Papers in Evolutionary Economic Geography

16.22

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Robert Hassink, Huiwen Gong, Fabian Faller

Utrecht University
Urban & Regional research centre Utrecht

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Robert Hassink, Huiwen Gong, Fabian Faller

Kiel University, Department of Geography, Hermann-Rodewald-Str. 9, 24098 Kiel, Germany, hassink@geographie.uni-kiel.de

Submitted to Papers in Evolutionary Economic Geography

Abstract

Since the launch of new economic geography by Paul Krugman there have been intensive debates between geographical economists and economic geographers both about the ways they differ from each other as well as about potential complementarities. Overman's (2004) provocative article, titled "can we learning anything from economic geography proper?" has been not very helpful in developing the latter. By responding to his core critiques we provide a much more positive answer to his question, do justice to economic geography and show more complementarities between geographical economics and economic geography.

Key words: economic geography, geographical economics, debate, paradigms

1. Introduction

Since the launch of new economic geography by Krugman (1991), or what later has been called geographical economics, there have been intensive debates between geographical economists and economic geographers about the ways they differ from each other (Martin, 2011; Garretsen and Martin, 2010; Hassink and Gong, 2016; Sunley, 2012). Although both might "do" economic geography, their approaches and methods strongly vary because of differing ontology and epistemology (explaining vs. understanding), which are in turn related to differing disciplinary origins (economics and human geography). As Martin (2011, 54, 55) wrote about geographical economics: "… the adherence of the use of formal mathematical deductive models to theorize the economic landscape has remained its foundational and distinctive feature. It is this issue of formal models that is at the heat of the tension between PEG [proper economic geography] and NEG [new economic geography]".

However, there have also been an increasing attempts to cooperate (Sjöberg and Sjöholm, 2002; Garretsen and Martin, 2010; Brakman et al., 2009) exemplified by the establishment of the Journal of Economic Geography, a publication platform for scholars of both sub-disciplines. Others have tried to quantify the relationship between both groups by looking at publication data and cross-referencing (Brakman et al., 2011; Sternberg, 2015). Despite intensive debates, clearly more economic geographers have reacted to geographical economics than the other way around. Henry Overman's (2004) highly provocative piece titled "can we learn anything from economic geography proper?" is one of the few papers written by geographical economists directly addressing economic geography. In his article he is not only very critical about economic geography, he also sees little room for cooperation. Moreover, he does not satisfactorily tackle the posed question in the paper's title but rather addresses why geographical economics is more robust than economic geography (due to their internally consistent models and their use of rigorous empirical methods that are in clear relation with the underlying theory). In our view, this is mainly due to his reductionist treatment analyzing just one paradigm (relational economic geography) and just a few articles in one special issue, instead of a proper scrutiny of economic geography. But we give credit to Overman for his appraisal of economic geography's strength to thoroughly consider space, place and scales - even though without elaborating on this any further - and that he saw this as crucial perspective geographical economists could learn from. We will deal with this issue in more detail throughout the paper, and in particular in Section 2, and will give a summarization in the conclusion.

Surprisingly, Overman's article, despite its provocative character, has hardly been cited by economic geographers and no systematic and comprehensive response by economic geographers has been published so far. It is high time to come up with such a response! Economic geography has namely moved on and the narrow view on economic geography by Overman was not only wrong back then, it is peculiar if we look at it now, as economic geography is much more than just relational economic geography.

Therefore, the first aim of this paper is to take issue with recent misconceptions of economic geography in the vein of Overman's provocative piece. This is necessary in order to be able to give a fair answer to the question: can we learn anything from economic geography? We acknowledge that some of the critiques by Overman (2004) were situated a decade ago, but in our view, many were already fundamentally wrong at that time, and some others need to be revisited and reconsidered given the emergence of new theories in economic geography and given new insights in the relationship between economists and geographers.

Secondly, we aim at working out potential complementarities between the two sub-disciplines, something Overman (2004) also aimed for but unfortunately failed to deliver, partly because of his selective and hence non-representative choice of cases of economic geographers' theories and empirical work, but partly also because of his too harsh and unfair critique of economic geography. On what basis can a positive agenda been built if one discipline is harshly criticized? Others, such as Garretsen and Martin (2010) or Rodrìguez-Pose (2011), have written on this, but there is still much room for progress, as these first attempts have been looking at economic geography in a relatively undifferentiated way and have not come up with concrete conceptual and methodological complementarities.

The structure of the paper is based on three main issues we have with Overman (2004). First, as stated above, a far too narrow view on economic geography hindered him to give a fair answer to his

question. Our contribution will focus on a broader set of paradigms, theories and questions in economic geography in Section 2. Secondly, we disagree with his critique of the qualitative case-study research method of many economic geographers and will deal with this issue in detail in Section 3. Thirdly, we disagree with the impression Overman (2004) gave that geographical economics, thanks to its robust methods, is much better capable of giving answers to real-world problems and hence deliver sounder policy recommendations than economic geography in Section 4. Finally, we will draw conclusions and will, on the basis of those conclusions, formulate several complementarities between geographical economics and economic geography in Section 5, also illustrating why economic geography's conception of space and place is fostering our understanding of economic development in theory and practice.

2. Economic geography is much more than what Overman tried to make us believe

Although Overman (2004) stated that there have been several turns in economic geography (relational, cultural, and quantitative turns among others) in the beginning of his article, he then just focuses on the relational turn in the remainder on the basis of one particular special issue published in the Journal of Economic Geography on relational economic geography (Vol. 3, No. 2, April 2003). He uses a selection of articles published in that special issue to generalize about economic geography as a whole. However, this is highly problematic, since it strongly reduces the opportunities for exchange and complementarity between geographical economics and economic geography. Relational economic geography was already at that time not the only or main paradigm in economic geography. Moreover, since the publication of his article, new influential paradigms have been launched. In fact, over the last fifteen years, economic geography is increasingly fragmenting into different research themes (Aoyama et al., 2011), educational backgrounds of researchers (e.g. human and physical geography as well as economy, business administration, political sciences or sociology), and theories and paradigms. Therefore, a reassessment of Overman's critique is necessary.

2.1 Paradigms of economic geography

Currently, the following main paradigms of economic geography can be distinguished: Evolutionary economic geography, relational economic geography, institutional economic geography and geographical political economy. Global production networks can be considered as an additional theoretical approach. It goes beyond the scope of this paper to deal with all the paradigms in much detail, but we will provide short sketches in the following in order to enable a fruitful exchange in the remainder of this paper (for comprehensive overviews see Hassink et al., 2014 or Barnes and Christophers, 2017b; for more detailed accounts of individual paradigms, see Boschma and Frenken, 2016; Bathelt and Glückler, 2011; Sheppard, 2011; Gertler, 2010; Yeung and Coe, 2015).

Evolutionary economic geography, which is growing fast, attempts to overcome differences between economic geography and geographical economics (see Boschma and Frenken, 2006). It deals with "the processes by which the economic landscape — the spatial organization of economic production, distribution and consumption— is transformed over time" (Boschma and Martin, 2007, 539). Important explanatory notions used in this paradigm include path dependence, lock-ins, related variety and unrelated variety. *Relational economic geography* "focuses on a relational understanding of economic action which is analysed in spatial perspective" (Bathelt and Glückler, 2011: 6). Relational here means to put emphasis on actor networks and interrelations, power, social agency, socio-cultural embeddedness of actors in multiple networks, and the interrelatedness between scales at individual level, when explaining the success of firms and regions, rather than on firm-centred organisational routines. The somewhat older paradigm of *institutional economic geography* focuses on formal and informal institutions at several spatial scales (Martin, 2000). Gertler (2010) put a strong emphasis on geographical variation, namely through a better understanding of how formal and informal institutions at different scales interact to produce a specific outcome. *Geographical political economy* is a term that can include a variety of approaches, as illustrated by Sheppard (2011) and Jones (2015) who use it as an umbrella for several trends within economic geography. Nonetheless, in the definition of Pike et al. (2009) and Martin and Sunley (2015) geographical political economy is mainly concerned with the relationships between the state, labour and capital and the inherent tendency of capitalism to generate uneven spatial development. *Global production networks* has recently been upgraded from an analytical framework, working with the conceptual categories of value, embeddedness, power and strategic coupling, toward a dynamic theory (Yeung and Coe, 2015). The latter aims to "explain why and how three competitive dynamics - optimizing cost-capability ratios, sustaining market development, and working with financial discipline - interact with firms and nonfirm actors … to produce … different actor-specific strategies for organizing global production networks …" (Yeung and Coe, 2015, 32). So as interim conclusion we can state that economic geography is a diverse discipline with many more conceptual facets than Overman tried to make us believe.

Interestingly, most of these paradigms are highly influenced by economics, albeit other branches than geographical economics. They are in fact more influenced by what is also called heterodox economics, a fuzzy and heterogeneous group of approaches that only shares a critical stance to mainstream economics. It includes Analytical Marxism, Evolutionary Economics, Feminist Economics, the French Regulation School, Institutional Economics, Marxian Economics, Neo-Ricardian Economics, the Performativity Approach, and the Polanyian Approach (Barnes and Christophers, 2017a). Peck (2015) goes even so far as seeing economic geography itself as a branch of heterodox economics.

2.2 Space, place and scales in economic geography

As noticed by Overman (2004), the notions of space, place and scales remain the key concern and strength of economic geography. However, he didn't elaborate on why this is the advantage of economic geography. In fact, the fundamental question of economic geography on how to explain the riddle of uneven spatial development necessarily entails discussions of these notions (Garretsen

and Martin, 2010). Geographers have three distinctive conceptions of space - that is, absolute space, relative space and relational space (for an elaboration of the three categories, see Harvey, 1973, 2006). For economic geographers, "space is neither absolute, relative or relational in itself" (Harvey, 2006, 125), but depends largely on the nature of the phenomena under investigation. While this is true, Harvey argues that absolute spaces may have little meaning in functioning economic terms. "If the aim is to understand uneven regional economic development, then relative and relational conceptions of space (and space-time) are necessary" (Garretsen and Martin, 2010, 143). Indeed, a step-by-step shift has happened in economic geography in considering space from 'absolute' to 'relative', then 'relational' conditions (Jones, 2009). Further, as claimed by Harvey (2006), the issue of geographical scale is also very important for economic geography as different economic processes may operate at different scales in the relative and relational spaces.

Although currently almost all paradigms of economic geography share both relative and relational perceptions of space, as well as a critical stance towards the use of absolute or neutral space or space as a container, they also have their own slightly varying conceptualizations of space, place and scales (Garretsen and Martin, 2010, 141). Evolutionary economic geography has a tendency to favor upward spatial causations because of emphasizing the role of routines of firms as engine of change and because of downplaying the role of nation states as actors in the economic landscape. Geographical political economy, on the other hand, tends to stress downward spatial causations because of the emphasis on the role of the state and formal institutions. Relational economic geography has strengths in dealing with spatial scales in networks with a more open view on the level of spatiality. Institutional economic geography also stresses the importance of different scales, albeit from an institutional perspective, and how they interact to produce a specific outcome in regional economies. The older work on global production networks had strengths in conceptualising territorial embeddedness with regard to strategic coupling processes between regional institutions and assets and transnational corporations steering global networks (Coe et al., 2004). The newly developed dynamic theory of global production networks, however, leaves much open when it comes to the conceptualization of space (Yeung and Coe, 2015).

To sum up the elaborations on space, place and scales, for economic geography, no matter which paradigm you take as a starting point, space is much more than just something that creates transport costs. It is something that both emerges from and acts as one prerequisite of economic activity. And scales are more than just analytical levels, but also considered as "socially constructed, fluid and contingent" (Marston 2000, 204). Moreover, in contrast to geographical economics, economic geography avoids crude generalizations regardless of spatial scales, as phenomena might be valid at one scale but not at another.

2.3 Research questions in economic geography

Closely related to the different theories and paradigms are the questions dealt with in economic geography. Again, Overman (2004) addressed this issue in an overly simplified way. According to him, both geographical economists and economic geographers focus on answering the same three questions, namely: 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? Overman (2004, 502-503) even goes so far as to claim that "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". In a similar vein Sunley (2012, 584) argues that "both PEG and NEG share similar fundamental research questions". Only scholars who would try to find answers to these questions, no matter whether they are geographical economists or economic geographers, would be doing economic geography.

But is this really true? First, even if these questions were the only valid ones, there would for sure be differences in emphasis between geographical economics and economic geography. The former might rather stress question 2 and economic geographers question 3. In a broader epistemological view, however, there are clear differences, as particularly pointed out by Krugman (2011). Economists are in general interested in "what if", or "what can we do" questions, whereas economic geographers want to know: "how did we get there"? Moreover, given the plethora of theories and paradigms with different academic roots in different parts of heterodox economics presented above, it is increasingly hard to reduce the research questions of economic geography to the three questions identified by Overman (2004). In other words, within economic geography the questions depend very much on the paradigm. A typical question for evolutionary economic geography is for instance: why do some regional economies manage to renew themselves or to lock themselves out, whereas others are more locked in decline (Martin and Sunley, 2006)? Geographical political economy, on the other hand, would ask: what is the impact of power asymmetries between transnational corporations and local firms, capital-labour relations and the state regularities on the evolution of territorial disparities?

Overall, it should have become clear that economic geography is much more pluralistic than Overman (2004) tried to make us believe. In fact, it consists of several paradigms, each with its own research questions and slightly different conceptualizations of space, place and scales. Economic geography, although a relatively small sub-discipline, is much richer concerning theories, paradigms, questions and topics than what Overman (2004) presented in his article. To be fair, some of this richness emerged after 2004, but already by the early 2000s economic geography encompassed a wider range of academic approaches than just relational economic geography.

3. What should be wrong with the empirical work going on in economic geography?

Overman (2004) also criticized the methodologies used by economic geographers (c.f. Rodriguez-Pose, 2001; Martin and Sunley, 2001). He mainly based his empirical critique on the paper by Murphy (2003) about industrial networks in Mwanza, Tanzania, and concluded that "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" (Overman, 2004, 511). While various research methods (both qualitative and quantitative) have been used in economic geography, the case-study-based empirical research receives most of this sort of criticism. Still it is one of the most significant methods in economic geography, and therefore, we see it as a necessity to justify the role of case studies in economic geography. This builds on Flyvbjerg's (2006) work on 'five misunderstandings about case study research', namely: (1) general, theoretical (context-independent) knowledge is

more valuable than concrete, practical (context-dependent) knowledge; (2) one cannot generalize from a single case, therefore, the single-case study cannot contribute to scientific development; (3) the case study is most useful for generating hypotheses, whereas other methods are more suitable for hypotheses testing and theory building; (4) the case study contains a bias toward verification, that is, it tends to confirm the researcher's preconceived notions; (5) it is often difficult to summarize and develop general propositions and theories on the basis of specific case studies.

To begin with, Overman actually did a "case-study" himself: criticizing economic geography's research methods by focusing only on one or two papers. In the latter part of his paper, Overman (2004, 512) did admitted that good case studies are helpful for understanding economic-geographical phenomena: "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."

Furthermore, focusing the selection of cases and potentials for deriving analytical knowledge, he claimed that "given current empirical evidence these locations remain the exception not the rule [… but…] geographical economists think that it is most important to explain the rules" (Overman, 2004, 512). He then continued criticizing economic geographers for their focus on exceptions by arguing that "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"… "Economic geographers proper need to remember that good empirical work … needs to identify the core, ignore the trivial and deal with issues of refutability, causality and observational equivalence" (Overman, 2004, 512-513). This adheres to the conventional misunderstanding that context-independent knowledge is more valuable than concrete, context-dependent knowledge. But unlike what Overman said, empirical work in economic geography not only produces exceptions, but also generates rules. Admittedly, some case studies contemporarily in economic geography are problematic (e. g., superficial, sloppy, anecdotal, trivial), but there are still plenty of high-quality case studies which contribute to the improvement of theories as well as people's understandings of economic-geographical phenomena. For example, Grabher's (1993) work on the lock-in of regional development in the Ruhr area contributes to our understanding of crucial concepts such as path dependence and lock-ins in economic geography; Saxenian's (1996) work on the regional advantages of Silicon Valley and Route 128 Boston is highly influential for theorizing on cluster evolution and industrial culture; based on data in the Netherlands, Frenken et al.'s (2007) research on related/unrelated variety and regional economic growth contributes to the knowledge of related varieties, as well as the development of the evolutionary economic geography paradigm. It is these good, careful case studies that contribute to the knowledge and theoretical development of economic geography.

Secondly, although general, context-independent knowledge is important, we see much value in concrete, context-dependent knowledge, as well. According to George and Bennett (2004, 4-5), much of what we know about the empirical world has been produced by case study research, and many of the most treasured classics in each discipline are case studies. This is particularly true for economic geography. Economic geographers have long recognized the relevance of case studies (Tokatli, 2015). The appropriateness of this overall acceptance is clear given the highly influential examples of many good case studies in economic geography (e.g., Grabher, 1993; Saxenian, 1996; Frenken et al., 2007). Boschma et al. (2014) manifested that case study research is extremely helpful

for exploring complex issues. Kuhn (1987) even values the significance of case studies so much so that he, as noted by Flyvbjerg (2006, 242), claimed "that a discipline without a large number of thoroughly executed case studies is a discipline without systematic production of exemplars, and that a discipline without exemplars is an ineffective one". Besides the examples mentioned above which contribute to the understanding of general phenomena in economic geography, there is a large body of highly contextual, locational-dependent empirical case studies that have been conducted by economic geographers. While these case studies might not add to the knowledge of a global-range economic geographical phenomenon, they are useful for the understanding of national, regional, and even urban and rural levels of economic activities. In addition, as Overman (2004) also agrees, one of the main tasks of economic geography is to explore the causes and consequences of unequally distributed economic activities in space. Consequently, we do not see such concrete, and context-dependent empirical work problematic (neither do we see economic geographer's seeking for exceptionism as problematic), but we believe that well-prepared case studies based on specific conditions can contribute to the improvement of general theories as illustrated above.

Another misconception that Overman (2004) has towards case studies in economic geography is that he does not see how case studies of a limited number of regions could allow us to reach general conclusions. Since the role of high quality cases in generalizing theories are self-illuminating as we have demonstrated shortly before, we are not going in detail in that direction here. Instead, we want to introduce another way through which case studies are ideal for generalizing, that is, using the type of test which is called "falsification". Tokatli's (2015) work is actually one of the best examples of such test: instead of attaching to the traditional model of scientific production—the hypothetico-deductive model, she testified the previous proposition that Zara is a 'home-sewn exception to globalization' by re-scrutinizing Indetex's corporate reports from 1998 to 2012 and found that the conventional wisdom was wrong. Such falsification (although it is based on a single case) is one of the most rigorous and efficient ways one can use to test scientific propositions: if just one observation does not fit with the proposition, it is considered not valid generally and must therefore be either revised or rejected.

A last issue we have with Overman's critique is his idea that generalizing and replicability are the only ways to work as geographical economists. First, as illuminated by Flyvbjerg (2006, 226-227), formal generalization is only one of many ways by which people gain knowledge and "is considerably overrated as the main source of scientific progress." Any forms of model that contribute to the gaining of knowledge should be seen as scientific. Therefore, "a purely descriptive, phenomenological case study without any attempt to generalize can certainly be of value and often helped cut a path toward scientific innovation" (Flyvbjerg, 2006, 227). And secondly, undoubtedly, in natural sciences, replicating previous experiments can be a good method to test whether some experiments or theoretical propositions are rigorous and evidentially strong. But in social sciences in general and economic geography, in particular, the number of cases that can be revisited is small, given that (a) the context required for replicating other's work is highly specific, and (b) the empirical work worth to be revisited should be particularly good and careful (Yin, 2009). What we stress here is not that it is worthless trying to replicate some good cases, as has been done for instance by Mossig and Schieber (2014), but rather that it is not that easy to replicate other's work as the conditions for such empirical work vary from place to place, and time to time. And it is particularly this, what is economic geography's key strength: understanding economic processes with a rigid account of their spatial and temporal dimensions.

4. Why economic geography has more to offer to solve real world problems

Recently, geographical economists have been increasingly active in gearing their research towards policy-making. In Overman's (2004) and Krugman's (2011) work one even gets the impression that only geographical economists can solve real world problems due to their robust research methods (see also Martin and Sunley, 2011). They are arguably better at answering the "what if" and "what do we do" questions policy-makers want to have answers to (Krugman, 2011). Compared to geographical economics, economic geography in fact "invariably plays second fiddle when it comes to major policy-making bodies " (Martin and Sunley, 2011, 365), which is no wonder, being a much smaller discipline with hence much fewer graduates working as policy-makers. The most prominent example of the influence of geographical economics on regional policy-making is the World Bank Report "Reshaping Economic Geography" (World Bank, 2009). In this report the importance of agglomeration economies in general for regional economic development has been emphasized.

Related to the critiques presented in Section 2 and 3, the policy recommendations derived from geographical economics have been criticized by economic geographers (Martin and Sunley, 2011; McCann and Rodríguez-Pose, 2011) for two main reasons.

First, the use of models is seen with caution by Martin and Sunley (2011, 366): "… the usefulness, the plausibility, of 'what if' policy analyses using NEG models turns on the credibility and transparency of those models in the first place, and these are often dubious". Due to strict assumptions and the reductionist treatment of space and scales, the models applied would have little explanatory power in the real world (Martin and Sunley, 2011).

Secondly, since the policy recommendations are based on generalizable, abstract models not accounting for contextual, geographical differences, plausible critique has been raised regarding one-size-fits all approaches to regional policy. According to Rodríguez-Pose (2011, 352), "for geographers … *space is relative and variable and this makes context king* [italics added]. The multiplicity of interactions occurring at diverse geographical scales and the variegated spatial forms they generate mean that … 'one-size-fits-all' approaches are anathema …" This neutral view on space by geographical economics consequently would lead to questionable regional policy recommendations.

These contrasting views originate from differing conceptualizations of *space* in geographical economics and economic geography, which have significant implications for policy recommendations. According to Rodríguez-Pose (2011, 352) the two different views of geographical economics and economic geography have led to "radically different policy solutions": place-neutral (or spatially-blind) vs. place-based approaches. Economic geographers are in favor of the latter one (Barca et al., 2012) - like the OECD (2009a, 2009b). The key strengths of place-based approaches to regional policy are that they

- take real places with their unique institutional context serious;
- aim at providing tailor-made policy solutions and
- can be regarded as the opposite of a one-size-fits-all approach to regional policy.

Again, we argue that economic geographers have much more to offer to solve real world problems than geographical economists – particularly Overman (2004) – try to make us believe. It is the increasingly popular place-based approach to regional policy, which gives economic geographers opportunities to monitor, evaluate and also theorize about place-based policy approaches. They are fit to do this kind of job, as they have competence concerning space, place and scales, case-study research, qualitative comparisons, and also soft-skills emerging from the wide-ranging backgrounds discussed above (e.g. language ability). In more general terms, economic geography puts emphasize "… on the observation of real-world phenomena and […] accordingly constructs more plausible accounts of those phenomena. It therefore can provide useful *ex post* assessments and evaluations of policy impacts in specific local contexts, which is a useful role" (Martin and Sunley, 2011, 367). It has also qualities to answer how-did-we-get-here-questions, asking for a hermeneutic approach.

Finally, when actually looking at ongoing policy debates, we see an increasing interest for economic geography's way of understanding development. The most recent regional policy concept in Europe, smart specialization (Foray, 2015), even puts the place-based approach as one of four key priority setting rationales next to related variety, revealed competitive advantage and entrepreneurial discovery (Grillitsch 2016, Morgan, 2016). And also in academia, many economic geographers have recently been involved in doing qualitative research on this new strategy (see for example the recent special issue in European Planning Studies: Vol. 24, Issue 8, 2016). We regard this as an example of the role economic geographers can play in policy-advising in the near future.

5. Conclusions and complementarities

In this paper we have taken issue with the three main points Overman (2004) has made in his provocative article: (1) economic geography can be treated as one group (relational economic geography) which focuses on the same questions as geographical economists; (2) qualitative case-study research cannot be taken seriously; and (3) only geographical economists can solve real world problems. We have done this, and this was the first motivation to write this paper, in order to give a fairer answer to Overman's (2004) question: can we learn anything from economic geography proper? Our answer to his question is: yes, we can! A second motivation and aim of this paper was to come up with potential complementarities between geographical economics and economic geography; in the following we will draw our conclusions concerning the three main points and in parallel address complementarities we see between the two disciplines.

First, as we have shown in this paper, there are many different paradigms in economic geography and economic geography, as a whole, is more than what has been proclaimed by Overman (2004). While he notes that one of economic geography's strengths lies in thoroughly considering space, place and scales, the different paradigms of economic geography understand these terms in slightly different ways. However, what modern economic geographers share in conceptualizing space and places is their critical stance towards the use of absolute or neutral space or space as a container, and their preference for working with relative and relational space. In order to give a fair answer to the question "can we learn anything from economic geography proper", this contribution has focused on a broader set of paradigms and theories in economic geography, including evolutionary

economic geography, institutional economic geography, geographical political economy, and global production networks. This multiplicity leads to a large number of complementarities with geographical economics. There have already been some complementarities between evolutionary economic geography and geographical political economy on the one, and geographical economics on the other hand since they partly share mathematics as a language for theorization (for details, see Boschma and Frenken (2006) and Sheppard (2011)). Other, non-mathematical based paradigms such as relational and institutional economic geography might see less hope for mutual recognition because of the different methodological bases. However, researchers in these two sub-fields as well as geographical economists could still learn from each other by stepping out their comfort zones. By learning to appreciate mathematical approaches, economic geographers would know more about the robustness of quantitative modeling. Geographical economist can learn to treat spaces heterogeneously and see qualitative methods as complementarities to mathematical modeling, what will also help to know better about the strength of non-mathematical based paradigms in economic geography.

Moreover, the questions economic geography and geographical economics tackle only partly overlap. Krugman (2011) even pointed at fundamental differences: economists would focus on “what if” and “what can we do”, and economic geographers on “how did we get there” questions. We see interesting complementarities already within these fundamental questions. We argue that one first needs to know the answer to the last question “how did we get there” in order to be able to find the right answer to the previous “what can we do” question.

Secondly, the qualitative case-study research method used in economic geography has much more to offer than Overman (2004) stated in his article. Although Overman (2004) and some other authors (e.g. Martin and Sunley, 2001; Plummer and Sheppard, 2001; Rodríguez-Pose, 2001) have realized the essentiality of integrating qualitative and quantitative methods in economic geography, hardly any of them have provided detailed answers and feasible strategies on how such complementarities can be achieved. Enlightened by Steckler et al. (1992), we see four potential ways that qualitative and quantitative methods could be integrated in economic geography (see also Pike et al. 2016).

To begin with, qualitative methods can be used initially to help develop quantitative measures. In economic geography research, good case studies at the beginning stage of a research project usually lead to the generation of hypotheses. Based on such hypotheses, quantitative methods such as mathematical modeling relying on data of larger populations can be applied to test whether these hypotheses are valid, or whether they need to be modified. Since the results of qualitative studies are normally based on small numbers of cases, the qualitatively-based methods can be complementary to the quantitative, hypotheses-based methods at the early stage.

In the second approach we see as promising, a predominantly quantitative study can be deepened with qualitative results to help interpret and explain the quantitative findings. This is the approach in which geographical economists can benefit most from qualitative research carried out by economic geographers. Since the work of geographical economists is predominantly quantitative, it might lead to consequences such as the reduction of the “complexities and richness of EG to stylized mathematical models” (Krugman, 2011, 2), the abstraction of models in favor of ‘discursive persuasion’ (Martin, 1999, 80), and the lack of “social, cultural, and institutional understandings of the economy“ (Amin and Thrift, 2000, 8), which receive most criticisms from economic geographers. In our view, this could be essentially avoided if qualitative studies are used to help interpret and

explain the quantitative findings. For instance, after conducting a numeral-based modeling, the results which geographical economists generalize to larger populations can actually be tested by carrying out in-depth case studies. Thereby, not only could the abstract outcomes gained by mathematical-based models be properly interpreted for certain places, industries, or even firms, but also could lead to a revision of results if some exceptions are found by these specific cases. This is also termed as the "falsification" function of case studies (Flyvbjerg, 2006), which connects to the foundation of regional economist's modeling in critical rationalism.

In contrast to the second model, in the third approach we suggest, quantitative results are used to help interpret predominantly qualitative findings. Economic geographers can use this approach at the conclusion of an in-depth case study when they conduct a survey of the members of a community they have been studying. This approach provides a solution to critique that economic geographers usually receive for their work—the lack of ability to generalize their results. The misunderstanding of the inability to generalize case studies has already been tackled in section 3, but what we highlight here is that the inclusion of quantitative methods (e.g. questionnaire survey, large sample data collections, social network analyses, etc.) can only make the qualitative results stronger if the quality of the results are really good enough.

The final possible approach is called "methodological triangulation" (Flick, 2004, 178). In this approach research is based on different methods, separately conducted, and the results from each approach are used to cross-validate the study findings ex-post. In economic geography the applicability of triangulation depends on whether both methods are useful for testing the same socio-economic phenomenon. This is a big challenge because the work done by economic geographers and geographical economists is often based on different scales and objects. But to keep the conversation between the two groups of scholars rolling, both sides should identify mutually interesting topics, and focus on the same economic phenomena so that their findings can be compared and cross-validated. Triangulation cannot only be seen as a validation strategy (e.g. reciprocal validation between qualitative and quantitative results), but a strategy for justifying knowledge by gaining additional knowledge (Flick, 2004; Kelle and Erzberger, 2004). That said, by doing research on the same economic phenomenon, both sides could get additional knowledge– which might in turn lead to novel research questions and hypotheses.

Overman's third critique - geographical economists as only problem-solvers - brings us to the third complementarity: Economic geography has the potential for solving real-world problems and can complement the rather less space sensitive policy recommendations favored by geographical economics with place-based policy advices. Rodríguez-Pose addressed this issue from two perspectives: on the one hand, he questions whether place-based and place-neutral (or spatially-blind) policies are really "as incompatible as they may seem at first sight" (Rodríguez-Pose, 2011, 353). On the other hand, he notes that "… it is possibly in the policy realm where the greatest scope for collaboration lies" (Rodríguez-Pose 2011, 353) between geographical economics and economic geography. Although there seem to be complementarities, these still are mainly empirical questions that need to be answered. What is for sure needed in the future is profound, comparative research on the effects of place-based and place-neutral policy approaches in order to overcome ideological inertia.

Moreover, in more general terms and going beyond the question of place-based vs. spatially-blind policies, there seem to be complementarities between geographical economics and economic

geography when it comes to evaluating regional policies. The quantitative evaluation methods favored by geographical economists could be well extended by qualitative case-study research carried out by economic geographers on the effects of regional policy programs or individual policy measures.

Overall, much can be learned from economic geography proper and there are many more complementarities than Overman (2004) presented in his article if economic geography is regarded in its full breadth, if case-study research is seen as a rich, complementary research method and if policy recommendations go beyond one-size-fits-all solutions. Furthermore, and this runs through all the points we have discussed, much can be learned concerning a relational and nuanced view on place, instead of using absolute and neutral space. In other words, geographical economics and economic geography are much more complementary than mutually exclusive.

Although we are convinced that geographical economics can certainly learn from economic geography, one of the problems economic geography is struggling with is its lack of voice vis-à-vis neighboring disciplines, such as geographical economics, but also within human geography. As a relatively small sub-discipline economic geography is surprisingly fragmented and heterogeneous, as discussed in Section 2. Since these paradigms and turns operate relatively separated from each other, Barnes and Sheppard (2010) have called this 'fragmented pluralism' (for a wider view on human geography, see Mohan, 1994). When it comes to interacting and co-operating with neighboring disciplines in the social sciences, such as economics and sociology, this clearly is a weakness, because economic geography's unique selling point does not become clear enough. With this paper we hope to have provided a first common ground for further collaboration between economic geography and geographical economics.

Acknowledgements

A first draft of this paper has been presented at the 8th Summer Conference of the German Section of the Regional Science Association (*Gesellschaft für Regionalforschung*), Kiel, Germany, 2-3 July 2015. We are thankful for comments received after that presentation. We are also grateful to Andy Pike for useful comments on an earlier version of this paper. The usual disclaimer applies.

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