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## New Economic Geography: history and debate

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## Abstract

This paper aims to synthesize the main conceptual and ontological discussion around the field of New Economic Geography. It starts out by laying down the fundamental reasons and motivations that led to the surge of New Economic Geography and provides the background in adjacent fields of economic theory which made this possible. I then provide an overall assessment of the state of the art in NEG and track the intellectual evolution of the field since the nineties up to the present, focusing on the intrinsic criticism that it has been subject to throughout its history. This criticism has its roots in the different ontological conceptions of geography (space) and history (time), as well on the methodological differences, between economists and geographers. Another concern of this paper is to analyze the evolution of the debate and communication between geographical economists and economic geographers.

**Keywords:** economic geography; geography and history; ontological debate;

**JEL codes:** N7, N9, R12.

## 1 Introduction

Paul Krugman's Core-Periphery (CP) model was the first general equilibrium model to explain the riddle of uneven spatial development and, in particular, the forces that lead to spatial agglomeration of industry, giving rise to the New Economic Geography (NEG). In fact, it was the seminal works by Krugman (1991a, 1991b) that launched the New

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Economic Geography. However, economic agglomeration should not be held in a too general way, as its interpretation depends on the spatial (and even historical) scale. In that sense, Krugman's NEG cannot be held entirely brand new, as there have been other equally high quality papers in urban economics and location theory previous to NEG. As such, to provide a brief overall assessment of the subject from an historical perspective requires one to identify both the early and recent theoretical contributions in economics that served as background to NEG. To accomplish such a goal, this paper describes the early studies on land use theory by von Thünen (1826), followed by a brief mentioning of the contributions from early urban economics and location theory to NEG. It then tracks the limitations of general equilibrium theory under the assumption of constant returns up to the path-breaking surge of models of monopolistic competition with increasing returns to scale, which serve as a bulwark for most NEG theorists.

Several works building on Krugman's seminal CP model have emerged and extended its original framework in order to provide new insights on NEG and on the study of economic spatial imbalances. Some have come to incorporate and endogenize the role of cities and urban systems (e.g. Fujita and Krugman, 1995). Others explain the forces that contribute to, or drive away from, agglomeration outcomes, through the interaction of vertical linkages (e.g. Krugman and Venables, 1995) among producers. Another wave of NEG models have been dedicated to the study of the bilateral relations between agglomeration and regional growth (e.g. Baldwin and Martin, 2004).

Although NEG has remained a challenging and vibrant field within the economics profession, it has been susceptible to a wide range of criticism, early from its start. The critiques stem both from economic geographers and from geographical economists alike. The latter have raised issues towards the subject, frequently arguing that NEG needs to overcome several technical limitations that keep thwarting its development (see e.g. Fujita and Mori, 2005; Behrens and Robert-Nicoud, 2010; and Fujita and Thisse, 2009).

In spite of the technical issues raised by geographical economists, one can arguably say that their criticism lies in no comparison with that of economic geographers *proper*.<sup>1</sup> Paul Krugman anticipated the criticism by noticing that geographers would most likely disdain the economics profession's love for rigor and contempt for realism. In fact, Martin (1991) has argued that, contrary to geographical economics, economic geography proper involves "a firm commitment to studying real places (the recognition that local specificity matters) and the role of historico-institutional factors in the development of those places". As such,

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<sup>1</sup>The distinction between geographical economists and economic geographers proper was first used by Ron Martin (1999).

it involves a rejection of abstract models in favour of “discursive persuasion”. Recently, Paul Krugman responded to this critique by stating:

I have no problem with people investigating local specificity and engaging in discursive persuasion. But the new economic geography was designed to attract the attention of mainstream economists. And mainstream economics decided long ago that devising abstract models is an essential part of being a useful profession. (Krugman, 2010).

Going deeper into the conceptual “clash” between economic geographers and geographical economists, two aspects of NEG, which constitute the main targets of criticism by geographers, are in order: (i) the treatment of geography, i.e., of space itself, and (ii) the notion of history and time in NEG.

In general terms, the conceptualization of space for geographers is generally something to be theorized, whereas for NEG the economic landscape is typically treated as given in the form of an abstract geometric space. Similarly, time in NEG is also circumscribed in the sense that the focus is on equilibrium outcomes and local stability analysis, rather than on real time, or history (Boschma and Frenken, 2006; Martin, 1999; Garretsen and Martin, 2010). As such, according to economic geographers proper, NEG models do not give an account of actual evolutionary and adjustment paths.

Geographical economists have not been very concerned with the critiques coming from economic geographers proper or with the part played by geography and history in the explanation of uneven spatial development (Krugman, 1995, 1998; Overman, 2004; Duranton and Rodríguez-Pose, 2005; Scott, 2006; Garretsen and Martin, 2010). Proper economic geography (PEG), with its focus on cultural, social and institutional accounts and its discursive persuasion, is cast aside by geographical economists because of its neglect of formal modelling. Moreover, economic geography’s analytical and formal answer to NEG, dubbed “evolutionary economic geography” (EEG) (Boschma and Frenken, 2006), is still in its infancy. While PEG followers have reacted strongly to the surge of NEG and dismissed it as being non-relevant, developments in PEG have not provoked much of a response by geographical economists, who view PEG as non-scientific. In reality, the hypothetical potential debate between the two fields amounts to a case of “two disciplines ignoring each other” (Duranton and Rodríguez-Pose, 2005).

In order to shed some light on the range of topics analyzed by NEG throughout its rather brief history, I seek to provide a quick albeit comprehensive review on the literature related to the aforementioned topics. The first part of this paper thus contains an overall description of NEG’s history from an economics perspective. This is done in section 2. The second part of the paper is dedicated to the conceptual debate between economists and

geographers. For this purpose I start by focusing on the ontological conceptions of space and time inherent to NEG, PEG and EEG. The paper then shifts its attention towards discussing the dialogue (or lack of it) between economists and geographers, conveniently highlighting the methodological differences in approaches taken up by economic geography and geographical economics. All this is left for section 3. Finally, section 4 is left for some concluding remarks.

## 2 Brief history of NEG: an economics perspective

In this section the focus on NEG lies from an economics perspective. I would not claim for once that the history of NEG can be fully assessed without considering the contributions, even if many times under the form of prospective criticism, and perspectives of economic geography *proper*. I feel, however, that the issues raised by geographers towards NEG belong more to the conceptual and ontological realm and do not compromise the economic historical motivations and developments behind NEG. Thus, I choose to separate the debate between geographical economists and economic geographers from the historical account of NEG and leave the former for the next section. Again, it cannot be overemphasized that this does not in reality reflect my idiosyncrasies as an economist, but rather reflects the notion that NEG is actually more narrowly focused than the broader subject of economic geography, building more on the contributions borrowed solely from the economics profession. This may in fact add to the widespread criticism in the sense that NEG should take both history and geography more seriously, as is argued by Garretsen and Martin (2010). This point will be discussed thoroughly in section 3.

### 2.1 From von Thünen to Krugman: background of NEG

Back in the nineteenth century, von Thünen (1826) pioneered the explanation of patterns of agricultural activities surrounding cities in pre-industrial Germany. Thünen was the first to build a theory focused on the transport-cost differentials across locations. For this purpose, he used a model in which space is represented by a plain on which land is homogeneous in all respects except for a city centre in which all transactions regarding agricultural goods must occur. Allocation of some land near the city centre for the cultivation of some crop affects the costs of transporting all other crops as these are forced to be grown farther away from the centre. This setting is simple albeit rich enough to show how a competitive land market can structure the use of land across space by perfectly divisible activities. Thünen devised a process whereby a farmer makes an offer based on the surplus he can generate by using one unit of land available at any particular location. This has led him and his successors to develop the concept of bid rent function

(see Thisse, 2011), which describes the maximum price an agent is willing to pay to occupy each location. Since land is allocated to the highest bidder, economic activities are distributed to the farmers across concentric rings, which surround the city centre, each of them being specialized in one crop. Land rent is decreasing in distance from the city centre at a rate which is constant in each ring and decreasing from one ring to the next. More than a century later, Samuelson (1983) provided a closed solution to this general “spatial” equilibrium model, in which the real wage common to all workers is endogenous.

The seminal contribution of von Thünen to economic thought remained in the shadow of mainstream economics for several decades. Launhardt (1885) was the first to give a formal treatment of von Thünen’s ideas, followed up by Dunn (1954). Notwithstanding, it was not until Beckmann (1972) that von Thünen’s model was endowed with a neoclassical production function.

The apparent contempt toward von Thünen does not strike as surprising if one considers the generalized neglect of international economists concerning concepts such as distance, space and transportation costs, in general. Among the early attempts (thus exceptions to the rule) to study the role of distance for the analysis of international trade is the “Theory of Interregional and International Trade” by Bertil Ohlin (1933), who sought to understand how transportation costs affect patterns of trade and specialization.

Recently, the field of urban economics has had the merit of delivering important contributions, such as providing microfoundations of urban agglomeration economies and explaining the impact of neighborhood effects and spatial externalities on the stratification of cities. I choose to mention the work of Henderson (1974, 1988), which has served as a foundation to a large flow of research on urban systems. It provides an elegant description on how cities with different sizes may emerge.

Location theory, deeply rooted in Hotelling’s “Stability in Competition” from 1929, studies the geographical distribution of industry and geographical variations in mark-ups. Though relevant fields in their own right, neither of them has achieved the interest reached by economic geography. One of the reasons might be that, unlike the other two, NEG has a well defined and broad enough objective. It is the first field in economics that provides a detailed description of spatial inequalities that emerge as the outcome of a general equilibrium model. Paul Krugman, as a precursor of NEG with his seminal article from 1991, was arguably the first to show how regional imbalances arise, with a particular focus on agglomeration patterns.

The reason why it took so long to come up with an explanation probably hinged on the technical impossibilities imposed by dominant paradigms of economic theory up to that

date. The debate about whether the general equilibrium model is comprehensive enough to fully reflect the working of the spatial economy has a long tradition. A famous critique towards general equilibrium analysis was brought forth by Isard (1949), when, addressing Hick's "Value and Capital" from 1939, he argued that Hicks confined his analysis to "a wonderland of no spatial dimensions". He further criticized the implicit way by which economists generally treated transportation costs for the sake of tractability.

Important contributions, such as Allais (1943) and Arrow and Debreu (1954), among others, made attempts to integrate space within general equilibrium analysis. The latter has been useful for the study of commodity flows in space, provided that both firms and households have *exogenously given locations* (Thisse, 2010). Notwithstanding, according to Krugman (1995), the analysis is much more complicated once agents are free to choose their locations. To sum up the main argument by Krugman (1995), it is useful to state Starret's (1978) "Spatial Impossibility Theorem". Starret (1978) shows that the simultaneity of assumptions, of perfect competition and constant returns to scale, preclude any possibility of explaining spatial imbalances. The Theorem states that in an Arrow-Debreu economy, with a finite number of agents and locations, homogeneous space and costly transportation, there is no competitive equilibrium involving inter-locational trade (transportation). By homogeneous space it is meant that both preferences and the set of production technologies are independent of location. In other words, if economic activities are perfectly divisible, there is a competitive equilibrium such that each location is autarkic and "backyard capitalism" is the only possible outcome.

Bearing the previous in mind, it is not difficult to find that, in order to explain spatial inequalities and regional specialization, one must violate at least one of the assumptions stated in the Theorem. As Duranton (2008) argues, taking transport costs as an unavoidable fact of life, explaining uneven spatial development requires one to assume some sort of non-homogeneity of space and/or non-convexity of production sets.

With the surge of models of imperfect competition, pricing decisions by firms depend on the spatial distribution of both consumers and other firms. Inside imperfect competition, we can separate models in two major groups: those of oligopolistic competition, and those of monopolistic competition. The former assume the existence of few but large agents who interact strategically. As argued by Fujita and Thisse (2002), these kind of models are more suited to study applications in game theory and spatial competition. The latter allow for some interdependencies between firms beyond the perfectly competitive setup in the sense that firms are able to set prices, goods are differentiated, and firms operate under increasing returns to scale. Models of monopolistic competition are favoured in particular in most fields of the economics profession (Fujita and Thisse, 2009). One

reason is that, since the firm space is atomistic, there is no strategic interaction between firms, and thus the common problem of existence of equilibrium which occurs frequently in oligopolistic competition is not so problematic in this case. Another reason pertains to the higher tractability of monopolistic competition. A particular case of monopolistic competition is the Dixit-Stiglitz (1977) framework where consumers value variety of an horizontally differentiated good and where there exists a continuum of firms operating under internal economies of scale for each good. Since the framework implies increasing returns to scale at the plant level, and transportation is costly, location decisions are not trivial. Hence, both ingredients are essential to any model that aims to explain the space-economy. In fact, it is widely acknowledged that the organization of the space-economy results from a trade-off between scale economies in production and transportation costs. This fact was already stressed by Koopmans and Beckmann (1957).

Seeing that the space-economy is a result of agglomeration forces operating against dispersion forces, Fujita and Thisse (2009) scrutinize the main principles at work that led to NEG. Price competition is known to be a strong dispersion force as a result of the “Principle of Differentiation” (Tirole, 1988). However, product differentiation alleviates price competition and hence allows firms to locate where it has access to a bigger market and higher demand, and where transport costs are lower. The other principle at work is that of the home market effect, whereby large markets are relatively more attractive to firms. But this view assumes that the market size is exogenous, i.e., that consumers are not allowed to migrate between regions. The path-breaking contribution to tackle this issue is the Core-Periphery (CP) model by Krugman (1991).

## 2.2 Contributions in New Economic Geography

The starting point of the CP model is that the migration of some workers affects the global welfare and thus changes the relative attractiveness of both origin and destination regions. These effects can be seen as externalities because workers do not take them into account in their decisions (they are short-sighted). The basic layout of the CP model comprises two regions, two sectors, one operating under monopolistic competition *à la* Dixit-Stiglitz and the other operating under perfect competition, and two factors of production. One factor is regionally immobile and is used as an input in the agricultural sector. The other is regionally mobile and is used as input in the industrial sector. There is a cumulative process whereby market size and cost of living effects work in a way that promotes agglomeration of industry in one region. As this region becomes bigger, so does the market, thus attracting more industry. This circular causation was noted by Krugman (1991a). On the other hand, a more concentrated market enhances price competition thus working as a dispersion force (market crowding effect). All things considered, the key-factor for

determining the spatial distribution of industry is the level of transportation costs. Thus, contrary to the neoclassical model that predicts only convergence, the CP model accounts for both convergence and divergence.

Fujita and Mori (2005) separate the first generation (benchmark) models of NEG in three different classes: the CP model, previously discussed; urban and regional systems; and agglomeration and trade.

The NEG models that belong to the class of urban and regional systems deal with the spatial distribution of agglomerations without considering their internal spatial structure. One precursor in this literature refers to the “Racetrack Economy” by Krugman (1993) which extends the original CP model to include 12 regions around an homogeneous circle with the same distance between each adjacent region. The main conclusion reached by Krugman (1993) was that a simulation with a nearly uniform initial spatial distribution of industry around the circle would always end up with all manufacturers concentrated equally at two regions located at exact opposite sides of the circle. A more realistic approach is taken up by Fujita and Krugman (1995) which sets out from the von Thünen’s city centre. The main prediction of the model is that, as population increases, the borders of the agriculture hinterland locate sufficiently far from the city centre so that firms find it attractive to locate out of the city centre, thus giving rise to a new city. This process is self-perpetuating as more cities arise.

Krugman’s contribution from 1991 has also allowed to provide insights on city formation and urban systems. The paper by Fujita and Krugman (1995) was arguably the first to explain how urban and agricultural land use patterns emerge endogenously. Their framework closely relates to that of the original CP model, with some differences. Henderson (1974, 1988), as already argued above, developed an approach that allows him to describe the emergence of a hierarchy of cities. Fujita, Krugman and Mori (1999) further extend this approach by considering several differentiated industrial goods in a model similar to an NEG model.

Inasmuch as there is also a need to explain how agglomeration outcomes are possible when factor mobility is reduced, several models have come up with explanations based on input-output linkages. The last class contains models in the view of agglomeration patterns as a result of vertical linkages. The starting point is the paper by Krugman and Venables (1995). The main idea is basically that agglomeration of a sector in a region occurs because there is a vertically linked sector that is already agglomerated in that region. The forces at work in this framework are different. There is also a market expansion effect but in this case it is due to higher income (higher wages since labor supply is inelastic) that leads to higher consumer demand. However, if wages are

too high, some firms will want to relocate their production to the periphery, so there is also a dispersion force. The advantage of this framework is that a self-perpetuating agglomeration process may not happen. Instead, economic integration yields a bell-shaped curve of spatial development. As such, these kind of models account for the possibility of reindustrialization of the periphery.

The original CP model has also been extended to study regional growth. Baldwin and Martin (2004) and Fujita and Thisse (2002) have attempted to take advantage of the common “tools” shared by both NEG and “new growth” theories. In the latter, the authors show that the growth rate of the global economy depends on the spatial distribution of an innovation sector (that applies mobile skilled workers) across regions. This lends support to the evidence that there is a trade-off between growth and spatial equity. Overall, the result is good in the sense that, as the economy agglomerates in a region, the innovation rate tends to increase, and all workers benefit from this including those living in the periphery. Hence, the overall outcome is Pareto superior. Of course, workers in the core benefit from a higher welfare than workers with the same set of skills that live in the periphery. Thus, the main implication of these works in terms of policy issues is that there may be a conflict between social cohesion and economic growth.

Whatever the agglomeration mechanism, be it labour migration as in the original CP model, input-output linkages as in Venables (1996) or capital accumulation as in Baldwin (1999), and even with marginal changes in functional forms, the key features of Krugman’s original model do not change. As long as goods are manufactured under increasing returns to scale and markets are segmented in a standard model with 2 regions, what happens in one region impacts the other one. The effects of the economic geography of the two regions is similar across most CP models, as the change in the relevant market sizes for CES-good producers is at the heart of agglomeration and dispersion forces. It turns out that all simple NEG models with Cobb-Douglas-cum-CES preferences, iceberg costs and constant returns to labour share the aforementioned features and “are *isomorphic* in an economically meaningful state space” (Behrens and Robert-Nicoud, 2010). This is also in accordance with Ottaviano and Robert-Nicoud (2006) and Robert-Nicoud (2005).

### **3 The conflict between geographers and economists**

Since the mid-1950s to the mid-1970s, geographers have sought to explain spatial development imbalances that hinged on Löschian and other German location theories. From that point on to the end of the 1980s, economic geography started to draw heavily on approaches influenced by Marxist political economy and related methods of historical

materialism (Garretsen and Martin, 2010). Lately, institutional economics, economic sociology, cultural theory and evolutionary economics have all come to try to provide new insights on the riddle of uneven spatial development.

In sharp contrast with these views, mainstream economics has disregarded the importance of space and geography. Some exceptions that fall in the class of economic heterodoxies are, e.g. the works by Myrdal, Kaldor and Jacobs, who have argued for the recognition of regions as key factors for economic development. As mentioned in the previous section, urban economics and regional economics have also contributed to the study of urban agglomeration economies and the geographical distribution of industry, but have remained in the shadow of mainstream economics. The contributions by Krugman (1991a, 1991b) have thus been the founding fathers of the new stream of NEG (or geographical economics), which is explicitly concerned with understanding spatial agglomeration patterns of economic activities.

Notwithstanding the remarkable path-breaking contributions of early works in NEG, a wide array of criticism has spawned, particularly from within the geography profession. For the latter, NEG has been commonly credited with “reinventing the wheel” and has thus not impressed many economic geographers proper. Early criticism towards the subject results from geographers such as Hoare (1992), Johnston (1992), Martin and Sunley (1996), Martin (1999) and Scott (2004). The common view shared by these works is that NEG is clearly limited in accounting for the social, institutional and cultural factors that shape the economic landscape and that the enterprise of trying to reduce the complexities and richness of economic geography to stylized mathematical models is fundamentally misguided. Another critique is that NEG, in the interest of theoretical clarity, focuses on processes that were important a century ago but much less relevant today (Krugman, 2010; Rodríguez-Pose, 2011). This is not to say that this fact was neglected by prominent economists from within NEG (see e.g. Krugman, 1995 and, more recently, Krugman, 2010).

Another critical view, which has also rooted from within mainstream economics, concerns the oversimplifying assumptions underlying the framework of most NEG models. Such assumptions pertain to the economic structure itself, such as the dimension of location space or the consideration of just two productive sectors. In fact, most NEG models are comprised of only two regions, a manufacturing sector (operating under increasing returns), and an agricultural sector (under constant returns). Other unrealistic assumptions have to do with strict homogeneity among consumers and firms and the oversimplifying CES monopolistic competition framework based on Dixit and Stiglitz (1977). The latter, together with the iceberg transportation cost assumption (after Paul Samuelson), are used for tractability purposes but quite often overlook empirically observed facts that may in-

deed influence the agglomeration of economic activities in space. Recently, a lot of works from prominent geographical economists have come to provide an overall assessment of these limitations of NEG, and try to provide new avenues of search along which the field should develop if its aim is to provide new important insights on the riddle of uneven spatial development (see e.g. Fujita and Mori, 2005; Behrens and Robert-Nicoud, 2010; and Fujita and Thisse, 2009). According to Garretsen and Martin (2010), this line of criticism is not solely a critique of NEG, but of mainstream economics, mainly neoclassical and general equilibrium economics, as a whole. Nonetheless, the criticism from within the economics profession can be seen more as prospective and not as destructive.

The debate between geographers and economists, on the other hand, is quite more serious. I shall focus on two aspects of NEG which are the main targets of criticism by geographers: (i) the treatment of geography, i.e., of space itself, and (ii) the notion of history and time in NEG. In the view of Garretsen and Martin (2010), economic geography at large could only benefit from an “inner-exchange” of views regarding the treatment of geography (space) and history (time) and there indeed a need for a methodological pluralism within the field. Furthermore, as Duranton and Rodríguez-Pose (2005) have argued, notwithstanding the growing division between economic geographers and geographical economists that has been very detrimental to the two professions, there is indeed a large scope for discussion between them. It is these points that I shall explore in greater detail in the next subsections, in close relation with Martin (1999) and Garretsen and Martin (2010).

### 3.1 Ontological concepts of geography and space

In the original CP model (Krugman, 1991) with two regions, the transportation cost parameter is the embodiment of geography. In an abstract two-region setting, the distance between hypothetical regions is normalized to unity and “dimensionality” is captured by transport costs, without which there is absolutely no role for geography. Subsequent developments of the original CP model, such as those presented in section 2 of this paper, have provided qualitative different and arguably more realistic results compared to Krugman’s CP model. Notwithstanding, these more recent NEG models “essentially do *not* offer a different or more substantive analysis when it comes to the role of geography or history” (Garretsen and Martin, 2010). In reality, the depiction of geographical scale has to take account of the fact that it cannot be represented in terms of just transport costs, and is more complex than the simple geometries used in NEG theory to portray geography.

Bearing the previous in mind, what kind of ontology regarding the conception of space and regions should be adopted by NEG? And inasmuch as most criticism in this

line stems from PEG, what can NEG theorists learn from the latter in this regard?

PEG proponents argue that NEG models treat space and time as though they have no influence over economic activities. Moreover, since geographical space is considered to be exogenous, it is external to economic activities. The points representing regions in NEG models remain immutable over time even if the region's economy grows. Moreover, space is given and is invariant under the different economic activities. Finally, according to Martin (1999), space in NEG models exists outside of the actions of economic agents and is not socially and historically constructed through those actions.

The nature of space and the definition of regions can be seen as a fundamental research issue for geographers. In economic geography, the focus is on the uneven geographical development which implies discussions on the nature of geographical space and of regions. According to Harvey (1985), a region pertains to a bounded geographical unit that is characterized by its own unique economic, social and cultural "structured coherence". It turns out that regional spaces are rarely structurally coherent or contiguous, but are rather characterized by economic, social, cultural and spatial discontinuities. Moreover, regions are produced and modified by both economic and social structures operating over time. All this boils down to the conclusion that the concept of regions is anything but easy to define. As a result, NEG theorists may answer the critiques from PEG by arguing that the latter is not able to provide a superior set of operational concepts that NEG should use instead.

From this point on, I find it useful to follow Harvey (2006) in his refinement of a tripartite distinction concerning the ontology of space: absolute space, relative space, and relational space. A close inspection on the differences between each of these concepts may prove useful for the development of NEG. As Garretsen and Martin (2010) did in their paper, I opt to cite David Harvey in what concerns these three concepts:

If we regard space as absolute, it becomes a "thing in itself" with an existence of independent of matter. It then possesses a structure which we can use to pigeon-hole or individuate phenomena. The view of relative space proposes that it be understood as a relationship between objects which exists only because objects and relate to each other. There is another sense in which space can be viewed as relative and I choose to call this relational space, i.e., space regarded in the manner of Leibniz, as being contained in objects in the sense that an object can be said to exist only insofar as it contains and represents within itself relationships to other objects. (Harvey, 1973).

The first conception, that of absolute space, can thus be seen as the space of Newton and Descartes; a pre-existing, fixed and immutable grid susceptible to measurement and quantification. Geometrically, it is tantamount to euclidean space. Relative space is analogous

to that of *some* non-euclidean geometries. I highlight the word “some” because spaces such as elliptic or spherical spaces<sup>2</sup>, which are clearly non-euclidean, hold many of the same properties of the euclidean geometry.<sup>3</sup> According to Harvey, in relative space there are multiple geometries from which to choose and the spatial frame depends essentially on what is being relativized. As a result, the uniqueness of locations patent in absolute space gives way to a conception of multiple maps of relative locations differentiated by distances measured in terms of costs, time, etc. Therefore, space here may not be continuous. In other words, we may have a different metric depending on the activities and agents involved. The third conception of space, relational space, implies that processes do not occur in some pre-given space, rather they define their own spatial frame. The idea is that an event at a point in space cannot be understood by looking solely to what exists at that point. Instead, one must consider the transfactual structures that surround it and influence it.<sup>4</sup> For instance, time and space cannot be understood separately, nor are they independent concepts (one may think of space-time in the general relativity theory of Einstein).

At first sight, it would seem that geometry in NEG models is essentially euclidean, independent of the economic process and pre-given. On the other hand, it is also true that NEG models internalize location into the economic process. In fact, growth in NEG models affects the distribution of economic activities and vice versa (recall Baldwin and Martin, 2004 and Fujita and Thisse, 2002). Geographers still argue, however, that regions in NEG models remain spaceless entities: they have no spatial extent and thus no internal spatial structure. While this may be true, at least to some extent, this fact has also already been acknowledged by some NEG theorists. In fact, Behrens and Robert-Nicoud (2010) not only are aware of this but also emphasize the role of urban hierarchy formation and the spatial sorting of heterogeneous individuals across cities as new avenues of search for the development of NEG. Moreover, some NEG models allow for regions to differ in size. Notwithstanding, these differences are always exogenous (or pre-given), fixed and purely quantitative, thus belonging to the absolute spatial frame.

Flows in NEG models such as labour migration and knowledge spillovers may be argued to define relative rather than absolute spatial frames. According to Garretsen and Martin (2010), some NEG models even combine different notions of relative space. The real problem, transverse to both economists and geographers, is when considering relational space. This is due to the fact that “measurement becomes more and more

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<sup>2</sup>The common geometries used to picture spherical models, such as the earth globe.

<sup>3</sup>For instance, space is continuous, homogeneous, isotropic, and without boundaries. An example of a property of euclidean geometry that does not hold in elliptic space is the Pythagorean theorem.

<sup>4</sup>External influences get internalized in the processes that define the spatial frame.

problematic the closer we move to a world of relational space-time” (Harvey, 2006). In the case of NEG, according to relational space, regions would be defined by the processes that operate across the economy and by their relationships with all other regions. As a matter of fact, NEG can somewhat relate to the concepts of relational space, mainly through the mathematical expressions for real wages in each region. Actually, a real wage of a region is a function of the spatial distribution of economic activities (demand, income, farmers, entrepreneurs, consumers) in all other regions and the cost of transporting goods to those regions. As a result, a change in one of those items influences every region’s real wage and thus impacts on the pattern of agglomeration across all locations, and this change reshapes the economic potential surface. According to Harvey (2006), this economic potential surface is one embodiment of material relational space. This clearly mitigates the relevance of the geographers’ criticism towards the conception of space in NEG. Moreover, PEG itself finds it problematic to translate conceptual notions of space and regions into empirically operational forms.

Bearing the previous in mind, where does this leave NEG’s representation of geographic space? The answer may lie in both PEG and NEG arriving at a middle ground. The fact is that economic geographers put a considerable larger emphasis, compared to NEG, on the role of flows, linkages and networks as mechanisms that shape the economic landscape. Goyal (2009), for instance, urges NEG theorists to take more interest in the role of networks. To sum up, both disciplines should acknowledge the discontinuities in the economic landscape, and that the latter is simultaneously absolute, relative, and relational.

### 3.2 Ontological concepts of time and history

In most NEG models, since the initial conditions shape the equilibrium outcome that is achieved, in the case of a fully symmetric model where multiple equilibria exist, history does, in fact, matter. However, as geographers would argue, given the model’s parameter values, the number, nature and stability of equilibria are fixed and pre-determined. Furthermore, for models in which different equilibria can be simultaneously stable, the equilibrium outcome depends on the *path* followed to reach the value of the bifurcation parameter (in this case transportation costs) that deems those equilibria simultaneously stable. In other words, changes in transport costs will set of adjustment paths that depend on where the system started originally, leading to the argument of NEG theorists that NEG models usually exhibit *path dependence*. However, history is still simply a set of pre-determined states where which one is selected depending on the model’s initial conditions. Attention within NEG literature has focused not on whether this representation of history is ontologically meaningful, but rather on empirical testing of stability of

equilibria in response to shocks in the geographical distribution of industry. This limited treatment of time and history in NEG models actually reflects its roots in mainstream economics.

However, the economic landscape can be seen as an historical process, and can only be understood as such. For many years in mainstream economics, the economy was typically viewed as an adjustment process towards a general equilibrium. In recent years, a renewed interest in the existence of multiple equilibria has spawned the idea that “history matters” in the determination of the equilibrium to which a trajectory converges to. Nonetheless, this “appeal” to history seems more like a methodological device for narrowing down the selection of solutions than a genuine attempt to explain how the spatial economy actually evolves. As Krugman (1996) himself notices, NEG models allow only for extrinsic and not intrinsic sources of change. Thus, once the economy has reached the equilibrium, history and change effectively end and the economic landscape is left in a position of stasis. Using history as a process of selection between multiple equilibria thus overlooks the possibility of spatial economic development creating its own set of outcomes in the course of evolution. This is fundamentally and ontologically different from an evolutionary process, whereby an environment is continuously reconstructed in the course of the evolution of a system (Setterfield, 1997). This view is taken up by most economic geographers proper, who consider that the idea of equilibrium is difficult to reconcile with the idea of an economy viewed as an evolutionary system, which has the capacity to transform and mutate from within. These are basically the views of Martin (1991) and of the proponents of evolutionary economic geography (EEG), whose main contributions are, in my view, those of Boschma and Frenken (2006) and Boschma and Martin (2007).

Evolutionary economics must be dynamical, deal with irreversible processes, and must cover the generation of novelty (evolution). As a result, it not only precludes comparative static analysis, but also dynamical processes based on stationary states. Furthermore, to be evolutionary in orientation, the criterion of novelty requires that economic evolution be driven by the creative capacity of economic agents and by the creative functions of markets (Metcalfe *et al.*, 2006). One could then say that evolutionary economics is not concerned with equilibrium outcomes but “with processes of long-term and progressive change” (Nelson and Winter, 1982). To cite Boschma and Frenken:

Evolutionary theory deals with *path dependent* processes, in which previous events affect the probability of future events to occur. (Boschma and Frenken, 2006).

That what is arguably the main limitation of EEG is that it is mainly a new direction and not a current, fully operational and developed, body of work within economic geography.

According to Garretsen and Martin (2010), the studies contained within EEG, while suggestive, do not yet add up to an integrated explanatory or methodological framework.

A recurring idea in EEG is the emphasis given to the idea of path dependence. It basically comprises the notion that initial historical events have long-run consequences by triggering self-reinforcing processes that “lock-in” a particular trajectory (recall the original CP model). A way to reconcile the notions of evolution and equilibrium has been pointed out by David (2005), with the notion of “punctuated equilibrium”. This states that a historically contingent event triggers a path dependent movement towards *one* equilibrium in which, once reached, the system remains unchanged until disrupted by an external shock. Some geographers argue that many real world examples of regional example are in sharp contrast with this version of path dependence and the concept of “lock-in” to an equilibrium state. In fact, as Martin (2010) argues, regional economies adapt and mutate over time, thus preventing an economy from becoming locked in a particular stable state or trajectory. Herein lies what I consider to be a misunderstanding of dynamical adjustment processes on behalf of economic geographers. If these processes are governed by non-autonomous systems (which generally is the case in economics), it is not possible for solutions (or trajectories) to cross each other. Since an equilibrium is a particular solution of a dynamical system, at which a trajectory stays put if the initial condition is the equilibrium itself, it is not possible for any trajectory “beginning” away from that equilibrium to get “locked-in” at that same equilibrium. Instead, asymptotic stability implies that a trajectory will converge to, but *never* reaches, the equilibrium. A direct implication of this is that no external shocks in the geographical distribution of the economy are needed for a trajectory to move away from an equilibrium. A change in parameter that deems an equilibrium unstable (e.g. transportation costs in NEG) will suffice to blow any trajectory that was previously convergent away from the previously stable equilibrium. Far from being just a mathematical pettiness, this indeed implies that sectoral structure changes may reverse agglomeration processes in NEG. All that said, geographers are still right in the sense that a pre-determined convergence process will not endogenously change and one can safely adopt the “lock in” concept from this perspective.<sup>5</sup> Another problem with path dependence economics is that it fails to account for how new paths of economic development emerge, i.e., change is essentially accidental and random. Unfortunately, EEG actually does not provide a better account in itself (Garretsen and Martin, 2010).

From the aforementioned analysis, are there any important lessons NEG can hold from

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<sup>5</sup>Notwithstanding, many systems in economics do exhibit chaotic behaviour (see e.g. Zhang, 2005). This is not a feature present in NEG models, nevertheless.

EEG? The answer is positive, provided that NEG can somewhat accurately incorporate the more realistic historical dimension of EEG into its models. The emphasis given to variety and novelty, to innovation and technological change, in EEG, is on how these processes stimulate and form the ongoing evolution of the economic landscape. On the other hand, NEG treats innovation and technological change far too restrictively, the latter only determining the extent of spatial agglomeration and not the changing nature of economic activity.

### 3.3 A lack of dialogue?

When the collision, based on ontological, epistemological and methodological differences, between geographers and economists, is brought up for discussion, the picture is actually more that of “two disciplines ignoring each other” (Duranton and Rodríguez-Pose, 2005). Yet, economists and geographers, or rather, geographical economists and economic geographers, have long been inherently interested in answering to the same questions. There is an increasing belief among those theorists who bet on the inter-disciplinarity between the two fields that, while geographers have much to benefit from the ever-evolving techniques developed or adopted within economics, the latter has also a lot to gain from geography’s broad and complex vision if they are to expand their views on a number of subjects.

Recently, a number of workshops have tried to promote these initiatives. One example is that of the European Science Foundation (ESF) and the Centre for Economic Policy Research (CEPR) called “Topics in Economic Geography: A Dialogue Between Geographers and Economists“. Its main objective was to promote the dialogue by bringing economists and geographers together to address issues in areas of common interest, in order to avoid future ontological and methodological confrontations that could potentially drive each other further apart (Martin, 1991; Overman, 2004; Sheppard, 2001). If nothing else, the workshop served to highlight that economists and geographers are not in a collision route and that there is a great scope for dialogue. Notwithstanding, the barriers that impede the dialogue are still significant.

The lack of collision (dialogue) is patent in the work by Duranton and Rodríguez-Pose (2005). They show that most papers published in mainstream economic geography scientific journals do not reference any papers from mainstream economic literature. Conversely, economists are mostly concerned with the work of other economists, ignoring political science, sociology and other social sciences. This extends to the cross-referencing between economic geographers and geographical economists.

This point is made even clearer if one considers how economic geographers welcomed NEG (Martin, 1999) when it first arrived. As many authors would argue, a lack of dialogue stems mainly from a clash of cultures. Finding a middle ground between geographical

economics and economic geography is difficult because they reveal different approaches to the way research is conducted and disseminated. Firstly, economists seem to persist much more on core questions, such as the causes for agglomeration and urban growth, compared to geographers. In contrast, geographers explore a broader variety of directions. Secondly, whereas economists prefer mainly rigorous deductive analytical approaches with incremental value to existing knowledge in view, geographers adopt individualistic approaches with a greater variety of methods which are predominantly qualitative and inductive. So the methodology clearly takes opposite forms in both disciplines.

When there is a true collision between the fields it is mainly in the publication process, namely when it comes to refereeing for mainstream journals in both disciplines. It is here that the level of incomprehension and animosity between the disciplines shows itself and the way it manifests clearly reflects the ontological and methodological differences, which is in the nature of each professional's idiosyncrasies. It is thus not surprising that, during refereeing processes, geographical economists have to endure comments about the lack of a theoretical base, the incapacity to reproduce the real world, the absence of groundbreaking results, or the inadequacy of methodology. Conversely, economic geographers have to put up with accusations that their works lack structure and rigor, make abusive use of jargon, make non-formalized conjectures and rely heavily on impertinent evidence. All this clearly thwarts any effort for constructive dialogues:

The problem is that, if the only place where actual dialogue is taking place is the publication process and this dialogue is conducted by stealth, the potential for any constructive engagement between economic geographers and geographical economists is minimal. In fact, shielded by anonymity, the venom and incomprehension that many of these referee reports ooze may do more harm than good and irretrievably damage any possibility of future dialogue. And one such negative report may suffice to reverse all the good of ten constructive reports put together. (Duranton and Rodríguez-Pose, 2005).

In spite of the clear differences, some authors have managed to cross the disciplinary and methodological boundaries and continue to exert influence on the other discipline. Remarkably, recent years have witnessed the rise in inter-disciplinarity between economic geography and geographical economists. To illustrate this, I end this section with a brief acknowledgment of a scientific journal to which many authors from the two fields have submitted works and have sought to promote the synergy between the two disciplines: the *Journal of Economic Geography* (JEG).

The purpose of the JEG is to stimulate the inter-disciplinarity between geography and economics by trying to promote the communication between “proper economic geography” (PEG) and “new economic geography” (NEG), since it seems at least reasonable to claim

that both economic geographers and geographical economists try to address the same fundamental questions concerning the spatial distribution of economic activities. Until 2001 (which marks the birth of the JEG), communication between geographers and economists was almost non-existent.

As Garretsen and Martin (2011) point out, several contributions published in the JEG have had the merit of being both retrospective and prospective. Some papers in NEG have come to take account of the growing importance of heterogeneity among agents, thus bringing NEG closer to some issues tackled by both PEG and EEG (Ottaviano, 2011 and Venables, 2011). On the empirical perspective, the incorporation of heterogeneity strengthens the need to develop work with micro-data (see e.g. Combes *et al.*, 2011). Fingleton (2011) also deals with empirical issues, particularly the question of which geographical scale and over which time period NEG is most relevant empirically. This is in close relation with the paper by Head and Mayer (2011). These papers have come to confront the criticism towards the approach taken in geographical economics.

On the “geographical” perspective, the work by Boschma and Frenken (2011) highlights how research in evolutionary economic geography (EEG) has led to an increase in empirical studies that confirm its hypothesis and how it deals with issues that arguably cannot be tackled by NEG. In another paper, McCann (2011) studies international business importing ideas from geographical economics. Contrary to EEG, Sheppard (2011) advocates a quite different analytical framework as he uses concepts from geographical political economy. Other “geographical” papers provide a more general view on the future of economic geography. The paper by Storper (2011) points out fundamental questions in economic geography theory and argues that the existing approaches found in the subject are left wanting. In a paper relating geographical economics and economic geography, Rodrigues-Pose (2011) argues that, though the findings in both fields are similar, the underlying theoretical differences are significant and thus calls for more interaction between the fields. Another mentioned paper is that of Martin and Sunley (2011), which focuses on policy issues on NEG. They argue that NEG models are limited in providing good policy analysis as their credibility in explaining the spatial distribution of economic activities is questionable. But the limitations on a policy perspective also pertain to “proper economic geography”. In that sense, both NEG and PEG need reassessing. Finally, the paper by Brakman *et al.* (2011) seeks to measure the extent of the communication between geographical economists and economic geographers. For this purpose, he uses the papers of the JEG from the last decade to assess the cross-referencing between economists and geographers. Their main finding is that cross-referencing is more abundant in JEG compared to other journals of the field, thus highlighting the relative success of JEG in promoting the communication between economists and geographers in the last decade.

## 4 Concluding remarks

Although fields such as urban economics and regional economics have contributed to the study of urban agglomeration economies and the geographical distribution of industry, they have remained in the shadow of mainstream economics and have not comprised a solid body of work with which one could study spatial imbalances at a geographical scale. It was not until the contributions of Krugman (1991a, 1991b), the founding fathers of the new stream called NEG, that we became able to understand spatial agglomeration patterns of economic activities. Since then, NEG has evolved into a very consistent field within economic theory and a lot of developments more or less similar to the original Core-Periphery model have contributed to enrich the study of uneven spatial development.

Notwithstanding the remarkable contributions of early works in NEG, the field has always been the target of a wide array of criticism from within the geography profession. Most of this criticism has its origin on the fact that NEG operates with highly simplistic and idealized conceptions of space and time. What results from this is that NEG should take both geography and history more seriously.

From a geographer's perspective, NEG should not limit itself to conceptualize space as solely absolute, but should also treat it in relative and relational terms. However, while it is true that NEG undertakes unrealistic absolute space geometries, most models in NEG do in fact exhibit some relative and relational notions of space in the way regions are interrelated to one another through its *economic potential surface* (e.g. real wages). What is perhaps more fragile in NEG models is the crude form taken by these representations.

Another limitation of NEG is concerned with the way it accounts for history. Following the steps of evolutionary economic geography, NEG could well adopt an evolutionary approach, emphasizing the role of novelty, selection, adaptation and self-organization. However, such concepts are indeed hard to translate into analytical frameworks and convincing models. To sum up the view, economic geography at large could only benefit from an "inner-exchange" of views regarding the treatment of geography (space) and history (time) and there is indeed a need for a methodological pluralism within the field. Geographers, whether belonging to PEG or EEG, would argue that the fundamental impediment to making NEG more geography and history oriented is its dependence on formal models, since these are based not on abstracting from concrete reality, consisting of real world cases in real space and time, but on the basis of assumptions concerning solely consumer and firm behaviour in highly simplified hypothetical contexts.

Ontological differences have resulted in a lack of dialogue and even collision between geographers and economists. Even when there is scope for collision between the two professions, the methodological differences generally spawns incomprehension and animosity towards one another, and generates idiosyncrasies that lead to the refutation of theories

and results without any objective foundation. However, as Duranton and Rodríguez-Pose (2005) have argued, notwithstanding the growing division between economic geographers and geographical economists that has been very detrimental to the two professions, there is indeed a large scope for discussion between them. Fortunately, recent years have witnessed a sort of reconciliation between economic geographers and geographical economists, much due to the surge of workshops and scientific journals aimed at promoting the interdisciplinarity between both fields.

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