Can we learn anything from economic geography proper?

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Abstract

This paper considers the ways geographers (proper) and (geographical) economists approach the study of economic geography. It argues that there are two areas where the approach of the latter is more robust than the former. First, formal models identify which assumptions are crucial in obtaining a particular result and enforce internal consistency when moving from micro to macro behaviour. Second, empirical work tends to be more rigorous. There is much greater emphasis on identifying and testing refutable predictions from theory and on dealing with issues of observational equivalence. But any approach can be improved and so the paper also identifies ways in which geographical economists could learn from the direction taken by economic geographers proper.

Keywords: Economic geography, geographical economics, regional science, relational economic geography

JEL classifications: B41, B52, F12, R00

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“I don't know what they have to say,

It makes no difference anyway.

Whatever it is, I'm against it.”

– Groucho Marx in “Horse Feathers”

1. Introduction

A recent special edition of the *Journal of Economic Geography*[1] was completely devoted to what Boggs and Rantisi (2003) call the ‘relational turn’ in economic geography. The research agenda outlined in that special issue is one approach that has been advanced in response to the paper by Amin and Thrift (2000) calling for a *cultural turn* in the study of economic geography.[2] It is not the only response. Indeed papers published in a special issue of *Antipode* in 2001[3] suggest a range of possible alternatives including the aforementioned *relational turn* (Ettlinger, 2001); a *quantitative turn* (Martin and Sunley, 2001; Plummer and Sheppard, 2001; Rodriguez-Pose, 2001)[4], a *holistic turn* (Perrons, 2001); or an *about turn* and engagement with the mainstream economist ‘enemy’ (Yeung, 2001). This paper is a reaction to these two special issues focusing on one particular topic – the relationship between economic geography and economics.

Before we go any further, however, I should make a confession. Quite simply, I am a red in tooth and claw, full on, mainstream economist. I have a PhD in economics from the London School of Economics, that bastion of neoclassical economics. I have an MSc in Econometrics and Mathematical Economics from the same institution. My research papers, although empirical, draw on insights from formal mathematical models. The empirics themselves tend to be econometric number crunching exercises often based on large amounts of secondary data. I like working in a geography department but would feel equally at home in an economics one.

If my instincts on the current state of the relationship between ‘economic geography proper’ and ‘geographical economics’[5] is correct then I can make a couple of predictions on the basis of my two paragraphs so far. Because of paragraph one I am in danger of losing the 50% of my audience that consider themselves (geographical) economists – probably on the basis of the four words: cultural, relational, holistic and turn. Because of paragraph two I am in danger of losing the

[1] Journal of Economic Geography, Volume 3, No. 2, April 2003.

[2] The cultural turn suggested by Amin and Thrift (2000) would see economic geography disengaging with mainstream economics and forging a new alliance with, for example, evolutionary political economy, economic sociology, feminist economics, organisation theory and parts of environmental economics.

[3] Antipode, Volume 33, No. 2, April 2001.

[4] The quantitative turn could involve a greater or lesser role for mainstream economics. Martin and Sunley (2001) and Rodriguez-Pose (2001) argue for continued interaction with neo-classical economics. Plummer and Sheppard (2001) in contrast argue that “quantitative approaches should be liberated from their needless association with mainstream economics and its own vision of science, truth and evidence” (Plummer and Sheppard, 2001, 198).

[5] I follow Martin and Sunley (2001, 149) and use the labels of ‘economic geographers proper’ and ‘geographical economists’ to distinguish between geographers and economists, respectively. However, as will be become clear, I believe that the latter group are clearly doing economic geography even if their approach differs from that of the former group.

other 50% that consider themselves (economic) geographers proper – probably on the basis of just two words: mainstream economist.

Casual empiricism lends some support to these predictions. With regard to the first I simply note that, with the exception of this paper, not a single mainstream economist has so far contributed to the debate on the future of economic geography. Indeed, that debate has "unfortunately been dominated by discipline-political arguments, opinions and claims" (Bathelt and Glückler, 2003, 118). Further, "geographical economists" (Martin and Sunley, 2001, 149) have not even felt the need to respond to the direct criticisms of their approach made by, among others, Dymski (1996), Martin and Sunley (1996), Martin (1999a, 1999b), Sheppard (2001), Sunley (2001) and Berry (2002). With regard to the second prediction, note that the suggestion to move away completely from the "long shadow of economics" (Amin and Thrift, 2000, 8) is welcomed by the majority of those responding in *Antipode*. The controversial and unsubstantiated assertion that "most economists have been enlisted in the causes of the powerful" (Amin and Thrift, 2000, 8) also seems to be accepted by most of those authors.

In this paper I will argue that this indifference, on the one hand, and outright hostility, on the other, are a pity for two reasons. First, because geographical economists seek the answers to key questions that lie at the heart of economic geography. What are the causes and consequences of the fact that economic activity is unequally distributed across space? How often can empirical observations be explained by general rules? What locational specificities explain the exceptions to these rules? If economic geographers proper are no longer seeking answers to these questions, then I do not understand how what they are doing is economic geography. Assuming that this is not the case, then geographical economists and economic geographers proper are seeking answers to the same fundamental questions. That is, both groups are *doing* economic geography even if their *approaches* differ. This brings me to my second reason for lamenting the current state of the dialogue. I would argue that the time is right for some mutually beneficial exchanges of ideas between the geographical economists and economic geographers proper. To this end, the rest of this paper sets out to do two things. First, it offers a defence of geographical economics. In writing this defence, however, I wanted to try to draw out some lessons for economic geographers proper in terms of how they communicate their research agenda to economists. It turns out that the articles in the recent special issue of the *Journal of Economic Geography* provide an excellent context in which to do this. My second task is to try to spell out a more positive agenda for future interactions between the two groups.

2. In defence of geographical economics

In what follows, I will mainly focus on geographical economics (à la Krugman) rather than contributions in urban economics. This choice purely reflects the fact that most of the debate amongst geographers is a response to the former approach rather than the latter. Personally, I find many geographers' apparent willingness to disengage with urban economics far harder to understand than I do their annoyance with Krugman. After all, Krugman (and many of his followers) managed to effectively ignore several decades of work in economic geography and to focus on economic mechanisms that were considered 'old hat'. The same cannot be said of urban economics, as is clearly demonstrated by the papers of Audretsch and Feldman

(2003), Duranton and Puga (2003), Gabaix and Ioannides (2003) and Rosenthal and Strange (2003) for the forthcoming *Handbook of Urban and Regional Economics*.[6]

With this proviso in mind, I want to emphasise two things that geographical economics does well relative to economic geography proper. First, it develops economic geography models that are built up in an internally consistent way from micro economic foundations. This is true for both partial equilibrium and general equilibrium or 'macro' models. Despite assertions to the contrary, these models have provided new insights about, for example, the role of market structure and the nature of price competition in determining location. Second, geographical economics insists on rigorous empirical methodologies and a clear relationship between these methodologies and the underlying theory. Thus, in the words of Martin and Sunley (2001, 152), it aims to avoid "vague theory and thin empirics". As this quote makes clear, economic geographers proper are aware of this issue. However, I would argue that their reaction to it may not be the most optimal in attempting to generate a dialogue between the two approaches. I try to explain precisely why this might be in what follows. Despite these two clear advantages, the geographical economics approach is not without weaknesses. In particular, geographical economics is often insufficiently careful about what is a 'location' or 'place' and the fact that what is true at one spatial scale may not be true at another. There is also a tendency to privilege particular economic forces purely because they are more amenable to the theoretical and empirical tools used by mainstream economists. To the extent that geographical economics is not blind to these weaknesses there remain clear areas where there is scope for dialogue between the two approaches. I suggest two possible areas for beneficial interaction – one theoretical and one empirical.

2.1 Micro foundations

I want to start by laying out my understanding of what a relational theory of economic geography might look like. This sketch draws heavily on Bathelt and Glückler (2003)[7] the first paper in the recent special issue of the *Journal of Economic Geography*. In that paper, the authors sketch out a research design for a relational perspective. First, we need to acknowledge that "regions are not real actors" (Bathelt and Glückler, 2003, 121) so that "economic actors and their action and interaction should be at the core of a theoretical framework of economic geography and not space and spatial categories" (Bathelt and Glückler, 2003, 124). Second, once we move away from space as an entity, we observe that "economic actors themselves produce their own regional environments" (Bathelt and Glückler, 2003, 123). Third, firms are "not independent entities, but are closely interconnected in communication and adjustment processes with their suppliers, customers and institutions" (Bathelt and Glückler, 2003, 126). Fourth, in such an environment, we need to think about contingency rather than causality. That is, accept the idea that "one event does not *necessarily* cause another event. Therefore, identical preconditions for human action do not necessarily have the same consequences at any time and place" (Bathelt and Glückler, 2003, 127, italics in original). Putting all of this together, we get a theory of

[6] It is interesting to note that the reaction of urban economists to New Economic Geography has been almost the exact opposite of that of economic geographers proper. This is despite the fact that the early contributions in the New Economic Geography also ignored recent work that had been done in that field.

[7] This sketch of Bathelt and Glückler (2003) deliberately downplays some of the emphasis those authors put on institutional factors. This will help when I come to identifying the similarities with geographical economics later.

economic geography which is "contextual, path-dependent and contingent" (Bathelt and Glückler, 2003, 129).

The strangest thing happened to me when I was reading this paper. I started thinking that I might not be just a straightforward mainstream geographical economist. In fact I started to believe that I might, after all, be a relational economic geographer! Rather like the ugly duckling, I suddenly realised that perhaps I was not an economist duck at all, but instead a beautiful white geographer swan. I could suddenly picture myself walking tall amongst my geography department colleagues – a 'proper' economic geographer at last. Talking to other geographical economists it became clear that I was not alone in my reaction to this piece. From our viewpoint there are some pretty deep similarities between the "research design" advocated by relational economic geography and what Krugman (1991) and others have achieved in geographical economics. In fact I would argue that the micro foundations emphasised in Krugman-type new economic geography gives us a set of models that have *precisely* the characteristics called for by those advocating a relational approach. These similarities are so important that I think it is worth spelling them out in some detail.

Relative to much of the existing regional science literature Krugman (1991) started with individually optimising firms and workers, not regions, as actors. Economic interactions between firms and their consumers (demand linkages) were at the core of understanding divergent spatial outcomes. As transaction costs fell, relocation of these firms and workers could change regional economic environments, endogenously separating a-priori identical regions in to core and periphery. Later work by Venables (1996) extended the range of economic interactions to consider input-output relationships between firms, so that firms were now linked to both customers and suppliers. Interactions between firms could now lead to the emergence of specialised regional economic environments. The full range of models in this tradition, summarised in Fujita et al. (2001), Fujita and Thisse (2002) and Baldwin et al. (2003), show that often outcomes are path dependent and contingent. History matters and similar changes in economic parameters do not always necessarily lead to the same outcomes. Finally, outcomes can be sub optimal from both an efficiency (lock-in) and welfare perspective. Given these characteristics, I would argue that these formal mathematical models, written down by mainstream economists, do provide us with a theory of economic geography that is "relational" in nature. It is just that these models focus on different economic variables: Geographical economics concentrates on market transactions (traded interdependencies) whereas the relational approach emphasises non-market transactions (un-traded interdependencies) and the role of conventions and institutions.[8]

There are two common reactions to this observation. The first, is to claim that this sort of formal theorising based on traded interdependencies misses a lot that is important. As Perrons (2001, 209) so nicely puts it, "it is precisely the issues considered intangible by NEG I [Krugman] that form the basis of the substance of NEG II [Amin, Thrift, Storper and Scott[9]], where soft factors – that is the relational, social and contextual aspects of economic behaviour – are emphasised". The second

[8] These conventions and institutions play two roles. They may facilitate transactions outside of markets, but they also underpin the functioning of markets.

[9] I should note that, unlike Perrons, I would draw a clear distinction between the work of Storper and Scott and the cultural turn proposed by Amin and Thrift. The work of the former clearly draws on insights from economic theory and I am not sure that either Storper or Scott would advocate a disengagement from mainstream economics.

is to turn against the methodology and, in particular, to portray its reliance on individual optimising behaviour, equilibrium outcomes and mathematical modelling as a regressive step for economic geographers proper who have "long since abandoned location-theoretic and regional science models" (Martin, 1999b, 388).[10] As resolving the importance of the first reaction is essentially an empirical issue, I want to leave that aside for one moment and focus instead on the theoretical.

The following statement comes with a fairly significant health warning – reading it may confirm all your biases about geographical economists. Despite the danger, and in the interests of making some progress in the dialogue between geographers and economists, I think that I may as well state this as clearly as possible: Geographers will never get economists to give up their belief in formal modelling with its emphasis on individual optimising behaviour and equilibrium outcomes when the alternatives are the kind of theorising outlined in Bathelt and Glückler (2003) or Ettlinger (2003).

The reason for this is quite simple: This way of modelling allows one both to check the internal consistency of one's models and to see which assumptions are crucial in obtaining a particular result. Because I will have more to say on this second feature when I turn to empirical issues I focus here on the enforcement of internal consistency. This is particularly important when moving from partial to general equilibrium (i.e. from micro to macro behaviour). Time and again, economists have found that this is vitally important because partial equilibrium reasoning (looking at just a small part of any problem) often fails to provide the right reasoning in a general equilibrium context (when taking the economy as a whole).[11] It turns out that this particular argument is very important for the field of economic geography when it comes to understanding what falling transaction costs imply for the role of traded intermediates in determining the location of economic activity. For some economic geographers proper, the logic of proximity to reduce costs disappears as transaction costs fall, meaning that traded interdependencies can no longer explain the persistence of agglomerations.[12] However, in a whole variety of formal models this simple logic fails to hold. Instead, the key finding is one of 'putty-clay' economic geography. Thus, "the recent fall in transport costs seems to allow for a great deal of flexibility in where particular activities can locate, but once spatial differences have developed, they tend to become rigid" (Fujita and Thisse, 2002, 18). Falling transaction costs can generate new agglomeration patterns and help enforce existing ones – quite simply, they do not provide an *a-priori* reason for moving away from models based on traded intermediates towards models based on un-traded independencies.[13]

This insistence on being able to move from micro assumptions to macro outcomes is not just an obsession of economists. Storper (1997) when discussing models based on un-traded interdependencies notes that, in his opinion, "we do not yet know how to get from small processes of interaction and evolution to big regularities" (Storper, 1997, 81). Indeed, he argues that the theorising he provides is "a long way from rigorously attacking the relationship between micromodels and bigger regularities."

[10] As pointed out by one anonymous referee, this kind of criticism suggests that what some people dislike about geographical economics is the economics not the geography.

[11] The international trade literature provides a host of examples where this statement holds true.

[12] In passing, I note that some well known economists have also made similar sorts of assertions. For example, Glaeser (1998, p. 145) suggests that "If cities' only advantage was eliminating transport costs, then [falling transport costs imply] cities would indeed cease to exist"

[13] My argument here is that the observation of falling transport costs is *not* in itself sufficient to justify rejecting theoretical economic geography models based on transport costs on traded goods. A significant number of recent empirical papers suggest that such a rejection is not justified on empirical grounds either. See Head and Meyer (2003) for an extensive survey.

(Storper, 1997, 81). Reading through the special issue of the *Journal of Economic Geography* it is unclear to me that the proponents of the relational turn have made any significant progress on this issue.

2.2 Interaction - theory

Geographical economists find these arguments about the advantages of formal modelling pretty convincing. Economic geographers proper apparently do not. Does this mean that any hope for dialogue on theoretical issues between geographical economists and economic geographers proper is a non-starter? Surprisingly, in light of my comments above, I think that the answer to this question is 'not necessarily'. Hold on a minute, I hear the economic geographers proper cry. Didn't he just insist above that geographical economists will never abandon their formal models? If so, what possible scope can there be for dialogue? I think that the key issue here is for economic geographers proper to realise that economists will never abandon their tool box, but they are willing to change the nature of the jobs that they undertake with those tools. Two examples outline what I am talking about.

First, consider Ettlinger (2003), the second paper in the special issue of *The Journal of Economic Geography*. I will confess, the title alone - "Cultural economic geography and a relational microspace approach to trusts, rationalities, networks and change in collaborative workspaces" - is enough to stop most economists reading any further. In addition, once I started working through it, I started getting confused about what any of this had to do with economic geography. The "paper develops a relational, microspace framework to explain how social interaction (in and outside of workplaces) affects decision making, behaviour and performance in collaborative work" (Ettlinger, 2003, abstract). While I can see that these social networks may have a spatial dimension I do not see that this is necessary. As Ettlinger (2003, 167) notes "The anecdotes that I have offered and discussed entail face-to-face contact. […] Just as pertinent, however, […] are sources and practices of cohesion *across* space through, for example, imagined communities or virtuality." (italics in original). Abstracting from this paper's relationship to economic geography, as I understand it, Ettlinger (2003) makes two main points. First, decision making agents may be influenced by multiple spheres of life and social networks. This suggests that we can only understand actions with reference to multiple rationalities (governed by these differences) rather than appealing to a unidimensional rationality such as profitability. Second, when this is the case, to understand how these multiple rationalities play out we need to appeal to concepts of different types of "trust" that may have formed in these different spheres. Clearly Ettlinger (2003) feels that this sort of reasoning could never form part of a formal model based approach and calls instead for approaches grounded in cultural studies rather than economics.

Unfortunately, for Ettlinger (2003), but fortunately for the argument I am trying to make here, these sort of issues are the concern of a vibrant and growing research strand in economics concerned with social interactions.[14] This literature concerns itself with the formal mathematical modelling of two aspects of social interactions. The first strand considers the implications of social interactions in pre-determined groups (e.g. Akerlof, 1997; Brock and Durlauf, 1999, 2001a,b). The second strand considers what social interaction can tell us about group formation, particularly neighbourhood formation (e.g. Benabou, 1993, 1996; Durlauf, 1996a, 1996b and Hoff

[14] My discussion here draws extensively on the introduction to Brock and Durlauf (2003).

and Sen, 2000). The economists working in this field suggest that "In many respects, the new literature on social interactions addresses a famous criticism of economics made in Granovetter (1985, p55), 'Classical and neoclassical economics operates […] with an atomised and *under*socialised conception of human action […] The theoretical arguments disallow by hypothesis any impact of social structure and social relations'" Brock and Durlauf (2003, 2). They see "one of the appealing aspects of the new literature […] is that it has facilitated the introduction of sociological concepts and perspectives into economic modelling [showing how these] ideas may be formalized and extended using the formal rigour of economic theory" Brock and Durlauf (2003, 2). Interestingly, in a slightly different context, recent work by Ellickson et al. (1999) considers the general equilibrium of an economy where agents "can belong to *several* clubs and care about the characteristics of other members of the club […]. The central subtlety is in modelling club memberships and expressing the notion that membership choices are consistent across the population." (Ellickson at al, 1999, abstract, my italics). For me, two things stand out from this brief discussion. First, much of the difficulty in dealing with multiple "rationalities" (clubs) is in moving from micro foundations to macro outcomes. Again, I find it hard to see how this particular hurdle is going to be tackled if we head down the route proposed by Ellinger (2003). Second, the reaction of economists to the criticisms by Granovetter (1985) was *not* to ditch formal modelling, but to try to take account of those observations when developing new formal models.

Interestingly to me, it is precisely this reaction of economists that opens the way for economic geographers proper to influence the theoretical work of geographical economists – not in the way that they do their modelling, but in the economic content of those models. Most geographical economists are open to the idea that some sort of "technological externalities" should be added to formal models of economic geography. Indeed, chapters in Baldwin et al. (2003) already consider a model which includes both linkages and technological spillovers. In such an environment, ideas surrounding un-traded interdependencies and the development of localised relations and conventions as a way of facilitating these interactions offer insights that could take geographical economist's formal modelling down more interesting routes than simple black box technological spillovers.[15]

I would not claim that this interaction will be easy, but it is possible. The sceptics amongst you are referred to Leamer and Storper (2001) and Storper and Venables (2002) for proof that it can happen. (Yes, that's Michael Storper the *geographer*, Edward Leamer the *international trade economist* and Tony Venables the *geographical economist.*[16]) I would suggest that something similar could happen with respect to labour market considerations and other interdependencies more generally. Finally, I should point out that geographical economists are not the only ones who could benefit from this interaction. If economists tend to overemphasize the role of market transactions, it is clear that economic geographers proper are pretty sloppy about prices and the role of falling transport costs in determining location.

[15] This formal modelling to pull apart the black box is already occurring. See Duranton and Puga (2003) for a survey.

[16] Venables, of course, does trade too.

2.3 Empirics

To motivate a shift in focus from theory to empirics, I want to pick up on a comment by Ron Martin in his editorial for *Transactions of the Institute of British Geographers* where he states that "the 'new economic geography' can be heavily criticised, and *shown* to have considerable limitations compared to the approaches used by economic geographers proper" (Martin, 1999b, 389, my italics). This statement can be interpreted as talking about either methodology per se, or about the empirical evidence concerning the micro foundations of different theoretical models and I have seen both types of assertions made in the literature. As I have made pretty clear above, I can see that economic geographers proper have richer theories (in the sense of being more complex) but I am not convinced that they are necessarily better theories (in the sense of helping us to better understand the real world). When we turn to empirical work these theoretical differences play out as a trade-off between paucity (or clarity) and the overall predictive power of empirical models. For the sake of progress, I suggest that we agree to disagree on which approach is better. Instead, I want to turn to the empirical evidence concerning the micro foundations of models of economic geography. In particular, I want to focus on what we do and do not know about the empirical importance of traded versus un-traded interdependencies. It seems to me that this issue is key in *showing* that the empirical foundation for geographical economics is limited relative to that of economic geography proper.

Here, to my mind, there is an inherent contradiction in the criticism directed towards the new geographical economics. Critics are quite happy to state that "we need to convince economists […] that socio-institutional factors are central determinants of the development of the economic landscape, not just background 'noise'" (Martin, 1999b, 388). That "rich empirical research, to date, has illustrated that the quality and nature of ties [relations] are critical determinants for economic prosperity and that different forms of socio-economic coordination often lead to divergent levels of performance" (Boggs and Rantisi, 2003, 110). But at the same time, there is "concern over the thin empirics that seems to typify work in the 'new' economic geography [proper]" (Martin, 1999b, 154). "When empirical evidence is used, it is often limited to a series of case studies that are repeated almost ad nauseam and based on a limited amount of anecdotal information" (Rodriguez-Pose, 2001, 181). In deciding what to study economic geographers "have increasing difficulty in separating what is trivial from what determines the conditions of everyday life and well-being" (Martin, 2001, 156). It may not surprise you to find that I could not agree more with the criticism of much of the empirical work that is going on in economic geography proper. To my mind the last two papers in the special issue of the *Journal of Economic Geography* demonstrate these points perfectly.

Murphy (2003) studies industrial networks in Mwanza, Tanzania, based on eight months of fieldwork involving in-depth interviews with 41 managers. What do we learn from the analysis of this "rich empirical data" (Murphy, 2003, 182)? The paper draws three broad sets of conclusions relating to "credit-accessing, reputation-building and information-acquiring relations [that] encourage business people in Mwanza to interact socially" (Murphy, 2003, 182). What struck me as I started reading through the details on these three 'logics' was that I could not imagine a single local business environment, *anywhere*, where they would not hold true. Detailed consideration of just one area – credit – will hopefully demonstrate what I mean. This is not an arbitrary choice, but rather reflects the fact that Murphy (2003, 182) identifies "credit relations [as] the most commonly used and fundamentally useful business relations in Mwanza". We start by noting that "Agents in credit relations are viewed as givers, seekers and receivers" (Murphy, 2003, 184)[17]. Several factors influence agents' actions in the credit market.[18] First "a credit seeker or receiver should represent a reliable, accountable, and trustworthy person to the credit giver". This perception is influenced by "Friendliness, respectfulness, good appearance, habits, politeness, timeliness and being well spoken or a good communicator" (Murphy, 2003, 184). It is "especially important that credit seekers not seem pushy, boastful or proud" (Murphy, 2003, 184). What happens if a borrower defaults or as Murphy (2003, p 185) puts it "if a credit receiver fails to conform with the rules and expectations set by the credit giver"? Two things – they are unlikely to get credit from the same lender again and it can damage their reputation (credit rating?). This may mean that they go out of business if they need credit in the future but cannot get access to it. Finally, it turns out that repeated interaction helps deal with some of the asymmetric information and moral hazard issues in these credit relations – "credit is something earned through repeat business" (Murphy, 2003, 185).

Can I really be alone in thinking that (nearly) every formal or informal credit market works like this? I am not an expert in this field, so it would help me if someone could tell me whether any of these characteristics are likely to be unique to Mwanza? Murphy (2003) provides hardly any comparisons to existing literature. Papers by Fafchamps (1997) and Bagachwa (1997) quoted in a footnote suggest that there may be some similarities with Zimbabwe and Tanzania – but what are the differences? Interestingly, non-market institutions for credit and risk sharing are the focus of a large and expanding literature in development economics. These studies have tended to focus on the implications of risk for both individual behaviour and the evolution of institutions. Bardhan (1989), Alderman and Paxson (1992) and Besley (1995) all provide surveys and a wealth of references. Many of these approaches are inductive in nature, starting off with observations about real world institutions and then trying to build theoretical models[19] that capture these rules. Stiglitz (1974) provides an early example in his work on agricultural share cropping. While this work was distinctly non-geographical, more recent work has focused on particular institutions that have arisen in specific geographical areas. To take just one example, Udry (1994) studies informal credit arrangements between friends and families who live in four villages in northern Nigeria. His work is based on a survey involving repeated monthly interviews over a year long period with 200 families. His findings are particularly relevant to the current discussion: "The restriction of loan transactions to agents within a small social space allows the free flow of information between borrower and lender that is necessary to support state-contingent contracting and provides access to community-based mechanisms to monitor and enforce the [informal] contracts." (Udry, 1994, 522).

What do we learn from Murphy (2003) that we did not know already from this existing literature? These may appear like minor quibbles – but there are two fundamental issues here. First, a geographical economist coming to this sort of work gets no idea of the key contributions. What exactly are the refutable predictions that come from applying a relational approach to the study of these credit relations? Are there actually any hypotheses to be tested here? If so what do we actually learn about the empirical validity of these predictions? From a methodological perspective, I get

[17] That's lenders and borrowers with a distinction between those actually in credit relationships and those that would like to be in credit relationships.
[18] Presumably we are focusing on the informal credit market, although, nearly all these statements would hold true for formal credit markets.
[19] Most such models are based on ideas from information economics and contract theory.

no feeling for whether the piece is rigorous relative to the current agreed empirical standards and practices; I have no idea whether the piece provides any sort of methodological improvements over existing literature; whether the comments that are reported are representative of the sample of managers interviewed and if that sample, in turn, is representative of the population (no summary information is provided); whether any of the results might be significant (either statistically or in a broader sense). Finally, given all these problems, I have no idea whether we could expect another economic geographer proper faced with the same data, asking the same questions and using the same methods to come up with anything approaching the same answers. Economic geographers proper find that "in practice, too much intensive and ethnography-based work in economic geography is sloppy involving too few (and highly selective interviews), sloppy methodology, and little or no wider empirical contextualisation" (Martin and Sunley, 2001, 155). Imagine, then, what it is like for a geographical economist who comes to this empirical literature trying to learn something about the empirical role that relations/conventions may play in determining location. This brings me to the second fundamental issue. On the basis of existing empirical evidence I do not think it is possible to conclude that conventions/relations are central to our understanding of economic geography and that traded interdependencies only play a limited role. I suspect that this assertion brings me in to conflict with a wider range of economic geographers proper so I want to spend some time outlining why I feel this way.

Let me start with the simplest case by carrying on with my consideration of Murphy (2003). I see nothing in his analysis to convince me that these relations he identifies are fundamental *causes* shaping the economic geography of Mwanza. Outcomes, possibly, but not causes. Some good candidates for fundamental causes get short shrift – a few sentences in which we discover that Mwanza is "an important regional node for trade and [a] rapidly growing urban centre [which is] being transformed into [a] large scale urban centre through increased foreign investment, improved trade links, and rapid in-migration from rural areas" (Murphy, 2003, 174). And yet, despite this, the study claims to contribute to "our understanding of the social dynamics of innovation and industrialization in East Africa". How? I see no evidence mapping these social relations to innovation and location outcomes. No evidence that these things matter at all and no evidence that these social relations matter more than bog-standard economic ones. Of course, answering these questions is difficult because of a fundamental empirical problem of observational equivalence. When we observe economic geography outcomes how can we be certain we have identified the forces that are driving these processes? In many cases, we may not be able to answer this question in an either/or manner and it will come down to assessing which factors appear to have more explanatory power. To my mind, we are a long way from having solved this observational equivalence problem and yet economic geographers proper seem to have reached the conclusion that we *know* that conventions and relations are central and more important than explanations relying on traded interdependencies. I just do not see how case studies of a limited number of regions allow us to reach this general conclusion, even if they do tell us something about those particular regions. I want to return to this issue, but before that I want to show that this failure to grapple properly with the issue of observational equivalence is not just limited to the empirical literature on traded and un-traded interdependencies.

To make the point, I briefly consider Sturgeon (2003) the fourth and final paper in the special edition of the *Journal of Economic Geography*. Sturgeon uses the example of the American electronics industry and Silicon Valley (again!) to argue that an idea he calls ‘modular production networks’ is a better way of understanding that industries dynamics than ‘relational production networks’. I have no desire to come down in favour of one or the other of these approaches. Instead, I just simply note that nowhere in Sturgeon (2003) could I find some simple empirical predictions that break the observational equivalence between these two theories or a suggestion of how these empirical predictions might be tested in a wider industrial context. The conclusion does go in this direction when it states that “relational economic geography assumes that increasingly idiosyncratic interactions will cause spatial clusters to become more inward-looking over time just as institutional economics assumes that increasing asset specificity will drive firms to grow larger over time through vertical integration. The problem is that neither of these outcomes are fully captured by empirical observation.” (Sturgeon, 2003, 220). But this does not really leave me any the wiser as to what we should be looking for empirically, particularly as I am not sure that this characterisation of the predictions of relational or institutional geography would be shared by everyone.

2.4 Interaction - empirics

When I joined the Geography and Environment Department at LSE, the first book on economic geography proper that I read was Michael Storper’s excellent book outlining a new relational approach to economic geography. Storper (1997) although more careful than some, also seemed willing on occasions to make the same broad statements about the empirical role of traded intermediates. Thus care at one point “it seems unlikely that all clusters of intermediate-output producers reduce to market size” (Storper, 1997, 13) but just one page later we learn that “the localisation of traded interdependencies, is inadequate to the task of explaining the link between flexible production and the resurgence of regional economies in contemporary capitalism” (Storper, 1997, 14). It should be clear by now, that I do not think we are at a point where we can draw such a broad conclusion. However, I am willing to accept that in some instances, in some locations there does seem to be a strong relational/conventional element that differentiates that location from other apparently similar locations. I am not yet convinced that these elements explain how these territories form, or that they are *the* cause of superior local economic performance, because I do not accept that economic geographers proper have truly dealt with the issue of observational equivalence between these and competing explanations. But the detailed case studies on a *limited* number of locations have convinced me that *sometimes* these conventions/relations exist, that their specific form is fairly unique to that location and that, as a result, they may help us understand the economic evolution of that location. But given current empirical evidence these locations remain the *exception* not the *rule*. As Storper (1997, 48, italics in original) clearly states “*regional worlds of production* can emerge out of the *technological and organizational worlds that make regions*. But this occurs only in some cases; in many others, the regional economy remains, for the most part, a mere locational repository of organisational and technological worlds”.

I think that this distinction between the exceptions and the rules helps clarify another area in which interaction between the two approaches could be mutually beneficial. As I have already made clear, I think economic geographers proper and geographical economists should all be trying to answer the same three key questions about economic geography. What are the causes and consequences of the fact that economic activity is unequally distributed across space? How often can our empirical observations be explained by general rules? What locational specificities explain the exceptions to these rules? Geographical economists think that it is most important to explain the rules. Every context and every outcome is going to have some idiosyncratic aspects. But treating each situation as something unique and each idiosyncrasy as something crucial teaches us nothing. It is important to separate what factors are crucial in determining particular outcomes from those that are not. Because formal models allow us to see what factors are crucial and because geographical economists think that general rules are important, empirical work by geographical economists is making much better progress on the rules than economic geography proper.

I do not think that this has to be so. Good, careful case studies by economic geographers proper could be contributing to discussion on both the rules and the exceptions to the rules. As Rosenthal and Strange (2003) put it "even the most refined data and most sophisticated econometric techniques will not be able to address all the idiosyncratic conditions that contribute to agglomeration. Thus, there is much to be learned about the nature of agglomeration from case studies". That this is not happening at the moment, I believe, comes down to the failures that I discuss in depth above. Martin (1999, 388) can assert that geographers are "not opposed to generalisations or general theory" but that is not the impression that one gets when one turns to the current literature. Economic geographers proper need to remember that good empirical work (whether quantitative or qualitative) needs to identify the core, ignore the trivial and deal with issues of refutability, causality and observational equivalence.

There are lessons here for geographical economists too. As I mentioned earlier, geographical economics has tended to be rather imprecise about what is a location or place. That is, it has tended to suggest that similar rules may apply at all spatial scales. Economic geography proper, with its focus on idiosyncrasies, is much more careful about defining space. This should remind geographical economists of the dangers of ecological fallacy. That is, in the hunt for general rules or tendencies, it is important to remember that what is true at a given spatial scale might not be true at another.

3. Conclusions

Given the path dependent nature of academic disciplines it seems unlikely that we will see technological convergence between the economic geographers and the geographical economists anytime soon. This paper has identified two areas in which I believe the technology of the latter is more robust than the former. First, formal models help identify which assumptions are crucial in obtaining a particular result and enforce internal consistency when moving from micro to macro behaviour. Second, empirical work tends to be more rigorous, with much greater emphasis on identifying and testing refutable predictions from theory and on dealing with issues of observational equivalence. But any technology can be improved and this paper has sought to identify ways in which geographical economists and economic geographers proper could learn from each other. On the theoretical side, I believe that both groups could benefit from recognising that they tend to privilege particular types of economic interactions. Lessons for empirical work are harder to sum up in one sentence but I believe that both groups need to be more careful in the way that they take their theories to real world data.

If this interaction is to occur, it will depend on one group overcoming its indifference and the other overcoming its thinly disguised hostility. Whether such a programme is mutually beneficial will depend on the extent to which the two approaches are complements rather than substitutes. Of course, I would welcome the help either way.

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