

Unsatisfactory connections between former spatial research traditions and the New Economic Geography

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Abstract

In recent years there has been an increasing interest in mainstream economics in relation to the importance of spatial aspects of economics. Models in macroeconomics and international economics, which are based on ignorance of the cost of spatial interactions, are under permanent critique. However, there are many approaches concerning the topic, of which has differences in the treatment of space, the handling of research tradition and applied modeling techniques. The main aim of my paper is to examine the approaches of the so called “New Economic Geography” (NEG) and at the same time, present the superiority of the former non-mainstream approaches.

There is no question about it, that it is a welcome change with the growing popularity of NEG for economists to pay more attention to geographic concerns. However, beside some results of NEG there are also at least four critical points in connection with them. Firstly, it disregards or misinterprets both the former researches in the spatial aspects of economy, such as German location theory, regional and urban economics, regional science or economic geography, and some part of general economics as well. Its statements are only new from the point of view of mainstream economics, but not from the rich tradition of neglected former spatial researches by location theorists and geographers. The missing connection between various researches concerned with the same topic is a sign of institutional and/or intellectual failure. Secondly, it is based on a restricted meaning of economics, namely on a method oriented approach to economics which limits our methods in reasoning of economics. Thirdly, there are many (epistemological and practical as well) problems with its highly praised modeling techniques. Fourthly, it is based on an inadequate view of space, on the network of one point economies. These problems, which could be avoidable giving more attention to spatial research traditions, are examined in detail in this paper.

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Introduction

There are two main ways to investigate spatial economy: the empirical, historical description of concrete, real places, and the abstract, theoretical models and theories of spatial economy. On one hand, people are interested in concrete historical events, on the other hand the human mind is able to abstract from the complexity of the real world, build imaginary constructs, and by seeking theoretical explanations, creates theories. Neither of these two approaches are superior to the other. Competent historical research uses theories in the explanation of real phenomena and theoretical papers use examples from empirical writings to illustrate theory.

In recent years there has been an increasing interest in mainstream economics in relation to the importance of spatial questions of economics under the name of New Economic Geography or geographical economics. This research movement, which is fast increasing, at least as regards the amount of papers, has the merit to draw attention to spatial aspects of economy. However, it suffers from serious problems which are usual in mainstream economics. I will try to show that their approach has many shortcomings, such as the ignorance of previous research on the same area, superficial epistemological background, inappropriate modeling techniques, narrow-minded method-oriented definition of economics and an inadequate view of space. Therefore, it cannot be treated as a considerable contribution to our understanding of spatial phenomena, moreover it confuses some part of history of economic thought in general and concerning spatial economic theory as well. The general problems stem from its methodological starting point. The spatial part of these problems could be easy to avoid by paying attention to former spatial research traditions and the critics from the side of spatial researchers and economists.

The emergence and the critical reception of the New Economic Geography

The emergence of New Economic Geography (NEG) was owing to the fact that neoclassical mainstream, wrapping itself up in unrealistic mathematical formalism, highly disregarded the spatial aspects of economy. Spatial problems investigated by regional science was ignored by mainstream or was treated as an unimportant or esoteric question of detail, while German location theory was known only by misleading textbook-interpretations, which concentrate mainly on the formal and geometrical parts of the spatial arrangement of economy. Critiques of the spaceless characteristics of mainstream economics from the side of outsiders (such as regional scientists, geographers, sociologists, urban economists) were fruitless efforts until one of the leading mainstream economists, Paul Krugman nailed his colours to the mast of the matter of space in his article and book in 1991 (Krugman, 1991a; Krugman, 1991b). From a marketing point of view these works are successful. Its originality, deepness and correctness will be partly examined in this paper.

Filling the gap created by ignorance of space, Krugman's ideas have several followers and at the same time they became a subject of a moderate and vehement critique also. Due to space limitations, in this paper I only concentrate on Krugman's various writings. It is not a great restriction because from the point of view of methodology there is agreement in the NEG camp. As regards the treatment of research tradition, some differences can be found in emphasizes. However, it is important to stress here that Krugman's writings, despite their author's frequent emphasis on the importance of analytical rigour, as regards the history of economic thought, is not devoid of ambiguity, inaccuracy, obscurity and superficiality. Therefore I had to quote many times from Krugman's various works.

Opinions on the NEG are at the least contradictory. Mostly there is an agreement between supporters and critics that the issue of space in economics became more appreciated through

the NEG and discussions about NEG (Brakman–Garretsen, 2003). Some supporters of NEG acknowledge the pre-Krugman roots of the ideas of NEG (Ottaviano-Thisse, 2003). Many papers mention the relative scarcity of empirical studies concerning NEG (Neary, 2001; Brühlhart–Traeger, 2003; Combes–Lafourcade, 2003; Head–Mayer, 2003; Ottaviano–Puga, 1998). As Krugman argues, ‘a theory must survive or be discarded based on its empirical relevance. So empirical and quantitative work is clearly the next geographical frontier’ (Krugman, 1998, p. 16). This is a laudable ambition, but I am sceptical about its success, knowing the neoclassical interpretation of empirics and the history of neoclassical theories, and regarding the abundance of unreal assumptions. The main problem is that there is no real dialogue between the supporters and the critics of the NEG. The critics refer to Krugman and his followers but the followers of NEG typically seem to take no notice of critics.¹

Among the critical writing² the most comprehensive is Martin’s paper (1999b). According to Martin: ‘To geographers, the ‘new economic geography’ being promoted by economists has very little resonance with the theoretical or empirical concerns of contemporary economic geography proper. Rather, it represents a reworking of regional science and urban economics models, precisely the sort of approaches that geographers discarded years ago. The mathematics on which the ‘new geographical economics’ is being built may be sophisticated, but most geographers would view the results as not particularly novel and the empirical applications trivial’ (Martin, 1999b, p. 67). ‘These models do not lend themselves easily to empirical estimation or application, since they are typically too abstract, over simplified and too idealised: too much is held constant or ignored to allow the models to be meaningfully applied to, or tested against, the real world’ (Martin, 1999b, p. 70). Bryan Berry expresses his opinion in a less formal way in an interview: ‘The only reason it is called the New Economic Geography is that the proponents only talk to each other. They do not read the literature, they have no sense of intellectual tradition. (...) Krugman never cites his sources’ (Berry, 2004, p. 10-11).

Besides ignoring real places and the lack of testing of NEG models, the following criticism of NEG has been mentioned in literature:

1. Assumptions concerning economy (for example homogeneous products, capital, labour force; costless interregional transport of capital);
2. epistemological problems of mathematical formalism;
3. handling of history and time;
4. neglecting of institutional factors;
5. treatment of space;
6. the ignorance of issue of scale;
7. undefined character of fundamental spatial objects (cities, regions)
8. its right statements are trivial and long ago known among spatial researchers
9. ignorance, disdain or misinterpretation of former research about spatial (urban, transportational) aspects of economy;
10. colonisation of economic geography by economics.

The first seven problems have methodological or theoretical character, the last three indicate a failure of scientific communication. As regards the questions of details, the amount of criticism is much wider. It is a very regrettable situation that the critique mostly remains without answer. With an answer it could be shown that the critics miss the targets or they stem maybe from misunderstandings. It is not the task of my paper to examine the validity of

¹ There are some exceptions, see Brakman et al. (2001) and Overman (2003).

² See for example Bathelt (2001), Boddy (1999), Dymiski (1996), Goodacre (2005), Hoare (1992), Isserman (1996), Koschatzky (2002), Martin (1999a), Martin (1999b); Martin–Sunley (1996), Olsen (2002), Berry (1999), Berry (2004).

this criticism in all details. I deal only with the treatment of economics, the problems of modeling techniques, treatment of space and the treatment of research traditions.

The method-oriented definition of economics

It is useful to begin the concrete critique with the very starting point of all other mistake. According to Krugman in our time mathematical formalism is the only scientific way in the investigation of economic questions. ‘A rise in the standards of rigor and logic led to a much improved level of understanding of some things, but for a time it also led to an unwillingness to confront those areas that the new technical rigor could not yet reach’ (Krugman, 1995, p. 3). ‘Since economics as practiced in the English-speaking world is strongly oriented toward mathematical models, any economic argument that has not been expressed in that form tends to remain invisible’ (Krugman, 1990, p. 3). This statement is proved by Krugman’s works, where the assumptions are determined by analytical tractability and not by empirical relevance. It is an unproved, very general statement as well, that a rise in rigor led to a much improved level of understanding some things.³

However, Krugman is quite arrogant against the critics of formal models. He accuses the critics of political bias and mathematical unskillfulness (while his applied mathematical apparatus is very elementary from a mathematical point of view). ‘Many of those who reject the idea of economic models are ill-informed or even (perhaps unconsciously) intellectually dishonest’ (Krugman, 1995, p. 79). He refers to using formal models in practical political application as well. I think this is rather an unfortunate situation and not a proof of usefulness of formal models. As Krugman repeats it many times, verbalism is archaistic, murky, boring and unscientific. With this entirely false approach to verbalism it is interesting to quote Neary (2001), who in his rather advocating paper writes about Spatial Economy (Fujita et al., 1999) that ‘the authors have made a huge effort to expound the model clearly, but even they cannot prevent the calculations from degenerating at times into a near-impenetrable soup of CES algebra’ (Neary, 2001, p. 537).

According to Krugman, formal mathematical models, which are built on highly unrealistic assumptions, can be used in practical political proposals. The fundamental mistakes and dangers of this approach has been proved countless times, therefore I do not go into details about it. This mentality, which is based on an unjustifiable preconception on scientific method, limits the topics which can be investigated by science. This is admitted by Krugman himself also, but it does not mean any problem for his.⁴ The fact, that formal models are used for practical political proposals is not proof of its usefulness (as I mentioned), quite the contrary, it is only a sign of intellectual confusion. The method-oriented definition of economics has other specific shortcoming also, namely, it led to disregarding, misinterpreting or undervaluing the ancestors of scientific ideas, if it is not expressed in a formal way. Krugman’s observations on the history of economic thought can be accepted only from his particular, narrow-minded, method-oriented, restricted interpretation of economics.

³ Krugman uses rigor and logic in a very restricted way, he means the usage of algebraic formulas. However, I prefer the original meaning of rigor and logic, namely that we use unambiguous concepts and we prove our statements by the help of logical rules, apart from basic propositions. About the effect of rising rigor (in Krugman sense) it is interesting to quote Kaldor: ‘Since Walras first wrote down his system of equations over 100 years ago, progress has definitely been backwards not forwards in the sense that the present set of axioms are far more restrictive than those of the original Walrasian model. The ship is no nearer to the shore, but considerably farther off, though in a logical, mathematical sense, the present system of derived tautologies is enormously superior to Walras’s original effort’ (Kaldor, 1985, p. 13).

⁴ See Krugman (1991a), pp. 4-7; Krugman (1995), pp. 3-6.

Modeling techniques of NEG

In their book *The Spatial Economy* (1999), Fujita, Krugman and Venables mention four modeling tricks, which are used in NEG in order to manage the ‘technical difficulties’ involved in trying to deal with the subject. These tricks have been repeated in every Krugman paper about the topic. ‘Everyone recognizes that these are strategic simplifications, which is to say intellectual cheap tricks; but they do allow us to get past the technical issues and tell stories about the real economics’ (Krugman, 2000, p. 51).⁵ These tricks are in short: Dixit-Stiglitz model of monopolistic competition, icebergs, evolution and the computer. Firstly I present Krugman’s opinion about them, then I will demonstrate their weakness.

It is a permanently recurring thought in Krugman, that previous work in spatial analysis was based on constant returns and perfect competition. ‘Now of course the von Thünen model, like the bulk of economic models between 1820 and 1970, focused on the case of perfect competition and constant returns’ (Krugman, 1995, p. 76). However, the question is a little bit complicated, because Krugman is not entirely unambiguous in this topic. ‘New trade theory is an approach to international trade that emphasizes precisely the features of the international economy that traditional trade theory leaves out: increasing returns and imperfect competition’ (Krugman, 1990, p. VII). It can be known from other parts of Krugman’s book that in his strange jargon ‘traditional trade theory’ means neoclassical trade theory à la Samuelson that is pre-Krugman trade theory. However, Samuelson’s trade theory is for Krugman, equal to classical trade theory from the increasing returns and perfect competition point of view, the only difference is that the former is superior to later because of formalism. I think this is a gross misinterpretation, because classical trade theory is not based on perfect competition and for classicals the idea of division of labor and the principle of comparative advantages is only expressing the idea of increasing returns in another way. (I return to this question later.) In the 8 page introduction of ‘Rethinking International trade’ the phrase increasing returns can be found 27 times. On page 11 Krugman is more cautious, he restricts this assertion to *formal* trade models: ‘Nonetheless, increasing returns as a cause of trade has received relative little attention from formal trade theory. The main reason for those neglect seems to be that it has appeared difficult to deal with the implications of increasing returns for market structure’ (Krugman, 1990, p. 11). This shortcoming, according to Krugman, can be managed with the help of the Dixit-Stiglitz model of monopolistic competition: ‘The remarkable model of monopolistic competition developed by Dixit and Stiglitz (1977) has become a workhorse in many areas of economics. In the new economic geography, it has one especially appealing feature: because it assumes a continuum of goods, it lets the modeler respect the integer nature of many location decisions – no fractional plants allowed – yet analyze the model in terms of the behaviour of continuous variables like the share of manufacturing in a particular region. In effect, Dixit-Stiglitz lets us have our cake and cut it into arbitrarily small pieces, too. The price of that convenience is, of course, that Dixit-Stiglitz is a very restrictive, indeed in some respect, silly model’ (Krugman, 1998a, p. 164).

The problems arising from this interpretation can be divided into three parts: the uselessness of the model itself, the assertion that previous models were based on perfect competition, and the origin of the idea of increasing return. As regards the first point, I do not go into detail, but it is worth to emphasize, that the Dixit-Stiglitz model is nothing other than applied mathematics, which has nothing in common with reality and does not help understand economic problems. It uses mathematical concepts like social indifference curves, concave utility functions, constant elasticity subutility function and same, which do not have any contact with real economic phenomena. It can be admitted that the Dixit-Stiglitz model

⁵ The weakest point of this argument that it disregards the types of assumptions (or simplifications).

contains a little bit more of a slice of reality than the model of perfect competition, but in spite of this it does not deal with individual behaviour and entrepreneurial competition.

As Fujita says in an interview with him and Krugman, ‘as is well-known in modern economic theory, scale economies are inconsistent with perfect competition on which von Thünen’s model of agricultural land use was based’ (Fujita–Krugman, 2004, p. 155). This assertion is repeated several times in various papers of NEG. Ambiguous to state that Thünen’s model was based on perfect competition. On the one hand, Thünen’s model of concentric rings was based on a cost space in which there is only one transportation cost and everyone has a perfect knowledge about this. However, this is not equal to perfect competition. Thünen was aware of the fact, that his model is an idealization of the agricultural land-use pattern, he examines the differences between his idealization and reality (Thünen, 1930, pp. 264-324, and other sections as well). This is not a typical neoclassical feature. On the other hand, it is a self-contradictory assertion about a spatial model to be based on perfect competition. One of the many unreal assumptions of perfect competition is non-spatiality. Space cannot be made consistent with perfect competition. One part of earliest critiques of perfect competition was expressed by spatial researchers, for example Palander in 1935 (Palander, 1935, pp. 275-278). If not earlier, at least from that time it should be the incompatibility of perfect competition and spatiality an worn out question.

There are other problems with that frequent assertion, that the former (‘pre-Krugman’) spatial research was based on perfect competition. As Machovec demonstrates in his excellent book, rooted in Walrasian static equilibrium, ‘the perfect competitor is entirely a creature of the modern neoclassical mind’ (Machovec, 1995, p. 242)⁶. It is highly unhistorical, indeed nonsense to say about Thünen and every other spatial researcher, that his model was based on neoclassical perfect competition.

As regards increasing returns, it is an ancient idea anchored in the first ancient writings about economy. Plato in his Republic clearly formulates this idea, writing about the origin of state. The possibility of increasing returns belonged to the category of evident facts until the ascendancy of neoclassical formalism.⁷

Krugman’s chain of thoughts in connection with perfect competition and constant returns can be summarized as follows:

1. He declares that there is an essential difference between formal and verbal expression of the same thought.
2. He permits scientific status only to the formal expression. The adverbs formal, neoclassical and scientific have the same meaning.
3. He repeats a thousand times that previous works were based on the assumption of constant returns and perfect competition. Thanks to his reputation, through the repetition of his followers in every paper it became a believable (but incorrect) interpretation.
4. He seldom mentions that this is only true to *formal* (neoclassical, scientific) treatment of spatial analysis.
5. Neoclassical theory is formalized reformulation of classical (non-scientific) theory, therefore classical (non-scientific) treatment was also based on the constant returns and perfect competition.

⁶ Beside Machovec’s comprehensive survey there is many other papers which deals with the origin and consequences of this popular misinterpretation in the history of economic thought. See for example Blaug (1997), Hutchinson (1999).

⁷ Since Krugman himself made much for popularizing the mistake, namely that increasing returns have been recently discovered, writes also: ‘to those who imagine that increasing returns are something only recently discovered, it is startling to see how much attention is given in Marshall’s Principles to local externalities’ (Krugman, 1995, p. 50).

The dogmatic elements of this reasoning can be found in the first and second point. Krugman's interpretation can be accepted if and only if these dogmatic basic postulates are accepted, because this restricts in a drastic way the works which belong under the territory of economics. Between the fifth and previous points a logical connection does not exist.

Krugman writes about icebergs, that 'in location theory, transportation costs are of the essence; yet any attempt to develop a general-equilibrium model of economic geography would be substantially complicated by the need to model the transportation as well as good-producing sectors. Worse yet, transportation costs can undermine the constant demand elasticity that is one of the crucial simplifying assumptions of the Dixit-Stiglitz model. Both problems can be sidestepped with an assumptions first introduced by Paul Samuelson (1952) in international trade theory: that a fraction of any good shipped, simply 'melts away' in transit, so that transport costs are in effect incurred in the good shipped. In the new economic geography models, melting is usually assumed to take place at a constant rate per distance covered' (Krugman, 1998a, p. 165.).

In this short quotation the methodological weaknesses of neoclassical model building technique can be observed. Krugman aggregates the economic actors into two sectors, agriculture and industry, thus there isn't space for a transportation sector. (There is neither space for the heterogeneity of products and firms. By the way, the aggregate treatment of industry in itself questions the whole NEG, because in the industrial agglomeration the industrial branches play a fundamental element and not the industry in general.) With this treatment of transportation the transport sector uses the same inputs as the transported goods. It is easy to demonstrate that this treatment of transport cost leads to convex delivered prices (Table 1). However, every empirical result confirms that the concave form is typical (McCann, 2005). Where is in the iceberg assumption the increasing returns, is the diversity of transportation modes, where is the diversity of transportability and weight of goods? Are the transportation costs the same for brick, cement and microchips? Moreover, it implies that transport costs are directly proportional to the price of shipped goods, which is unrealistic also. Lastly, transportation costs are only a part of transaction (or exchange or interaction) costs. Transaction costs play a central element not only in divisions of labour and the theory of firm but in location decisions and the process of agglomeration.

'Evolution refers to how one thinks about how economy 'selects' one of several (or many) possible geographical structures. It is typically true of new economic geography models that they have multiple equilibria' (Krugman, 2000, p. 52). The problem lies not in the trick itself, but the treatment of the connection between theory and history. This question is also examined when this trick is discussed. In selecting the geographical structure, historical accident plays an important role. 'To a remarkable extent, manufacturing industries within the United States are highly localized; and when one tries to understand the reasons for that localization, one finds that it can be traced back to some seemingly trivial historical accident' (Krugman, 1991, p. 35) 'For at least insofar as the location of economic activity in space is concerned, the idea that an economy's form is largely shaped by historical contingency is not a metaphysical hypothesis; it is simply the obvious truth' (Krugman, 1991, p. 100). It is useful to emphasize that the main aim and task of theories and models is not the prediction or explanation of concrete, individual events. Categorical difference between theory and history is not registered by Krugman. The previous statements are only true if we consider the pure historical explanation of spatial events. The key task of an economic historian is (in one of Krugman's examples) to investigate the important elements, which are in the background of the establishment of the carpet manufacturing firms in Dalton after World War II. However, the task of theoretical explanation is the exploration of the reasons of industrial concentration in general. Any competent economist can explain by the help of theory why and how the agglomerations come into existence in various branches of industry. However, theoretical

explanation has its *a priori* limitations, namely no theoretical economist can explain why a particular firm was organised in a certain location and time without converting himself a historian and investigating concrete events.

Table 1 Unit price of good as a function of distance (melting: 1% per distance unit)

Distance unit	amount of delivered good	unit price of good	Absolute change in amount (per 10 distance unit)	Absolute change in price (per 10 distance unit)
0	100	100	-	-
10	90,4	110,6	-9,6	10,6
20	81,8	122,3	-8,6	11,7
30	74,0	135,2	-7,8	12,9
40	66,9	149,5	-7,1	14,3
50	60,5	165,3	-6,4	15,8
60	54,7	182,8	-5,8	17,5
70	49,5	202,1	-5,2	19,3
80	44,8	223,5	-4,7	21,4
90	40,5	247,1	-4,3	23,6
100	36,6	273,2	-3,9	26,1
110	33,1	302,1	-3,5	28,9
120	29,9	334,0	-3,2	31,9
130	27,1	369,3	-2,9	35,3
140	24,5	408,4	-2,6	39,0
150	22,1	451,6	-2,3	43,2
160	20,0	499,3	-2,1	47,7
170	18,1	552,1	-1,9	52,8
180	16,4	610,5	-1,7	58,4
190	14,8	675,0	-1,6	64,5
200	13,4	746,4	-1,4	71,4

Krugman writes little about the ‘computer trick’: ‘despite the best efforts of the theorist, all but the simplest models of economic geography usually turn out to be a bit beyond the reach of paper-and-pencil analysis. As a result, the genre relies to an unusual extent on numerical examples – on the exploration of models using both static calculations and dynamic simulations’ (Kugman, 1998a, p. 165). It is undeniable, that simulations with the help of a computer can help many complicated theoretical and practical problems, for example in technical, genetical or biological problems. However, simulations in ‘Spatial Economy’ say nothing about real spatial questions. Simulations are used describing ‘temporal processes’ without defining time scale (minute or millions of years). The main problem is however, that the formulas in mathematical equations of NEG cannot be connected with real world phenomena, while the connection can be got through in technical problems.

It would be a hard task to decide, which is the weakest trick of NEG. However, in NEG there are problems not only with these four modeling tricks. There are many other tricks as well. In spite of the emphasis on increasing returns and imperfect competition, in NEG models, agricultural good is produced using a constant-returns technology under conditions of perfect competition. Every consumer shares the same Cobb-Douglas tastes for the two types of goods (Fujita et al, 1999, p. 46); intraregional transport costs do not exist (it is unsaid assumption), the employers are homogeneous, the capital is homogeneous, the transport of capital and agricultural goods between region is costless, migration choices base on current

wage differences alone and so on. As regards to the main point, space itself, I deal with it in the following section of the paper.

The treatment of space

Spatial research often begins with a critique of the standard imaginary spaceless world of economics. This is true of NEG also. As ‘Spatial Economy’ begins: ‘Mainstream economics has traditionally paid remarkably little attention to the location of economic activity’ (Fujita et al, 1999, p. XI). The legitimacy of this type of criticism depends on three questions. Firstly, what is the impact of spacelessness on the validity of the theories? Secondly, is there a theory-building process in which spatiality can be apparent without its explicit reference? Thirdly, is the treatment of space adequate or not from the spatial problems’ point of view? Are the effects of assumptions concerning space to the applicability of theories thoroughly examined by theory building? The first and second very important general questions can not be investigated here. I deal only with the third question.

In NEG the most common definition of space is the discrete two-point-economy.⁸ The evident problem of this approach can be seen on Figure 1. The number of individual actors and their connections are much larger than those of the connections of aggregated regions, but these are not possible to depict. There can be legal, institutional, constitutional differences between intraregional and interregional flows, if regions are countries, or cultural, language, social barriers inside the same country. This was the real argument for a separate theory for international trade for classical economists, such as Ricardo and Cairnes. Only institutional factors would give reasons to treat regions as points, and the space between regions as ‘vacuum’. However, models of NEG do not really deal with institutional (and language, custom, culture and so on) factors, neither with the geographical unevenness of space, but only with pure economical factors.⁹ When working with implicit point concept, the usage of the word ‘region’ is an unjustified custom in neoclassical mainstream. In the case of legally and mentally free movements of persons and goods, the sources of differences can mainly be traced back to the differences of cost distances. It is a common misconception not only by the advocates of NEG, but in international economics and regional science also, to treat the spatial units as individual behavioural units, without their own spatial extent, without their internal complexity. Regions, similar to countries, merge economical activities and factors, which are spatially, temporally, in their degree of quality, in quantity and in behaviour, heterogeneous. The connection between macro variables is treated in NEG models as simply a connection between micro variables.

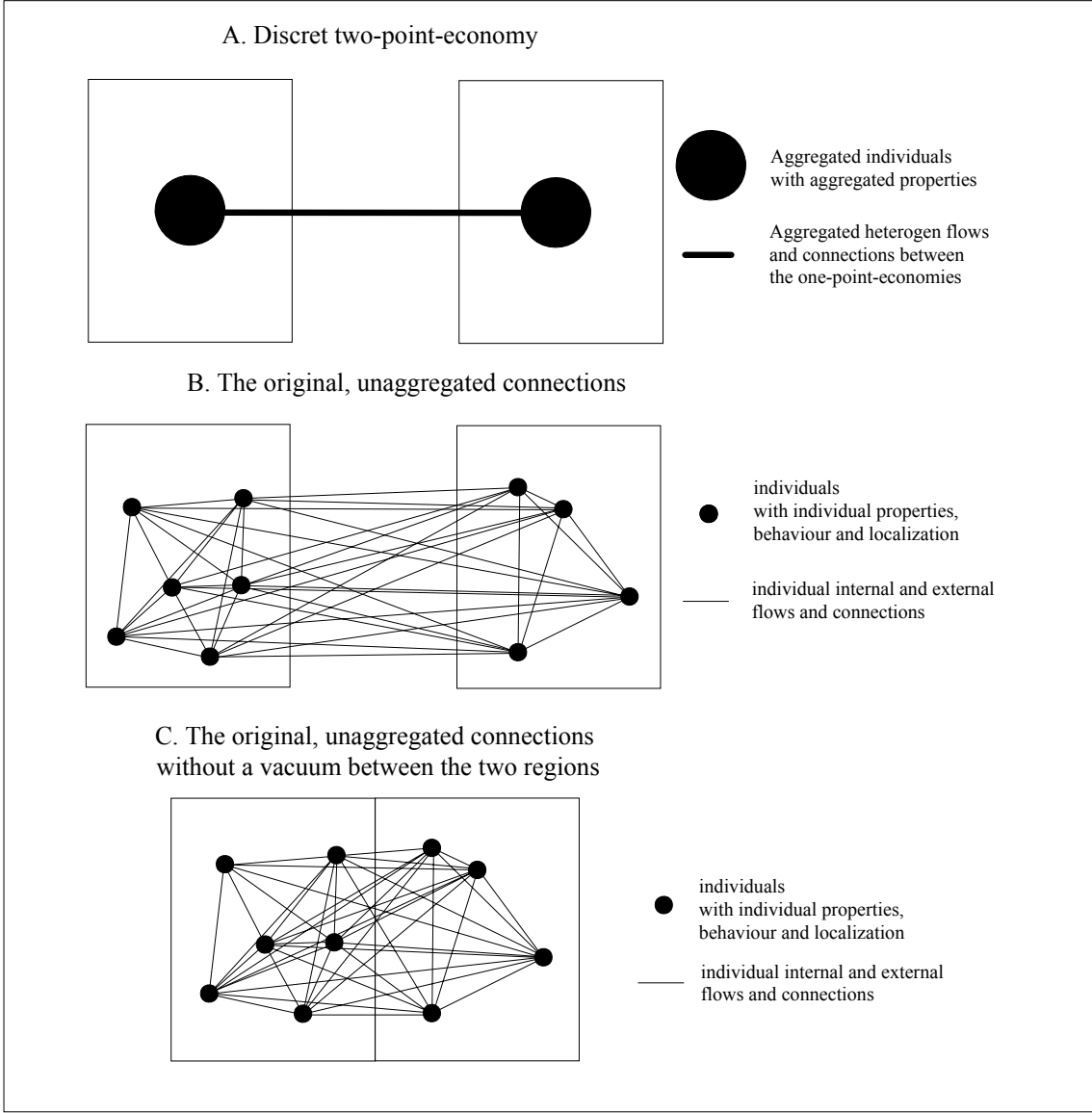
In the treatment of transportation costs, it typically uses various constrained unreal assumptions, such as there aren’t transport costs of agricultural products (or the only *one* undifferentiated agricultural product), transportation costs depend only on physical distance, differences in weight and quality do not play any role, and, of course, there isn’t intraregional transportation costs. The latest assumption (which is an implicit consequence of spatial

⁸ It is an implicit approach. There is another possible interpretation, namely that regions are perfectly homogenous surfaces; inside the regions there aren’t any differences, going over the border there is an abrupt changes in prices, costs and so on. The two-point-economy and two-homogenous-surface-economy approaches are false to the same degree.

⁹ Fujita et al. (1999) maintains once that ‘We are really interested in all of the costs of doing business over geographical space. In other words, we want for the theory a measure of the full cost, including all the cost of doing business at a distance – lack of face-to-face contact, more complex and expensive communications and information gathering, and possibly also different languages, legal systems, product standards and cultures. These things are difficult to measure directly but are revealed in the trade data’ (Fujita et al, 1999, p. 98). This remark does not substitute the real investigations of that factors which was mentioned in the quotation. In formulas of NEG models these factors do not play any role.

aggregation) means that, for example, transportation costs are the same between England and Wales, independent from the starting and end point of transport (for example transport costs are the same between York and Swansea and between Bristol and Cardiff). It is an obvious fact for spatial researchers, that there are a lot of methods to count average distances between two regions. However, averages are deceptive in the case of behavioural differences.

Figure 1 The two-point-economy



Other space definitions of NEG are more specific, but similarly inadequate. The ‘racetrack’ economy means that space is a bounded, one dimensional circle. This type of space is introduced in ‘Spatial economy’ in a very encouraging title: ‘Many Regions and Continuous Space’ (Sixth Chapter). However, in reality many regions mean that: ‘The R regions are equally spaced around the circumferences of a circle, with region r+1 next to region r, and with region R next to region 1. Agriculture is evenly divided among the regions. Transportation must take place around the circumference, with a constant fraction τ of each manufactured good melting away per unit distance’ (Fujita et al., 1999, p. 82). The title of the chapter is misleading; under continuous space, I mean not such a restricted concept of space, but a two dimensional space without arbitrary delimitations and aggregations.

The third type of space is a continuous one dimensional line with one center or ‘city’ (‘Thünen Economy’). (“We consider a long, narrow economy – effectively one-dimensional – that stretches sufficiently far that we can disregard boundary conditions’ (Fujita et al., 1999, p. 134). In this subtype of space there is more than one center. The fourth type of space is defined in a fuzzy, vague way, namely, there is R region with various constrained assumptions concerning the movements of goods and factors. This means that there is R one-point-economies. However, it cannot be known, if this space has one or two dimensions.

The fundamental mistake of all conceptions of space of NEG is that practically it treats regions as a natural given behavioural unit. In reality, the boundaries of regions are modifiable, the economic regions are not separated sharply from each other, but they have an overlapping character. Beside the modifiable areal unit problem, it is only partially examined, what is the effect of various restrictive assumptions concerning transportation and space. The generalization and the applicability of ‘results’ to any real problem is an uninvestigated issue. I do agree with Martin: ‘The fundamental and complex question of how ‘regional’ and ‘local’ economies can be meaningfully conceptualized, and how such conceptions can be translated into empirical terms, is not considered at all. Instead, there is an ontological slippage between regions as abstract points and spaces, on the one hand, and the uncritical use of whatever administrative units happen to be convenient for illustrative and empirical purposes, on the other’ (Martin, 1999a, p. 77-78). The declared program of NEG, namely to trace back the spatial arrangement of economy to the interaction of decisions by individuals, do not come about, because there isn’t differences in behaviour (same size of firms, same ‘Cobb-Douglas’ tastes etc.). With these homogeneity assumptions only a pseudo micro-foundation can be attained.

Answering the third question, I think this treatment of space is inadequate from the spatial problems’ point of view. Most parts of criticism concerning dimensionless aggregated one-point-economy is valid to two-point-economy, one-dimensional line economy, monocentric economy and network of one-point-economies. In the adequate analysis of spatial phenomenon one has to think about regions as overlapping entities and to use zoning-system-independent continuous space view, which is built by individual locations and individual actions and movements.

The treatment of research traditions

There are two main objections to the treatment of research tradition by NEG. Firstly, the disregard of the overwhelming part of extraordinarily rich research tradition in the area of spatial research by economists, regional scientists, geographers and so on. Secondly, the misinterpretation of some part of former spatial research. As regards the individual researchers, these two types of mistakes can be combined. For example, Lösch or Ohlin are often mentioned, but the interpretation of their view is grounded not on the original works of Lösch’s and Ohlin’s, but on the caricature of their views which are presented in the textbooks. The critique of previous works plays an important role in science (this paper is also mainly a critical review), and the original works, of course, could be an object of competent critique, which could contribute to a deeper understanding of spatial arrangement of economy. This competency, concerning previous works of regional economics, can hardly be observed in the literature of NEG. In this short review I will briefly discuss the content of references to Ohlin and Thünen, only concentrating on the most important parts of contestable questions.

Writing about Ohlin, Krugman himself confirms what I have written and writes about the reasons for disregarding previous research: ‘Let me begin with an embarrassing admission: until I began working on this paper, I had never actually read Ohlin’s *Interregional and International Trade*. I suppose that my case was not that unusual: modern economists, trained

to think in terms of crisp formal models, typically have little patience with the sprawling verbal expositions of a more leisurely epoch. To the extent that we care about intellectual history at all, we tend to rely on translators – on transitional figures like Paul Samuelson’ (Krugman, 1999). It is strange to me this flaunting, indeed boasting of ignorance. Samuelson was not a translator of Ohlin, but a transformer of Ohlin. He put out with his primitive 2*2*2 formulation of international trade the richness of Ohlin’s thoughts. Everybody knows, who has read original Ohlin, that Heckscher-Ohlin theorem has nothing in common with Ohlin’s *Interregional and International Trade*. Ohlin dealt with increasing returns (in transportation costs also, because in his theory transportation costs play a significant role), with institutions, with labour unions and many more topics, that can be read in the whole literature of NEG. Surprisingly, Krugman argues that ‘Ohlin actually gains something in the translation: Samuelson famously found implications in Ohlin’s view of trade that the great thinker himself, due to his ‘diplomatic style (...) had missed’ (Krugman, 1999).

Judgement about German location theory and Isard’s regional science is obviously based on superfluous knowledge about these research traditions. In *Spatial Economy* (and in previous works also), the Thünen model is criticized because ‘it simply assumes the thing you want to understand: the existence of a central urban market. Indeed, the whole thrust of the model is to understand the forces that spread economic activity *away* from that center, the ‘centrifugal’ forces if you will. About the ‘centripetal’ forces that create centers, that pull economic activity together, it can and does say nothing’ (Krugman, 1995, p. 53; on further pages – till page 59 – there are other misunderstandings also about Thünen; this argument is repeated in *Spatial Economy*, p. 17-18). Thünen’s aim with concentric rings was to give explanation to agricultural land-use patterns. To reach this goal, he temporarily assumed the existence of a central town. He deals with centripetal forces in other parts of his study. After writing ‘*Spatial Economy*’, Fujita read Thünen’s book: ‘Recently, however, I had the chance to carefully read the English translation of von Thünen’s work by Wartenberg (1966), and found that von Thünen himself provided a very systematic account of most factors explaining economic agglomeration. *Interviewer*: Really? I never heard such a thing. *Krugman*: It is also news for me. The *Isolated State* then is another classic to which everybody refers but few have actually read. *Fujita*: This is a good chance to remedy the history of location theory. Economic geographers and location theorists always refer to von Thünen, but (to the best of my knowledge) never in the context of agglomeration economies or city formation’ (Fujita-Krugman, 2004, p. 153).

In the literature of NEG citations it can hard to find contemporary traditional economic geographers. The citation practice of researchers of NEG vastly confirms Brian Berry’s opinion that the proponents of NEG only talk with each other. The citations to ‘classics’ (like Thünen, Marshall, Weber, Lösch) have an aim to make a sharp distinction between them and NEG, and to demonstrate the superiority of the NEG approach because of more theoretical rigour (defined in Krugman’s restricted way). Research ‘tradition’ in NEG often begins only with the contemporary works of Krugman. For example, Roos writing about the ‘first and second nature’ of agglomerations (it is Krugman’s innovation in terminology), ascertains that ‘one salient feature of the New Economic Geography is that it abstract from natural conditions’ (Roos, 2005, p. 605). This ‘salient feature’ has been the common approach in the last 180 years of spatial research.

Conclusions

The emergence of New Economic Geography cannot be understood regarding the logic of science. Since this approach has a lot of different shortcomings, its success is owing to institutional factors. The ‘discovery’ of space by neoclassical mainstream and the stress

placed on some long ago known factors of agglomeration and deglomeration is the only positive results of the movement. On the negative side can be found primarily the vanishing and misinterpreted non-mainstream research traditions, the inadequate view of space, and the illusory outcomes of the models. This paper hoped to be a paltry contribution to correct this failure on the market place of economic ideas.

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