Book Review of:

ECONOMIC GEOGRAPHY AND PUBLIC POLICY

By Richard Baldwin, Rikard Forslid, Philippe Martin,

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J. Peter Neary[1]

University College Dublin and CEPR

[1] Address for Correspondence: UCD School of Economics, University College Dublin, Belfield, Dublin 4, Ireland; tel.: (+353) 1-716 8334; fax: (+353) 1-283 0068; e-mail: peter.neary@ucd.ie.

This book explores the implications of the "new economic geography" for the theory of economic policy. As the authors note, much of the interest in this topic comes from its potential importance for policy, yet previous writers, including the originators of the approach, have avoided explicit discussion of policy issues. (See, for example, Fujita, Krugman and Venables (1999).) The authors argue persuasively that there are two reasons for this. First, the original new economic geography models cannot be solved analytically, so recourse to numerical methods is needed even to determine their properties when countries are ex ante identical, far less to solve the inherently more complex problems that arise in applying them to policy issues. Second, the central externalities in new economic geography models imply that "Agglomeration is unambiguously good for you". (The authors generously credit this argument to Neary (2001), but I am sure it has been implicit in the literature from the beginning.) Firms want to locate near their customers, workers want to locate where the cost of living is low. Taken together with the standard assumptions of new economic geography models (Dixit-Stiglitz preferences, increasing returns, and iceberg transport costs for manufactures), these imply that prices and/or costs will be lower in more agglomerated locations. This may be plausible in general, but its implications for policy are "too stark to be true": any policy (such as protection) which encourages in-migration of mobile factors will lower prices and raise welfare. Hence the policy implications of standard new economic geography models seem alarmingly like a restatement of import-substitution-led industrialization, which we know from the sad experience of many countries is rarely successful in practice.

This book sets out to counter these two barriers to applying new economic geography to policy issues. Part I deals with the theoretical objection by presenting in a unified framework both the original new economic geography models of Krugman and Venables and a number of more tractable alternatives recently developed by the authors (in various combinations). The remaining parts deal with the "too stark to be true" objection by exploring the models' implications for welfare economics as a whole and for a host of applied questions. Is it successful overall? In my view the first goal is brilliantly realized. Fans of new economic geography now have at their disposal a comprehensive catalogue of models with their properties clearly presented and compared. And those (if any) who are less enthusiastic about the approach also benefit, since the simpler models will allow them to cut in half the time they need to convey the basic results to their students! As for the second goal, I am not persuaded that it is fully realized, but there are many fascinating and novel policy applications to be found in these pages.

With (by my count) eleven different models in two hundred and eighteen pages, Part I may lead some readers to groan "So many models, so little time"! However, despite some repetition (which has the benefit that individual chapters are self-contained), there is a lot of good stuff here. It begins with a comprehensive and far from elementary exposition of the original CP (Core-Periphery) model of Krugman, including an appendix on "Everything you wanted to know about Dixit-Stiglitz but were afraid to ask"! Like most of the book, this concentrates on a special case which I suggested in Neary (2003) should be called "Dixit-Stiglitz lite" since it does not appear in the original: a symmetric CES sub-utility function for manufactures, which, along with the output of a perfectly competitive "agriculture" sector, is one of the arguments of an upper-tier Cobb-Douglas utility function. The presentation highlights the "fascinatingly complex results" (p. 27) of the CP model: at all levels of trade costs there are multiple equilibria, including a symmetric one with manufacturing evenly divided between two ex ante identical countries, and two asymmetric ones, in which one or other country prospers as the "core," hosting all manufacturing production, while the other languishes as the "periphery". The mechanism for selecting between equilibria is international mobility of manufacturing workers, and under both myopic and forward-looking expectations (the latter a welcome innovation in this book), their migration behaviour leads to a complex pattern of stable equilibria. For high trade costs, only the symmetric, fully diversified equilibrium is stable: imports cannot compete with local manufactures, so firms always have an incentive to produce locally. By contrast, for low trade costs, only the agglomerated equilibria are stable because the benefits of agglomeration outweigh the disadvantages of facing more local competition. Either country can end up as the core, and which one actually does so depends on factors that are not explicitly modelled, with an initial tiny advantage by one country leading it to attract the whole world endowment of the mobile factor. Finally, for intermediate trade costs all three equilibria are stable, so the model exhibits locational hysteresis or path dependence: for example, the same values of all exogenous variables may correspond to a diversified equilibrium as trade costs are gradually lowered, but to an agglomerated one as they are gradually raised.

These distinctive features of the CP model come at the cost of considerable analytical complexity, however. Hence the remainder of Part 1 proceeds to outline a range of alternative models, starting with the two main models used in the book: the FC (Footloose Capital) and FE (Footloose Entrepreneur) models. These two share a key assumption which makes them much more tractable than the CP model. Whereas the CP model assumes that manufacturing production is homothetic, so both fixed and variable costs use only the mobile factor, the FC and FE models impose an extreme form of non-homotheticity, with the mobile factor used only for fixed costs while variable costs require the same immobile factor that is used in agriculture. This assumption, which originates with Lawrence and Spiller (1983) and was also used by Flam and Helpman (1987), has proved useful in many contexts. In the new economic geography context it leads to an enormous simplification, since it breaks the link between manufacturing prices and the wage of the mobile factor. Manufacturing prices are independent of fixed costs, so they depend only on the local price of the immobile factor. But this is the same in both countries, because trade in agricultural goods (which require the immobile factor only) is assumed to be free. Hence producer prices for manufactures are equalized internationally, and so consumer prices differ between countries only because of trade costs. Since the model can in effect be solved separately for prices and mobile-factor wages, explicit analytic solutions for many of the variables of interest can be obtained.

While the FC and FE models share the non-homotheticity assumption, the former makes a second assumption which simplifies its properties even further, though at the cost of eliminating many of the distinctive new economic geography predictions. Specifically, it assumes that the mobile factor spends all its earnings in its country of origin, *not* its country of location. Income repatriation such as this is appropriate for capital, and also fits the case of migrant workers who remit all their earnings to family members at home. This single change in assumptions affects the model's properties a lot: international shifts in the location of mobile factors do not now depend on the local price index, so there is no cost linkage between countries, and they do not give rise to shifts in expenditure, so there is no demand linkage. As a result, catastrophic agglomeration cannot occur. By contrast, in the FE model, as in the original CP model, migrants (now identified as "entrepreneurs") spend all their income locally, so shifts in productive factors generate shifts in spending and give rise to cumulative causation. The FE model is intermediate between the FC and CP models both in tractability and in the richness of its predictions. In the FE model, the mobile factor accounts for only part of the cost of producing manufactures. Relative to the CP model, this weakens the link between production shifting and demand shifting, so agglomeration forces are weaker in the FE model.

Later chapters in Part I consider many other models. Some are old, like the Venables variant of the original CP model (where agglomeration is driven by intermediate goods rather than factor mobility) and its FC and FE extensions. Others are new. For example, what the authors call "linear" models replace Dixit-Stiglitz CES preferences with quadratic utility, implying demand functions that are linear rather than iso-elastic. This makes it possible to explore the effects of changes in the degree of substitutability between goods (whereas in Dixit-Stiglitz both own- and cross-elasticities depend on the elasticity of substitution). Models with capital accumulation rather than reallocation and with endogenous growth arising from knowledge spillovers are also explored in detail. However, the majority of the applications later in the book rely on either the FC or FE models. They constitute a valuable addition to the modelling portfolio of international trade theory and are sure to find many more uses.

Part II of the book turns to some general issues of policy and welfare. For example, it highlights the implications of economic geography models for threshold effects and hysteresis, which I like to call the "ketchup bottle effect": "First nothing happens, then nothing happens, then it's all over the place." Marginal changes in exogenous parameters (including policy ones) have initially no effect on the equilibrium. But once they pass a threshold level, there is a discontinuous or catastrophic shift from one equilibrium to another, and "Typically the outcome will be a massive delocation of industry" (p. 228). This part also gives a general typology of welfare effects, and explores trade-offs between equity and efficiency. The discussion is comprehensive, but I would have liked to see more attention devoted to two issues. First, conspicuous by its absence, is the principle of targeting. This is surely one of the most useful contributions of the traditional theory of trade and welfare, and should be relevant in new economic geography models too. Protection may induce reallocations that will raise welfare, but there are likely to be other more efficient ways of achieving the same end (unless manufacturing imports are eliminated instantaneously). Second, mentioned at a number of points but not highlighted, is the difficulty of making welfare judgements when some individuals are mobile. Should the welfare of the core count only those residents who were there before agglomeration occurred? Should the welfare of the periphery include those initial residents who have left to work in the core, and who consume all their income there? Do our views on these issues depend on how recently the migrations have taken place? There is surely a path dependence to our normative judgements as well as to the positive predictions of these models, and more discussion of these difficult issues would have been welcome.

Finally, on page 275 we get to what the authors call the "meat" of the book: Parts III, IV and V which apply the models to trade policy, tax policy and regional policy, respectively. Some of the trade results have been demonstrated before, but only using simulations. Here the authors are able to show analytically that unilateral trade liberalisation between identical countries has no effect on location until the threshold level is reached, beyond which it leads to catastrophic agglomeration. In the same vein, they show that reciprocal trade liberalisation between countries of different sizes encourages industry to relocate from the small to the large country. (Comparative advantage can offset these effects, a point to which I return below.) They also break new ground by considering the case of more than two countries. With many symmetric countries differing only in size, uniform multilateral liberalization favours industrialization in larger than average countries and delocation from smaller than average ones. Countries deindustrialize fully in order of size (smallest first), until eventually (before trade is fully liberalized) all industry agglomerates in the country with the largest market. As for preferential liberalisation, it triggers a two-tier home-market effect: industry is encouraged to locate in the preferential trading area, and within that area industry becomes more concentrated in the largest member country.

Later chapters turn to tax policy and show that, when trade costs are low and agglomeration forces strong, many of the standard results of the tax competition literature cease to hold. An important source of this is that agglomeration creates rents, and these can be taxed up to the point where the agglomerated equilibrium is eliminated. Hence, once having acquired all or most of world industry, the core country can impose higher capital taxes without risking deindustrialization. The opposite is true if agglomeration has not occurred: now the equilibrium is highly fragile and even a tiny tax may lead a country to lose all its capital. Traditional results on tax competition may also be overturned, though here the outcome is sensitive to the choice of model. The FC model exhibits the standard result that tax competition yields tax rates below the socially optimal level. However, the FE model is different: now the public goods financed by the taxes are enjoyed by the mobile factor only when it moves to the taxing country, and so a variant of the Tiebout hypothesis holds: tax competition is efficient.

The final chapters of the book turn to regional policy, showing how infrastructural policies which facilitate knowledge spillovers between regions can simultaneously raise average growth and lower interregional income disparities, and also considering the political economy of regional subsidies.

Implicitly throughout the second half of the book, and explicitly in their discussion of trade policy, the authors take up the challenge of my "too stark to be true" remark. As they note, the key feature underlying it is the "price-lowering protection" (PLP) effect in new trade theory models: protection attracts new firms, which lowers the home price index. The same outcome can occur in neoclassical models (where it is called the Metzler paradox). However, the mechanism is very different. Whereas the Metzler paradox comes from inelastic world demand (so world prices fall by more than the tariff), the PLP effect comes from the very strong sensitivity of the CES price index to the number of varieties. It is this which makes protection so powerful an instrument of industrialization in all the models considered.

The authors show that the PLP effect can be modified by assuming that relocation of firms is costly: this yields a hysteresis band within which protection does not affect location decisions and so does not encourage industrialization. But outside the band the PLP effect continues to apply. They also show that, if comparative advantage is sufficiently important, then the PLP effect is offset. (I will refrain from dwelling on the delicious irony of the authors invoking comparative advantage as a way of justifying the policy relevance of a new economic geography model!) In sum, if agglomeration forces are thwarted by relocation costs, or offset by locational advantages, then agglomeration will not occur and implausible policy prescriptions will not follow. But this seems more like an admission of the limitations in applying the models to policy issues than a defence of their relevance. The authors conclude that the PLP effect is "an artefact of several simplifying assumptions rather than a deep result" (p. 297), but the assumptions are precisely those of the new economic geography approach.

In conclusion, an overall assessment of this book is hard to separate from one's views about the new economic geography as a whole. While I find much to admire in this literature, I have some residual worries. One, discussed in Neary (2001), is the universal assumption of monopolistic competition, including in particular an infinitely elastic supply of ex ante identical firms. For many policy issues, such as competition policy which is mentioned briefly in the concluding chapter, oligopolistic models with some limitations on entry seem more appropriate. Another worry is that, while path dependence seems self-evidently a feature of the world, it does not follow that it must be a property of our models. From Kaldor's 1940 model of the trade cycle onwards, economists have been fascinated by the potential for "captivatingly complex behaviour" (p.

27) of models with multiple equilibria. But simpler explanations of empirical phenomena are often available, and by Occam's Razor they will usually be preferred. Moreover, taking models of multiple equilibria to data is a serious challenge. Recent work by Davis and Weinstein (2002) and Redding and Sturm (2005) among others makes an interesting start in this direction, but to date they do not provide persuasive evidence that multiple equilibria arising from agglomeration-related pecuniary externalities are pervasive.

These worries about the general approach aside, the book succeeds admirably in showing that new economic geography can throw interesting light on policy issues. The authors' enthusiasm for their topic is hard to resist and the book is a joy to read. Only curmudgeonly lapsed old trade theorists (CLOTTs) will find much to complain about in their exciting mix of interesting models, intriguing results, and apt acronyms.

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