

CRITICAL SURVEY

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The new 'geographical turn' in economics: some critical reflections

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Over the past few years, a new 'geographical' economics has emerged, focused on the spatial agglomeration of industry and the long-run convergence of regional incomes. Several leading names are associated with this 'geographical turn', including Paul Krugman, Michael Porter, Robert Barro and W. Brian Arthur. This 'new economic geography', it is argued here, is neither that new, nor is it geography. Instead, it is a reworking (or re-invention)—using recent developments in formal (mathematical) mainstream economics—of traditional location theory and regional science. As such it is quite opposed to, and difficult to reconcile with, the work on regional development and industrial agglomeration being carried out in economic geography proper.

If everything occurred at the same time there would be no development. If everything existed in the same place there could be no particularity. Only space makes possible the particular which then unfolds in time... [T]he question how the economy fits into space not only opens a new field but leads in the final analysis to a new formulation of the entire theory of economics. (August Lösch, *Epilogue, The Economics of Location*, 1939, 1943; English Edition, 1954)

There are three reasons in particular why it is important to start doing economic geography. First, the location of economic activity within countries is an important subject in its own right... Second, the lines between international economics and regional economics are becoming blurred... [H]owever, the most important reason to look again at economic geography is the intellectual and empirical laboratory it provides. (Paul Krugman, *Geography and Trade*, 1991)

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1. Introduction

It is over half a century since August Lösch argued that economists should take space and location seriously in their theorisations of the economic process. A one-time student of Joseph Schumpeter, Lösch's answer to the question of 'how the economy fits into space' was to build upon the long-standing Germanic tradition of equilibrium 'location theory', which had originated long before with Johann von Thünen's (1826) *The Isolated State* and then been revived in the 1920s and 1930s by Alfred Weber's (1929) *Theory of the Location of Industries*, and Walter Christaller's (1933) *Central Places in Southern Germany*. Lösch had hoped his work would stimulate the emergence of a new field of 'spatial economics', indeed the reformulation of economics itself. But in fact it never had much impact on the economics profession.¹ Instead, during the 1950s and 1960s, the basic esprit géométrique bequeathed by Lösch's classic, *The Economics of Location*, provided the foundations for two other disciplines, namely regional science and economic geography. However, while these two fields shared a common ancestry, they soon became sharply divergent, both theoretically and methodologically. By the late 1970s, regional science, championed particularly by writers such as Walter Isard in his *Location and Space Economy* (1956) and *Methods of Regional Analysis* (1960), had become a highly mathematical and esoteric theory of abstract, equilibrium economic landscapes, in effect the formalised successor to the German 'location economics' tradition.² Economic geography, on the other hand, had by this time evolved into a more eclectic and empirically-orientated subject, in which formal neoclassically-orientated location theory had been largely displaced by concepts imported from other branches of economics; for example, Keynesian business cycle models, Myrdalian cumulative causation theory, and Marxian notions of uneven accumulation. Since the late 1980s, economic geography has undergone a further vigorous expansion, incorporating ideas from French regulation theory, Schumpeterian models of technological evolution, and institutional economics. And, even more recently, it has turned to economic sociology and cultural theory for inspiration.

For their part, although during the post-war period economists occasionally flirted with geography, they never seemed willing to commit themselves to any serious or permanent relationship.³ But now, it seems, economists are at last (re)discovering 'geography'. This new movement is significant because it contains some highly prominent writers, such as Paul Krugman, Michael Porter, W. Brian Arthur, Robert Barro, Xavier Sala-i-Martin, Barry Eichengreen, Olivier Blanchard, Lawrence Katz, Anthony Venables and Danny Quah. In particular, three of Paul Krugman's recent books, *Geography and Trade* (1991A), *Development, Geography and Economics* (1995) and *The Self-Organising Economy*

¹ Curiously, in his *History of Economic Analysis* Schumpeter makes no reference to Lösch's work. Interestingly, however, he argues that von Thünen's contribution to economics—especially his marginal productivity theory of the spatial patterning of land uses—should be placed above that of Ricardo. Alfred Marshall likewise regarded von Thünen as a key founding father of marginalist economics.

² In fact it was Walter Isard who first coined the phrase 'regional science', by which he meant a form of geographical or spatial economics based on the application of mathematical economic theory to the study of the location of economic activity. He established the *Journal of Regional Science* in 1955. Since then, other related journals have appeared, including *Regional Science and Urban Economics*, *International Regional Science Review*, and *Urban Economics*. The *Journal of Regional Science* is still the leading publication in the field.

³ There were, of course, one or two (non-mainstream) economists who did recognise the importance of uneven regional development in the economy, notably Myrdal and Kaldor. But they were the exceptions that prove the rule. Keynes's only discussion of regional issues was made in the context of his proposals for steering the British state's rearmament programme in the 1930s towards the depressed peripheral areas of the country so as to minimise possible inflationary pressures 'at the centre' (that is the South-east and the Midlands). Other than this, he totally ignored the spatial dimensions of economic growth and instability.

(1996A) are devoted in large part to what he calls his current research project, building a 'new economic geography' (Krugman, 1998). By means of these books and an extensive series of articles (for example, Krugman, 1991B, 1993A, 1993B, 1994, 1996B), he has sought to construct a theory of economic localisation based on increasing returns. In his view, increasing returns are essentially a regional and local phenomenon, so that the study of spatial economic agglomeration and specialisation is sufficiently important to warrant 'the acceptance of economic geography as a major field within economics' (Krugman, 1991A, p. 33). Likewise, Michael Porter (1990, 1994, 1996) has stressed that the degree of geographical clustering of industries within a nation is a key determinant of that nation's international competitiveness. In a similar vein to Krugman, Porter also argues that there are strong grounds for making economic geography a 'core discipline in economics' (Porter, 1990, p. 791). These invocations appear not only to have sparked off a wider interest in the so-called 'new economic geography' and 'geographical economics' by other economists, but have also attracted the attention of key economic research and policy bodies, for example the Centre for Economic Performance at the London School of Economics, the London-based Centre for Economic Policy Research, which now has a major research programme on the 'new economic geography' (for example, CEPR, 1997), the World Bank (see Bruno and Pleskovic, 1995; also International Regional Science Review, 1996) and the Oxford Review of Economic Policy, 1998.

My aim in this paper is to provide some critical reflections on this new-found interest in 'geography' by economists. In one sense this recent conversion is to be welcomed, for there are good (and long-overdue) reasons why economists should indeed take geography seriously in their theorisations and analyses of the economy which, typically, they assume exists on the head of the proverbial pin. The greater the dialogue between economists and geographers the better. However, thus far geographers have not been particularly impressed by this 'geographical turn' in economics. 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. The 'new geographical economics' suffers from the same basic shortcomings and limitations as regional science, and is therefore open to the same criticisms that have long been levelled at the latter.

Essentially, the 'geographical turn' in economics embraces two main research programmes, concerned respectively with the spatial agglomeration of economic activity, and the dynamics of regional growth convergence.¹ I first discuss and evaluate each of these two themes in turn, and then raise some fundamental objections that apply to both, namely their preoccupation with mathematical mainstream economics, their neglect of geography, by which I mean real places, and their myopic view of 'theory'. By way of conclusion, I argue that the 'new geographical economics' represents a case of mistaken identity: it is not that new, and it most certainly is not geography.

¹ It is possible to identify a third strand, based on the neo-Marshallian industrial district economics of a small group of Italian economists. This body of work differs in several respects from the other two strands, and has had little impact on mainstream economics. For this reason, I exclude it from detailed discussion here. However, as I shall mention later, it has been quite influential in shaping economic geographers' work on industrial agglomeration.

2. The 'new economic geography' of increasing returns and spatial agglomeration

The first, and most developed, research programme is associated particularly with the work of Krugman, Arthur, and Venables, and is what Krugman means when he uses the term 'new economic geography'. It is based on the argument that increasing returns, economies of scale and imperfect competition are far more important than constant returns, perfect competition and comparative advantage in causing trade and specialisation; and that the market, technological and other externalities underpinning these increasing returns are not international or even national in scope, but arise through a process of regional or local economic agglomeration (Arthur, 1986, 1994A, 1996; Krugman, 1991A, 1991B, 1991C, 1993A; Venables, 1996A, 1996B; for a positive overview of the 'new economic geography' see Ottaviano and Puga, 1997).¹ Thus to understand trade it is necessary to understand increasing returns, and to understand increasing returns it is necessary to study regional economic concentration and specialisation. Hence Krugman (1993, p. 173) writes: 'The best evidence for the practical importance of external economies is...the strong tendency of both economic activity in general and of particular industries or clusters of industries to concentrate in space'.²

In one sense, the 'new economic geography' can be seen as part of the recent revival of interest in increasing returns within economics more generally (see, for example, Buchanan and Yoon, 1994). Indeed, Scotchmer and Thisse (1992) call this focus on increasing returns the 'folk theorem of spatial economics'. Different authors stress different forms of increasing returns in the process of spatial agglomeration. In the models by Krugman and Venables, for example, the agglomerating (centripetal) forces are basically Marshall's (1890) triad of 'localisation externalities' (labour market pooling, technological spillovers, and intermediate goods supply and demand linkages). These tend to lead to the local clustering of economic activity. On a broader regional level, pecuniary externalities, that is market-size effects, are also important, leading to a large-scale centre-periphery pattern of economic development within nations. The countervailing centrifugal forces making for locational dispersion are those arising from product-market and factor-market competition (such as the bidding up of local land and wage costs). Transport costs and labour (im)mobility are the key determinants making for spatial agglomeration or dispersion: the lower transport costs are, the more the forces of spatial agglomeration will prevail over those of dispersion; the more immobile labour is, the more dispersion will prevail over agglomeration (Krugman, 1991A, 1991B, Krugman and Venables, 1996; Puga and Venables, 1997A, 1997B; Venables, 1996A, 1996B; Ottaviano and Puga, 1997).

More recent variants link this basic model with elements of the new 'endogenous growth theory' that has emerged in recent years by focusing either on inter-regional transfers of human capital or localised technological progress as the mechanisms underlying the locational concentration of economic activity. Bertola (1993), for example, uses an endogenous growth model in the Romer-Lucas tradition to show how capital mobility

¹ It is clearly not possible in the limited space available here to review the full range and subtleties of the new spatial agglomeration models that have appeared within economics in the past few years. The survey by Ottaviano and Puga (1997) offers a fairly comprehensive coverage. In what follows I simply draw out what seem to me to be the main underlying and recurring features of this body of work.

² Note that while Porter (1990, 1994, 1996) emphasises the same basic elements in his discussions of geographical clustering of industry, his descriptive approach contrasts sharply with the formal mathematical models of Krugman, Arthur and other 'new economic geographers'.

and labour migration lead—via localised increasing returns—to the progressive concentration of economic activity and growth in some locations at the expense of others. Bertola uses his model to suggest that by increasing factor mobility, economic integration in Europe is likely to lead to greater regional concentration of production. Authors such as Martin and Ottaviano (1996A, 1996B), Baldwin (1997) and Walz (1996A, 1996B), on the other hand, use a Romer–Grossman–Helpman-type model of endogenous technological innovation to show how localised R&D can generate a process of spatial economic agglomeration. These variants can thus be seen as attempting to respond to a point stressed by Lucas (1988), namely that the economic mechanism at the heart of endogenous growth requires social interactions and external effects which, precisely, are mostly local in nature.

While much of the new economic geography has focused on the regional distribution of industrial activity, the same basic increasing returns models have also been used to model the growth of city systems (Arthur, 1994B; Henderson, 1996; Krugman, 1993B, 1993C, 1996A, 1996B). The essence of Krugman's models, for example, is that people concentrate in cities because of the higher wages and greater variety of goods found there, while firms concentrate in cities because these offer larger markets for their goods. In perhaps a spatial wrinkle on Say's Law, firms create their own markets by concentrating in urban areas. Under certain assumptions about the relative distance–decay of positive versus negative economies of localisation, these ideas are used to derive a range of urban geometries, including Lösch–Christaller-type central place systems, Alonso-type urban land use patterns, and the log-linear 'rank–size' distribution of city sizes (Fujita, Krugman and Mori, 1995; Fujita and Krugman, 1995; Fujita and Mori, 1996; Fujita and Thisse, 1996; Krugman, 1996A).¹ According to Krugman, these urban geometries are all manifestations of 'spatial self-organisation'. This is the claim that the economic landscape displays the same principles of 'order from random growth' to be found in a wide range of physical and biological systems, a notion that also features in Arthur's work in this area (see also Scheinkman and Woodford, 1994).² Other writers, however, have focused less on the geometries of city locations and distributions, and more on the endogenous determinants of urban growth and urban neighbourhood differentiation, particularly the increasing returns associated with localised human capital formation (see, for example, Glaeser et al., 1992; Benabou, 1993, 1994; Durlauf, 1994; Rauch, 1993).

A recurring argument running through these locational agglomeration models is that regional and urban development is characterised by 'path dependence', whereby 'historical accidents', in this case chance locational events, can have long-run cumulative consequences.³ The basic competitive (neoclassical) model of economic equilibrium

¹ Many of these models are essentially more complex versions of the market potential models that were common in economic geography in the 1960s and 1970s.

² Indeed, Krugman's aim in *The Self-Organising Economy* seems to be to derive a mathematically-based universal model of order, structure and evolution which covers both natural and social systems. To my mind, this ambition to construct a mathematical 'meta-model' of physical and social systems is highly flawed. Interestingly, the same totalising goal would appear to be behind Walter Isard's recent move to broaden his regional science focus by using biological evolution theory to explain society as a whole (see his *Commonalities in Art, Science and Religion*, 1997).

³ This is an application of David's (1985, 1988, 1994) 'economics of qwertz' to the industrial location and regional development problem. As developed by David, the 'economics of qwertz' refers to instances where decentralised individual decision-making leads to the 'lock-in' of a pattern that may not be collectively optimal but is nevertheless successful. The term 'qwertz' refers to the top line of letters (QWERTYUIOP) on typewriter keyboards, an arrangement which although originally designed in the nineteenth century and repeatedly shown since to be suboptimal, still lingers on to this day as the norm on contemporary computer keyboards.

implies a unique, optimal and inevitable locational pattern of industry pre-ordained by the initial spatial distribution of resource and factor endowments and transport possibilities. In contrast, Krugman (1991A, 1991B, 1991C) and Arthur (1994A, 1994B) argue that there is an element of uncertainty in industrial location and agglomeration, so that several alternative equilibria are possible. Which particular equilibrium spatial pattern of activity emerges will depend on 'history'. The initial pattern may simply be an 'accident of history' and thus not in any strict sense 'optimal' but, once established, forward and backward linkages and/or self-fulfilling expectations mean that this initial regional or urban pattern tends to become 'locked in' through processes of cumulative causation based on increasing returns. Thus 'irrational' economic decisions can generate suboptimal but equilibrium distributions. Fujita and Thisse (1996) sum this up as 'putty-clay' geography: there is a priori considerable uncertainty and flexibility in where particular activities locate, but once spatial differences take shape they become quite rigid.

To date, the 'new economic geography' has been long on mathematical modelling and exceedingly short on empirical application. According to Ottaviano and Puga (1997), the direct testing of these spatial agglomeration models is still in an infant stage. The fact is that empirical applications have been few and far between, and patchy. This is not surprising. 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.¹ Empirical 'tests' have therefore tended to be of an indirect kind. Thus, for example, authors often simply refer to special 'illustrative' cases, such as Silicon Valley (Arthur, 1994A), the industrial Midwest (Krugman, 1991A, 1991C), the state of Massachusetts (Krugman, 1993D), or individual North American cities (von Hagen and Hammond, 1994), as evidence of the 'consistency' of their models' predictions with real world examples, though empirical data from those cases are rarely used actually to calibrate the models. An alternative strategy has been to derive summary measures of regional patterns of industrial specialisation, often using Gini coefficients or other indices of regional concentration, to see if these are in line with what increasing returns location models imply (for example, Krugman, 1991A, 1993D; Brühlhart, 1996; Brühlhart and Torstensson, 1996; Davis and Weinstein, 1997; Ellison and Glaeser, 1997; Hanson, 1994, 1997A, 1997B). While these studies may provide some support for the role of increasing returns and externalities in spatial agglomeration, their neglect of a host of important forces that also influence the geographical distribution of industry and economic activity (such as the role of local infrastructure, local institutions, state spending and intervention, regulatory arrangements, foreign investment and disinvestment, and global competition), severely limits their explanatory power. Other, much more empirically focused work on the economics of technological innovation has begun to provide some interesting insights into the localised nature of technological spillovers (see, for example, Acs, Audretsch and Feldman, 1993; Antonelli, 1990, 1994; Audretsch and Feldman, 1994, 1996; Jaffe et al., 1993), but so far this seems to have had negligible impact on the spatial agglomeration models of the 'new economic geography'.

To economic geographers, the industrial agglomeration models of the 'new economic geography' generate a dull sense of *déjà vu*. There is a long geographical tradition of using externalities, increasing returns and cumulative causation in urban and regional analysis. Geographers were busy analysing industrial location in these terms back in the 1960s and

¹ In Krugman's *The Self-Organising Economy*, for example, there is not a single application of his models of spatial economic organisation to real world examples, using real world data.

1970s. During the 1980s, the analysis of industrial agglomeration was integrated within a wider theoretical and empirical focus on uneven regional development more generally. Since the late 1980s, interest in industrial agglomeration and externalities has resurfaced in an extensive body of geographical work on 'industrial districts', both new successful areas and old declining ones (for example, Amin and Thrift, 1992; Cooke, 1995; Gertler, 1992; Grahber, 1993; Florida, 1996; Harrison, 1992; Harrison, Kelly and Gant, 1996; Henry, 1992; Markusen, 1996; Park and Markusen, 1994; Phelps, 1992; Pike et al., 1992; Scott, 1988A, 1988B, 1997; Scott and Storper, 1992; Storper, 1992A, 1992B, 1995; Sunley, 1992). Likewise, economic geographers have long recognised the importance of history in shaping the process and patterns of uneven regional development (see Harvey 1982; Marshall, 1988; Massey, 1979, 1984; Smith, 1984), and the consequences of 'lock-in' (or what geographers used to call 'inertia') effects in the rise and decline of urban and regional economies (Grahber, 1993). Similarly, economic geographers have developed extensive research programmes on the key importance of labour and technology in regional development (on labour and human capital, see, for example, Storper and Walker, 1984; Massey, 1984; Clark, 1989; Bennett et al., 1995; Hanson and Pratt, 1995; Martin, Wills and Sunley, 1996; Peck, 1996; Storper, 1995; on technology, see Markusen, Hall and Glasmeier, 1986; Storper and Walker, 1989; Malecki, 1991; Hepworth, 1992; Asheim, 1997; Simmie, 1997; Braczyk et al., 1998). These studies provide greater insight into the role of labour and technology in regional development than do the new spatial agglomeration models.

3. The new economics of regional growth and convergence

The second major strand of 'geographical economics' to have emerged in the past few years focuses on long-run regional growth and convergence, rather than on industrial and urban location, although there are links between the two themes. In the same way that the interest in spatial agglomeration was promoted, in part at least, by the 'new trade theory', so interest in regional convergence has been stimulated by the so-called 'new growth theory' (see Barro and Sala-i-Martin, 1995), based on a reformulation of the neoclassical growth model and the development of augmented and endogenous growth variants (for useful reviews of the growth convergence issue, see Barro and Sala-i-Martin, 1995; *Economic Journal*, 1996; Durlauf and Quah, 1998).

The standard neoclassical (Swan–Solow) growth model assumes diminishing returns to capital and labour. In this framework, a relatively poor country with a lower stock of capital per worker has a higher marginal productivity of capital and a higher rate of return to capital. Hence the model predicts that poorer countries will grow faster than, and eventually catch up with, richer ones. According to Barro and Sala-i-Martin (1995), because there is much greater uniformity of structural, technological, institutional and social characteristics within nations than between them, the neoclassical convergence model is arguably much more applicable at the cross-regional than cross-national level:

Although differences in technology, preferences and institutions do exist across regions, these differences are likely to be smaller than those across countries. Firms and households of different regions within a single country tend to have access to similar technologies and have roughly similar tastes and cultures. Furthermore, the regions share a common central government and therefore have similar institutional set-ups and legal systems. This relative homogeneity means that absolute convergence is more likely to apply across regions than across countries (Barro and Sala-i-Martin, 1995, p. 382).

Using a neoclassical 'growth regression' (in which regional income growth rates are regressed on initial regional income levels) these and several other authors have found that the rate of regional convergence is remarkably similar across the United States, the European Union, Canada, Japan, China and Australia (see, for example, Barro and Sala-i-Martin, 1991, 1993, 1995; Coulombe and Lee, 1993; Cashin, 1995; Cheshire and Carbonaro, 1995; Sala-i-Martin, 1995; Shioji, 1993). However, the observed rate of regional convergence is very slow, about 1–2% per annum, and considerably lower than predicted by the simple neoclassical growth model.¹ These results imply, therefore, either that returns to labour and capital are non-diminishing—or diminish very slowly—or that interregional spillovers of capital, labour and technology are much less than expected, and hence that there are endogenous effects in regional growth. In this context, some studies have found evidence of regional convergence 'clusters' or 'clubs', or, more especially, spatial clustering of regions with similar growth rates (Armstrong, 1995; Canova and Marcet, 1995; Cheshire and Carbonaro, 1995; Quah, 1993, 1994, 1996, 1997A, 1997B); while others suggest that regional growth patterns show conditional convergence to different regional steady-state relative per capita income levels (Evans and Karras, 1996).

The slow rate of regional convergence, and the doubt cast on the validity of the neo-classical model of long-run regional growth, clearly provide a link to the increasing returns spatial agglomeration models. This link has recently received emphasis in the context of the debate over the regional implications of economic integration, not only within the European Union but also within other trading blocs such as NAFTA. Will integration lead to a higher or lower degree of regional specialisation and spatial agglomeration? Will there be regional convergence or divergence? According to the 'new economic geography', the impact of integration on the regional distribution of economic activity and wealth will depend on the relative scale of market-size effects (pecuniary externalities), the lowering of transport costs, and increases in labour mobility between regions. If integration increases pecuniary externalities and labour mobility, the models predict greater spatial agglomeration, and divergence between a rich 'core' and a less prosperous 'periphery' (see Krugman, 1991A, 1991B; Krugman and Venables, 1996). As empirical support for this prediction, authors point to higher levels of regional economic agglomeration and specialisation in the US, which is viewed as the sort of economic union towards which Europe is moving. On the other hand, if labour remains relatively immobile between regions (as is likely to be the case in Europe, because of linguistic and cultural barriers), so that labour and congestion costs eventually rise in the 'core', this will make for the spatial dispersal of economic activity and regional convergence.²

A not dissimilar argument can be found in a related literature concerned with comparing regional labour market dynamics, as between Europe and the US. Stimulated by the seminal paper on 'regional evolutions' by Blanchard and Katz (1992), this body of work argues that regional unemployment disparities are much more persistent (hysteric) over time in Europe than in the US (Eichengreen, 1993A, 1993B; Krugman, 1993, 1996; Decressin and Fatas, 1995; Bertola and Ichino, 1996). This is seen as evidence of the 'inflexibility' of European regional labour markets compared to their US

¹ A rate of 2% per annum (as found for the US) implies that it takes about 35 years for an initial disparity in regional relative per capita incomes to be halved, while a convergence rate of 1% (which seems to typify much of Europe) implies a 'half-life' of around 70 years.

² Taking the US as the model to which the EU is tending is clearly misleading. Not only is labour mobility likely to remain much lower in an integrated Europe than it is in the US, the prospect of a centralised system of automatic inter-regional fiscal stabilisation being implemented in the EU seems an extremely long way off.

counterparts (see, for example, Krugman, 1994A, 1994B). The implication that is drawn is that as European economic and monetary integration proceeds, so regional labour markets will behave more like those in the US. Thus Krugman (1993D) argues that the economies of scale promoted by European economic and monetary integration will lead to greater regional industrial agglomeration and specialisation, so regional labour market dynamics in Europe will become more idiosyncratic, akin to the sort of behaviour that characterises US regions.

The empirical applications of these regional convergence models raise as many questions as they answer. Although a considerable number of empirical studies have been conducted, almost all of these examine only one aspect of regional economic convergence, namely income or output per head. By contrast, there have been few attempts to unravel the relative role of capital flows, labour migration or technological spillovers in the evolution of the cross-region income distribution. Given that the estimated slow rates of convergence suggest there are significant endogenous growth (non-diminishing returns) effects at work in regional income trends, there have been surprisingly few efforts to evaluate whether certain endogenous growth mechanisms are more important than others, or indeed whether different mechanisms operate in different regions. Moreover, even if aggregate regional income convergence is observed, this leaves unexplained the more detailed socio-spatial inequalities in work and welfare that are found within regional and urban areas. Furthermore, as Chatterji (1992), Quah (1993), Pesaran and Smith (1995), and others have argued, convergence models only relate a region's growth to its own history, and not to the inter-regional system of which it is a part. By pooling data for all geographical areas in the system being studied, the growth models assume that the underlying convergence generating process is identical from region to region (see Quah, 1993, Canova and Marcet, 1995). Instead, what surely is of central interest is whether the rate of convergence differs across regions, and if so in what ways, and why. The recent studies by economic geographers of regional convergence within the European Union (see Dunford, 1993; Dunford and Perrons, 1994; Armstrong and Vickerman, 1995) suggest that such variations in regional convergence are indeed significant and that they reflect complex, geographically-differentiated processes.

This focus on long-run regional income convergence merely revives a theme that was first examined more than thirty years ago in the classic works by Borts and Stein (1964) and Williamson (1965). Geographers have never been particularly persuaded by the neo-classical prediction of regional convergence, and have been more disposed to Myrdalian, Kaldorian and Marxian theories that emphasise capitalism's tendency to generate regionally uneven development. More recently, geographers have moved on to episodic theories of regional economic evolution, which recognise that patterns of regional relative growth and decline historically undergo periodic reconfigurations. It is possible, therefore, to observe regional convergence in one period (such as the long, post-war 'golden age' between 1945 and 1973) and divergence in another (as in the period since about 1975). As Barro and Sala-i-Martin (1995) acknowledge, the new regional convergence models, with their asymptotic equilibrium tendencies, are unable to account for such reversals.¹ Economic geographers, in contrast, see these oscillations as inextricably bound up with the periodic systemic shifts in the structure and organisation of capitalism (such as the transition from Fordism to post-Fordism).

¹ Thus they are forced to appeal to ad hoc shocks to account for the recent reversals in regional convergence: in Europe the oil price hikes of the early 1970s, in the USA the policies of the Reagan administrations, and in Japan the excessive growth of Tokyo.

The empirical insight provided by both variants of the 'new geographical economics' is thus limited, and on this basis alone it fails significantly to measure up to the much more detailed and comprehensive work that has appeared in economic geography over the past decade or so. But it is not just this disappointing empirical contribution of both strands of the new 'geographical economics' that is at issue. For there are other more fundamental shortcomings and problems associated with the theoretical and epistemological foundations that underpin much, if not all, of this body of work.

4. The strait-jacket of the mathematical 'mainstream'

The aim of the new economics of spatial agglomeration is clear: to build upon the formalism of the early location-theory models by incorporating new maximisation-equilibrium solutions. The typical approach is based on the use of formal mathematical models of highly stylised abstract economic landscapes to simulate alternative equilibrium patterns of spatial agglomeration under different assumptions regarding the relative role of centripetal and centrifugal forces.¹ Indeed, according to both Arthur and Krugman, it is the recent advances in the mathematical modelling of imperfect competition and multi-equilibrium processes that have provided the 'breakthrough' needed to extend the simple spatial-economic models originally developed by the German location theorists. Thus, in Arthur's words:

In the 1920s and 1930s several members of the great German industry-location school argued that the geographical location of industry was not fully determinate. Where economies of agglomeration existed, so that firms found benefits to being near other firms (a form of locational increasing returns), industry could end up heavily clustered in places chosen by historical accident. But though intuitively appealing, this argument foundered for lack of a theoretical foundation. (1994A, p. 49)

Arthur's solution to the 'lack of a theoretical foundation' is to develop a Polya 'proportion-to-probability' model with which to simulate locational patterns. This mathematical mapping gives the probability that a particular locality will be chosen by the next entering firm, given that locality's current share of the total number of firms. The proportion of firms in a locality converges to a stable fixed equilibrium, where the proportion of existing firms equals the probability of the next firm entering there. When the probability function is non-convex, the model produces multiple fixed points of agglomeration. The model is essentially one of stochastic 'path dependence': the way in which the probabilistic entry process evolves determines the outcome.

Krugman makes a similar point about how the advances in mathematical modelling of economic processes have breathed new life into 'economic geography':

The neglect of spatial issues in economics arises for the most part from one simple problem: how to think about market structure. Essentially, to say anything useful or interesting about the location of economic activity in space, it is necessary to get away from the constant returns, perfect competition approach that still dominates most economic analysis. As long as economists lacked the analytical tools to think rigorously about increasing returns and imperfect competition, the study of economic geography was condemned to lie outside the mainstream of the profession. (1991A, p. 4)

His mathematical approach, which is different from Arthur's, is to develop simultaneous nonlinear equation models of locational attractiveness, typically involving a limited

¹ These highly stylised geographies include linear space economies, one dimensional and infinite cities, regular locational lattices, and so on. The models also assume full employment.

number of key variables (such as local income, transportation costs, local equilibrium wages), which are then solved using Fourier expansions (see his central place model in *The Self-Organising Economy*). These models are used to simulate different locational outcomes ('experiments') by changing key parameters in the equations.¹

As these examples show, the method and content of the new spatial agglomeration models have been narrowly predetermined from the outset. While the interest of Krugman, Arthur, Venables, Ottaviano, Puga, and their co-workers in geography and location may well be genuine enough, as 'mainstream' mathematical economists it was inevitable that they should turn their gaze to location theory and regional science as their version of 'geography', because these fields incorporate precisely the sort of maximisation-equilibrium framework that forms the reference point for their conception of modern 'mainstream' economics.² Underlying that conception appears to be the belief that the only rigorous form of economic theory is that which can be expressed in mathematical notation, or what Krugman calls 'Greek letter' economics.³ Now, clearly, there are aspects of economic development in general, and spatial agglomeration, in particular, that do lend themselves to mathematical representation and modelling. But there are also severe epistemological and ontological limits to such a narrow approach. For one thing, it means that 'messy' social, cultural and institutional factors involved in spatial economic development are neglected. Since these factors cannot be reduced to or expressed in mathematical form they are assumed to be of secondary or marginal importance and, as Krugman puts it, are 'best left to sociologists'. But it is precisely the social, institutional, cultural and political embeddedness of local and regional economies that can play a key role in determining the possibilities for or constraints on development, and thus why spatial agglomeration of economic activity occurs in particular places and not others. Crucial to an understanding of the space economy, then, is an explication of what geographers call the 'institutional thickness' of regions and localities (see Amin and Thrift, 1994, 1995), that is, of spatial variations in the range, density and functions of the institutions (both formal organisations, rules and practices, and informal customs, routines, norms, networks, and so on) that underpin (or in some cases undermine) economic activity, and how and why these institutional geographies themselves develop unevenly across space.

Furthermore, the focus on mathematical modelling results in serious misrepresen-

¹ The following quotation is typical: 'It is a simple matter to set up a circular model with a fairly large number of discrete locations, start with a random distribution of business across these locations, and simply see what happens for a number of values of the parameters' (Krugman, 1996A, p. 103). The dynamic behaviour of Krugman's spatial economic models is one of a sequence of general equilibrium problems. For any given distribution of economic activity across locations, the economy reaches an equilibrium that determines the real wage at each location. This vector of real wages then determines the distribution of workers, and the 'calculation can be repeated until the model economy converges on some long-run equilibrium geographical pattern' (ibid., p. 109).

² I use the term 'mainstream' here in the same sense that Krugman himself does, to refer to mathematically-based, maximisation-equilibrium economics. See Dow (1997) for a slightly broader definition of the 'mainstream'.

³ According to Krugman (in the *Age of Diminished Expectations*, 1997), there are three forms of economics: 'greek letter' economics (mathematical models, which he concedes are largely impenetrable to non-economists), 'up and down' economics (the sort to be found in the business sections of the media, and which he criticises for being 'stupefyingly boring'), and 'airport' economics (popular 'best sellers' usually found in airport bookstores, which he argues are fun but 'rarely well informed and never serious'). Apart from his own commitment to 'greek letter' economics, in his typically self-confident way Krugman has also claimed to have pioneered a fourth genre himself, the high-quality, non-technical popular treatise for the intelligent layperson, represented by his *Peddling Prosperity* (1994D), *The Age of Diminished Expectations* (1997) and *Pop Internationalism* (1996C). To my mind, these works are much more enlightened—and enlightening—than his contributions to 'economic geography'.

tations of processes that are deemed to be important by the new 'geographical economists'. This is the case, for example, with the notions of history and path dependence that figure prominently in the spatial agglomerations models. The 'history' referred to is not real history: there is no sense of the real and context-specific periods of time over which actual spatial agglomerations have evolved (and, in many cases, dissolved). Instead, in the locational models of the new economic geography the notion of time employed is that of abstract logical, or simulation, time. Likewise, 'path dependence' is simply a simulation or solution sequence in which the degree and regional pattern of 'path dependence' is determined solely by the specified initial conditions and parameters of the location model, rather than by a real, complex, locally-embedded and emergent socio-historical process of technological, institutional and social evolution. Essentially, David's concept of path dependence has been hijacked as a device by which the determinacy of the locational models can be rescued by arguing that which particular equilibrium landscape emerges (of the many possible) will depend on the 'initial conditions'.¹ And to complete the misrepresentation, 'history' somehow 'ends' when that spatial equilibrium is reached.² To be sure, Krugman and others recognise that local path-dependence may break down, resulting in substantial shifts and changes in the spatial distribution of economic activity. But such shifts are not adequately theorised and are seen simply as switches from one spatial equilibrium to another. Thus, while the claim that 'history matters' is certainly correct, the treatment of history in the new economic geography is more metaphorical than real and, despite the importance assigned to path dependence, this notion remains a conceptual and explanatory black box.³

The same unease surrounds the growth models used in the studies of regional convergence by Barro, Sala-i-Martin and others. Although these are admittedly much less based on traditional location theory and regional science, the ghosts of maximisation and equilibrium still lurk in the background. All of these studies (including the endogenous growth variants) are rooted in the conceptual and methodological strait-jacket of the Cobb–Douglas production function (for a critical assessment of these regional growth models, see Martin and Sunley, 1998). The commitment to the mathematical mainstream is thus pervasive.

This focus on 'mainstream' economics renders much of the 'new geographical economics' of dubious value. While spatial agglomeration is a key feature of the contemporary economic landscape, so is structural change. The pattern of uneven regional development is not a static one, but is continually evolving, entailing major qualitative as well as quantitative change. Spatial development patterns do get 'locked-in', but not indefinitely; the past is always present, but is not all-determining. Indeed, during the past two decades or so capitalism has been undergoing one of the most dramatic 'gales of creative destruction', to use Schumpeter's graphic phrase, in its history. Few regions or localities have escaped intense structural change; old patterns of regional development have been superseded by new configurations. However, neither the spatial agglomeration

¹ See Lawson (1997) for a critique of the way in which notions of path dependence and history have been appropriated and re-interpreted by mainstream economics.

² One is reminded here of the aptness of Joan Robinson's (1973) wonderfully witty but highly incisive critique of the way in which metaphors of 'space' and 'time' are misleadingly deployed in 'mainstream' economics.

³ In his original work, David (1985) emphasised two major 'carriers of history', or path dependence: institutions and technology. The spatial agglomeration models of Krugman and Arthur make reference to technology but have little to say about institutions. In the Swedish version of evolutionary economics, it is the complex interaction of institutional evolution and technological learning that is at the heart of path dependence (see Magnusson and Ottosson, 1997).

models nor the regional convergence models deal at all adequately with this dynamic, qualitative aspect of spatial development, trapped as they are in the mathematics of spatial equilibria and steady states. It is to other, non-orthodox or heterodox forms of economics that we must turn if these evolutionary and dynamic aspects of the economic landscape are to be treated as causal factors themselves, rather than as ad hoc aberrations from an otherwise equilibrium pattern.

Interestingly, imperfect competition, increasing returns, and cumulative causation were also at the centre of Kaldor's (1970, 1981, 1985) work on trade, endogenous growth and regional development. As Setterfield (1997) has convincingly demonstrated, Kaldor's theories can be extended and elaborated to incorporate not only notions of 'lock-in' and path dependence, but also to allow for the limits to increasing returns, the dynamics of structural change, growth reversal and relative decline (see also Skott and Auerbach, 1995). Likewise, the new evolutionary economics, with its focus on history, institutions, technological change, and human agency offers significant potential for a contextual approach to the study of the economic landscape and its development over time.¹ It is precisely these issues that economic geographers' work on regional development and change has highlighted in recent years. Krugman himself, in his book on *Geography and Trade*, acknowledged the relevance of Kaldor's economics to the study of spatial agglomeration:

This clear dependence on history is the most convincing evidence available that we live in an economy closer to Kaldor's vision of a dynamic world driven by cumulative processes than the standard constant returns model. (Krugman, 1991A, pp. 9–10)

He even goes so far as to suggest that in a sense his own work is only 'a repetition' of Kaldor's ideas. There are crucial differences between the two, however. In his quest for economic 'rigour', Krugman's mathematical formalisation of the processes of industrial agglomeration and uneven regional development has taken the 'new economic geography' well away from the richness of Kaldor's original approach, back into the cul-de-sac of regional science. Kaldor, one suspects, would have been extremely sceptical of the unrealistic, deductive model-building that is the hallmark of the 'new economic geography'.

5. Where's the geography? The neglect of real places

This links to another serious flaw in the 'new geographical economics', namely its neglect of real places. This neglect is most obvious in the mathematical models of spatial agglomeration. Here 'regions' or 'locations' are often just points along a linear economy, or concentric circles in a von Thunen-type landscape, or point-patterns in a Christaller-type surface. Real communities in real historical, social and cultural settings with real people, going about the 'ordinary business of life' (as Marshall once described economics) are completely bypassed. The fundamental and complex question of how 'regional' and 'local' economies can be meaningfully conceptualised, 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

¹ There are of course several variants of evolutionary economics (see Hodgson, 1993; Magnusson and Otosson, 1997; Reijnders, 1997). The case for a contextual-institutional approach to economic geography is set out by Sunley (1996).

empirical purposes, on the other. So cavalier is the treatment of space and place that the same model is often used to explain spatial agglomeration and specialisation at vastly different scales, from the international level, to broad core-periphery patterns within nations, to local urban industrial concentrations, and even intra-urban neighbourhoods. Processes are thus assumed to be largely scale-independent. For economic geographers, however, the issue of spatial scale is central. A given process or event (such as the local opening or closure of a foreign-owned plant in a particular location) can have different meanings and different implications at different spatial scales (the local, the national and the international), yet at the same time link those scales together. It seems likely that different forms of externality operate at different geographical scales. Although there is some suggestion in the 'new economic geography' literature that pecuniary (market-size) externalities operate over wider regional spaces than technological and informational externalities, which are more localised, how these different scale-dependent processes might intermesh and interact is not discussed. The spatial agglomeration models may well predict that, under specific assumptions, industrial localisation and specialisation will occur, but they are unable to tell us where it actually occurs, or why in particular places and not in others.

The regional concept used in the regional convergence models is also problematic. The interest in regional convergence by the new growth theorists derives as much if not more from the idea that regions offer a more 'controlled' test of their models (as compared to cross-national growth trends) as from a basic desire to provide new insights into the process of long-run regional growth and development. After all, according to Barro and Sala-i-Martin (1995), the virtue of regional convergence studies is precisely that regional differences in social, cultural institutional and regulatory characteristics can be assumed to be negligible. Even industrial-structure differences between regions are assumed to be unimportant (or simply reduced to dummy variables). It is as if convergence is assumed to operate at the regional level precisely because of the lack of geography. Thus, while the spatial agglomeration theorists recognise that locally-varying social, cultural and institutional factors may be important in shaping the economic landscape, but exclude them because they are not easily incorporated into formal mathematical models and cannot be 'rigorously' analysed, the regional growth and convergence theorists assume such factors to be insignificant in the first place. At the same time, there is little or no discussion of whether there is an appropriate regional scale at which to analyse convergence, nor analyses that seek to determine whether different trends in regional convergence may be occurring at different spatial scales. Yet, one of the intriguing features highlighted in recent years by economic geographers is that differential scale-specific trends in spatial economic disparities do appear to be at work in advanced capitalist economies, in that local differences in growth, employment and incomes have increased while broad regional disparities have tended to narrow (Dunford, 1993; Dunford and Perrons, 1994). These trends would seem to raise some major questions for the study of regional convergence.

This failure to take real geography and real places seriously takes its cue from traditional regional science, which has long suffered from this limitation. However, a critical self-assessment is currently emerging in some quarters of regional science studies which questions this very shortcoming. In recording forty years of the subject, two recent issues of the *International Regional Science Review* (1995A, 1995B) noted the field's preoccupation with abstract models, mathematical theorising and idealist methodology; the weakness of its links with policy and practice; and the lack of in-depth analysis of specific areas (see Bolton and Jensen, 1995; Isserman, 1995; also Isserman, 1993). There is now a

growing call for the discipline to refocus its analytical lens on the study of real places rather than imaginary spaces (Bolton and Jensen, 1995), to escape the strait-jacket of mainstream equilibrium economics, and to widen its conceptual base to engage with social theory (Warf, 1995). Whether it will manage to break free from its formal, abstract theorising, or whether the forces of intellectual 'lock-in' will prove too strong, remains to be seen. But the fact that a process of reassessment is at least taking place in regional science surely raises major questions about the wisdom of trying to build a 'new economic geography' on what are now at last being recognised as suspect intellectual foundations.

The contrast of the 'new geographical economics' with economic geography proper could not be greater. It is not possible to review the entire field of economic geography here (for a comprehensive survey of the current theoretical, empirical and methodological diversity of the subject, see Lee and Wills, 1997).¹ But certain key features and emphases are worth highlighting. The conceptual basis of economic geographers' work on the 'resurgence of regional economies' has not been equilibrium location theory or the new growth theory, but instead approaches that emphasise the political, economic, institutional and social bases of regional development and industrial agglomeration. A number of interlocking literatures and theoretical frameworks have informed and shaped this work. An initial influence was Michael Piore's and Charles Sabel's (1984) *Second Industrial Divide*, with its argument that we are witnessing the emergence of a new industrial-technological paradigm based on 'flexible specialisation' (see the excellent assessment by Gertler, 1992). Another has been the neo-Marshallian work of a group of Italian economists researching the industrial districts of the 'Third Italy'.² These economists differ significantly from their spatial agglomeration and regional convergence modelling counterparts in that their approach is firmly rooted in detailed empirical work on specific regions and stresses the social, cultural and institutional foundations of local industrial growth. Their work is referred to rarely, if at all, by the spatial agglomeration modellers, but has had a far greater impact than the latter on economic geographers.³ The Italian industrial districts literature has encouraged economic geographers to focus on the networks of trust, cooperation, competition and governance that characterise such areas.

A third influence has been French regulation political economy, with its theory of a contemporary transition from 'Fordism' to 'post-Fordism',⁴ and its stress on the mode of social regulation that supports and facilitates economic accumulation (see Amin, 1994). This has not only provided a 'big picture' framework within which the resurgence of regional economies can be conceptually situated, but more specifically has alerted geographers to the role that social regulation and institutional forms play in enabling and constraining local economic development. Geographers have also begun to draw upon the technological learning literature (for example, Lundvall, 1992), to focus on the nature and role of geographically-constrained untraded interdependencies such as technological

¹ Apart from research on spatial agglomerations and industrial districts, contemporary economic geography covers topics including the geography of money, consumption, services, corporate organisation and behaviour, labour and labour markets, welfare, state intervention, globalisation, trade, institutions, social regulation, and other themes besides. Here I simply highlight the work being done on spatial agglomeration and localisation so as to make direct comparisons with the new 'geographical economics'.

² This is the third strand of the new 'geographical economics' that I referred to above. Examples of Italian industrial district economists include Antonelli (1990, 1994), Brusco (1989), Sforzi (1989), Becattini (1990), Garofoli (1991), to name but a few.

³ Despite the fact the 'Third Italy' is probably the most celebrated—and debated—case of modern-day industrial localisation, I can find no mention of the work of these Italian authors in Krugman or Arthur.

⁴ There are also German variants of regulation theory, but it has been the French regulationists (Aglietta, Lipietz, Boyer, etc.) who have been most influential on economic geography.

spillovers, knowledge circulation, technical know-how and learning among the workforce (see Storper, 1995), and on whether and in what ways innovation is concentrated within area-based networks, or 'learning regions' (Storper, 1993; Maskell and Malmberg, 1995; Simmie, 1997; Braczyk et al., 1998). Another recent focus is on the economics of sunk costs and how these bear upon the spatial configuration of the firm and the geography of economic activity (Clark and Wrigley, 1997). Yet another emerging theme highlights the cultural bases of industrial organisation and corporate behaviour and how these interact with and in turn shape local-regional production and employment relations (Gertler, 1993; Schoenberger, 1997).

This theoretical and empirical pluralism offers more explanatory scope than the theoretical monism of the 'new economic geography', although there is arguably a need to impose some overall conceptual coherence.¹ But what does unite these various themes in economic geography is 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. For example, as Markusen's excellent (1996) survey makes clear, there is no one single type of industrial district or agglomeration; rather, such districts differ considerably in origin, economic structure, social regulation, institutional organisation and degree of political intervention. This diversity may well limit the ambition and success of constructing an over-arching universal model of spatial agglomeration. In fact, as Massey (1992) has emphasised, the relative locations of economic activity may be produced out of the independent operation of separate determinations, of happenstance juxtapositions, which may then give rise to particular emergent powers. Thus, the explanation of local agglomerations, of spatial economic differentiation necessarily involves close explication of locally specific and contingent factors as well as deeper, more general processes. Furthermore, as Cox (1996) and Storper (1993, 1995) argue, particular places are characterised by particular types of path dependence, and economic agents are not just locked into a particular developmental path but also into that particular place where their (path-dependent) powers can be exercised. Over time, different places develop different specific networks of inter-firm dependencies (both traded and untraded), specific institutional forms (including labour market rules and forms of industrial relations), and specific sets of economic, cultural and political practices (such as labour militancy and social attitudes) which become socialised and institutionalised and serve to influence the role that different places play in successive rounds or phases of economic accumulation. Path dependence thus has a place-dependent character. Path dependence does not just 'produce' geography as in the 'new economic geography' models; places produce path dependence.

6. 'Geographical economics' and economic geography: irreconcilable discourses?

Thus while location-theoretic models in the tradition of Weber, Lösch, Christaller, Alonso and Isard figure prominently in the 'new economic geography', they have long since disappeared from the research frontier in economic geography. It is precisely this disappearance that Krugman laments. In his *Development, Geography and Economic Theory* (1995) he identifies what he calls the 'five lost traditions' of economic geography: Ger-

¹ Nevertheless, as Lagendijk (1998) points out, the success of the 'new regionalism' in economic geography can be attributed to the way in which, while evolving individually, the various concepts used have tended to converge around key metaphors, such as embeddedness, networks, and governance.

manic location theory, social physics (gravity and potential models), cumulative causation, land use and land rent models, and local external economies. The reason why these languished, why they became 'outcast', he argues, was not because geographers rejected modelling as such, but because they lacked the sort of complex mathematical tools needed to develop these models further. As he puts it: 'One cannot fault the geographers for their failure to develop full maximisation-and-equilibrium models—although one can perhaps complain about their failure to understand how far short of that ideal they were falling. And one can understand the reluctance of the mainstream economists to muddy the clarity of that mainstream with the somewhat murky modelling efforts of the geographers' (ibid., p. 87). However, according to Krugman, relief is now at hand in that the developments in mathematical economics over recent years offer the opportunity to revive these 'lost traditions' and to 'integrate spatial issues into economics through clever models (preferably but not necessarily mine [sic]) that make sense of the insights of the geographers in a way that meets the standards of the economists' (ibid., p. 88).

Krugman is wrong in his explanation of the 'five lost traditions' of economic geography. These had largely disappeared from geography by the late 1970s not because of geographers' 'failure to understand how far short of the ideal they were falling'. Rather, they were deliberately abandoned on philosophical and epistemological grounds, as part of the large-scale movement away from logical positivism that occurred in geography at that time. The location-theoretic, regional science models were cast aside not because the mathematics of maximisation-and-equilibrium had (temporarily) reached their limits, nor because geographers were unable intellectually to elaborate those mathematical tools, but precisely because of the realisation that formal mathematical models impose severe limits on our understanding. Geographers became more interested in real economic landscapes, with all their complex histories and local contexts and particularities, and less entranced by abstract models of hypothetical space economies (Sheppard, 1995).

The key point is that the work of economic geographers and the new 'geographical economists' represent quite distinct methodological and epistemological genres. While the 'geographical economists' strive to build ever more complex mathematical regional science models of the space economy, economic geographers have turned to discursive modes of theorising and intensive forms of empirical investigation. Methodologically, geographers have abandoned positivistic accounts in favour of realist approaches, in which explanations are built 'from below', often relying upon close dialogue with individual agents and organisations, and linking this 'local' knowledge with wider, larger stylised facts and conceptual frameworks (see Clark, 1997, for a discussion of this methodology).¹ In the new 'geographical economics', by contrast, mathematical tractability drives the focus rather than the apparent diversity of economic systems. For the new 'geographical economists', the 'complexity' of the economic landscape (Krugman, 1994C) is one of mathematical solvability, rather than of empirical messiness and particularity.

At the heart of the difference between economic geography and the 'new geographical economics', therefore, is a fundamental difference of view about 'theory' and modes of theorising. 'Theory' in the 'new geographical economics' is assumed to be synonymous with formal, mathematical model-building: the method is one of deductivist, mathematical demonstration. In much of economic geography, the dominant mode of theo-

¹ The exploration of realist methods and approaches has also surfaced, of course, in economics (see Lawson, 1997, for a compelling manifesto). Realist methods are quite at variance with the idealist models that underpin mainstream economics.

rising is one of discursive persuasion. For Krugman, such an approach is not only inferior, but also self-deluding:

Many of those who reject the idea of economic models are ill-informed or even (perhaps unconsciously) intellectually dishonest... The problem is that there is no alternative to models. We all think in simplified models, all the time. The sophisticated thing to do is not to pretend to stop, but to be self-conscious—to be aware that your models are maps rather than reality... Some of us are self-aware: we use our models as metaphors. Others ...are sleepwalkers: they unconsciously use metaphors as models... [E]conomic thinkers who imagine that they have broadened their vision by abandoning the effort to make simple models have done no such thing. All they have really done is to use high flown rhetoric to disguise, not least from themselves, their lack of clear understanding. (Krugman, 1995, pp. 79–80)

Economic geographers would retaliate by arguing that the issue is not whether we all use ‘models’ of one sort or another in our explanations (few would dissent from such a view), but rather the nature of the ‘model’ that is used. They would argue that it is the mathematical model-builders who are deluding themselves into believing their deductivist theorising is somehow superior, and that discursive modes of theorising permit the construction of much richer ‘maps’ or representations of reality.

What in fact is striking about the ‘new economic geography’ is its almost total lack of awareness of any of the large corpus of work by economic geographers. According to Krugman, this is the fault of the geographers:

The decision by international economists to ignore the fact that they are doing geography wouldn’t matter so much if someone else were busy...looking at localisation and trade within countries. Unfortunately nobody is. That is, of course an unfair statement. There are excellent geographers out there... However,...economic geographers proper are almost never found in economics departments, or even talking to economists... They may do excellent work, but it does not inform or influence the economics profession. (Krugman, 1991A, pp. 3–4)

This is an extremely patronising view, and one that has received well-deserved criticism from geographers (see Hoare, 1992; Johnston, 1992; Dymski, 1996; Martin and Sunley, 1996). It could equally well be argued, of course, that it is the geographical economists who have failed to talk to the economic geographers; that the unwillingness of most economists (and not just the new ‘geographical economists’) to read outside the narrow confines of their own subject means that they are largely ignorant of the major developments that have been taking place in economic and industrial geography over the past decade or so.¹ Thus we find that in his *Development, Geography and Economic Theory*, Krugman refers to only two recent major geographical works, Dicken and Lloyd’s *Location in Space* (1990) and Chisholm’s *Regions in Recession and Resurgence* (1990). Others, such as Arthur, Quah, and Barro and Sala-i-Martin, do not refer to the work of geographers at all. Krugman goes so far as to dismiss contemporary economic geography as an ‘anti-model, anti-quantitative backlash’. The giveaway, Krugman complains, ‘turns out to be the phrase “post-Fordism”’: if you see that, it means that you are dealing with a member of the Derrida-influenced regulation school—deconstructionist geography!’ The idea that regulation theory is deconstructionist is, of course, quiet absurd.² Likewise,

¹ Symptomatic of this unwillingness to engage with economic geographers is the fact that, as far as I am able to ascertain, none has been invited to join the regular workshops on the ‘new economic geography’ held by the CEPR in London and elsewhere.

² This is not to say that economic geography (and human geography more generally) has remained immune from the deconstructionist ‘post-modern turn’ that has swept through much of social science over the past

Krugman's claim that the insights of geographers need in any case to be reworked (through 'clever models') so as 'to meet the standards of the economists' is equally arrogant. Whether mathematical economists have any such monopoly over analytical or theoretical standards can most certainly be challenged.

On this basis, the prospects of a conversation of the sort hoped for by Krugman would not seem particularly encouraging: a 'dialogue of the deaf' seems more likely. This is not to say that no scope exists at all for useful interchange between the two fields. Of the various models and theories that make up the 'new geographical economics', the regional and urban endogenous growth models, which at least seem to incorporate some awareness of the locally specific nature of technological spillovers, human capital formation, and learning-by-doing, would seem to hold out some possibilities for a cross-fertilisation of ideas between geographers and economists. But it is between economic geography and the various strands of non-orthodox economics (such as evolutionary, institutional, social, post-Keynesian, and Kaldorian economics) that more fruitful scope for dialogue exists. To date, non-orthodox economists appear to have been even less interested in space, place and location than have orthodox, mainstream economists. However, as geographers are showing, if economies of scale arise primarily at the local and regional scale, equally the processes emphasised by heterodox economics—such as technological innovation and learning, social and cultural regulation, institutional evolution, and path dependence—also have significant local and regional dimensions. Just as economic geographers have found the ideas and concepts of heterodox economics useful in their explanations of regional development, so heterodox economists could learn much from the work of economic geographers.

7. Conclusion: a case of mistaken identity

For the vast majority of economists, there are just two levels at which economic processes operate and at which theoretical exposition is needed: the micro-level of individual households and firms, and the macro-level of nation states. These two levels constitute 'the economy'. In reality, economic life is conducted in and across space (local, regional, national and global): it is organised geographically, and this spatial organisation has a crucial bearing on how the economy functions, on the performance of individual firms and on the welfare of individual households. It is not merely a case of recognising that the mechanisms of economic development, growth and welfare operate unevenly across space, but that those mechanisms are themselves spatially differentiated and in part geographically constituted; that is, determined by locally varying, scale-dependent social, cultural and institutional conditions. This is the stuff of economic geography proper. The question posed all those years ago by Lösch, of 'how the economy fits into space', is not one that can be answered by the abstract geometric solutions of formal, mathematical location models, nor by treating regional and local economies as if they are simply special ('scaled-down') cases of macro-economic processes or straightforward aggregations of micro-economic behaviours.

While the new 'geographical economists' should, I suppose, be applauded for wanting to persuade their colleagues that 'space matters' (Krugman, 1991A, p. 8), there is only

few years. It has been no less affected than other subjects. But to argue that regulationist-theoretic economic geography is deconstructionist is to reveal a lack of understanding of regulation theory. The latter is precisely the sort of 'meta-narrative' that post-modernists reject. There are Keynesian and Marxist versions of regulationist political economy (see Boyer, 1990).

one conclusion to draw from this review: their version (and vision) of economic geography is a case of mistaken identity. Krugman is right to argue that economists should start doing economic geography because of the 'intellectual and empirical laboratory it provides'. But that task still remains to be carried out. For what they have been working at is not economic geography, but a revamped regional science and regional economics. And as with regional science, the 'new economic geography' contains too little region and too much mathematics. Geographers have been rightly critical, therefore, of the claims made for the novelty and significance of 'new economic geography' (Dymski, 1996; Martin and Sunley, 1996).

It is possible to criticise the new economic geography both from without—that is, from the vantage point of economic geography proper—and from within—from the vantage point of other (non-'mainstream') forms of economics. Krugman himself is aware of these sources of criticism: as he has remarked, 'I am having a terrible time with my current work on economic geography; referees tell me that it's obvious, it's wrong, and anyway they said it years ago' (quoted in Gans and Shepherd, 1994, p. 178). His supremely self-confident style, he concedes, has conveyed an impression of 'maybe claiming more originality than I really have' (ibid.). Despite these criticisms, however, the movement continues to flourish, with more and more mathematical-theoretical papers on industrial location, spatial agglomeration and regional growth continuing to appear. More worryingly still, perhaps, is that some supporters feel the field is now sufficiently advanced to begin to move into the policy realm (see Puga, 1996, 1997). This prospect should be viewed with dismay. Regional science, as some of its exponents now openly admit, has not had much impact on—or indeed relevance to—policy issues. On its present showing, the 'new economic geography' seems unlikely to improve on regional science's record. Abstract models attached to the real economic landscape by the thinnest of conceptual and empirical threads do not, in my view, offer particularly convincing or reassuring material from which to weave policy prescriptions.

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