are influencing developments in places to very large respects. Concerning regional development, BATHELT *et al.* (2004) have drawn attention to the potential importance of global networks as sources of knowledge in shaping firm competitiveness in an area. This international relatedness in terms of trade, knowledge and relations is actually what is missing in many empirical studies of regional agglomeration, advantage and competitiveness. The international network dimension should be brought in the analyses of regional development and competitiveness – in the words of BRISTOW (2005),

all composite measures of regional competitiveness, particularly those which develop composite rankings, fall into the trap of trying to make comparisons across very different regional entities, without exploring the extent to which these places are indeed competing with one another in tradable and commensurable markets in a manner measurable on a common scale.

The cover of the book by Wood and Roberts shows stacks of containers – and that is exactly how far the networks and flows concepts are applied in this book. Networks are mainly represented by (nodes and hierarchies of) physical transport networks. I was a bit disappointed by that, given the non-physical nature of many economic relations. As said, I was intrigued by the book's subtitle, as it suggests a focus on networks and flows – a theme that is very important for geography and economics alike, and that receives still relatively little attention in mainstream disciplines. The book does not live up to my expectation on that issue but the theme remains crucial in future development. The book sets out as a plea for a more pluralistic economic geography. While anchored in a perspective that emphasizes the social and relational – and thus inherently political – nature of economic geographies, the text also seeks to highlight the contemporary resonance and value of traditional (location) approaches. Three foci are mentioned to be central in the book (p. 3): the relationship between geographic fixity and mobility; the relationship between economic actors and the networks in which they are embedded; and the spatial nature of economic practices and networks at different spatial scales. The multiscalar economy and multilevel governance are clearly presented in a political economic setting, stressing the diversity of the field. It is the idea of political economy that the authors find most convincing in explaining economic geography (p. 11). The book further pays attention to sustainability (without clearly defining it), is illustrated with many case studies which are enjoyable to read, introduces traditional location theories properly and in each chapter ends with interesting suggestions for further reading. Throughout the book the authors deliberately avoid proposing a singular approach, but instead suggest that 'there is a vital and urgent set of issues that economic geographers are poised to tackle in a diversity of ways' (p. 137).

Thinking of the two academic fields of research on a common subject, the diversity approach proposed by Wood and Roberts does not bring the two closer together. In my opinion the empirical suggestions made in the two other books are more convincingly moving towards a more integrated approach. Whether Paul Krugman is right that the two communities will stay apart because of dissimilar research methodologies can perhaps be judged when geographical economics has matured (in another twenty years?), taking on board the suggestions made by Brakman *et al.*, Combes *et al.* and STORPER (2011). For the time being,

the books of the two disciplines are not recommended together on Amazon.com.

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