

What's wrong with economic geography? Other thoughts on the rift.

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Trevor Barnes is to be commended for his investigations of how research and scholarship are actually practiced. In geography, his home discipline, he has revisited the origins of the so-called scientific and quantitative revolution. So it is only natural that he should turn his curiosity to the origins of regional science. In this paper, which was delivered with much eloquence and passion (I arrived just in time to hear the second half), Barnes again exhibits enviable skill in capturing the personalities and events of past times. Rightfully so, he devotes much of his attention to Walter Isard, who made indispensable intellectual and institutional contributions to regional science from the mid 1950s to the early 1980s, when his interests turned to peace studies. Barnes weaves a Spengler-like “rise and fall” motif across three different entities – the person (Isard), the project (regional science), and the nation (America). These three agents are structural substitutes in a story about Isard’s remarkable professional life and the beginnings of regional science at 3718 Locust Walk during the more innocent and upbeat days of the immediate post-WW II era. But, curiously enough, the story is truncated in the early 1980s when a number of prominent geographers – like Barnes, all from Britain – explicitly or implicitly renounce regional science. Furthermore, the story is in part unsatisfying because it remains so one-sided – we never do get a view of the ever-widening rift between geography and regional science from a regional scientist.

So, at the end of the paper, this reader was left with three nagging questions. First, what else could have contributed to this rift between

geography and regional science? Second, what has happened to regional science since the early 1980s? And third, what exactly happened at the Ambassador Hotel bar during the NARSC meetings in Chicago in 1978? I will try to address the first two of these issues, leaving the third for others that are better informed.

When did the rift begin?

It seems clear to me that the major impetus for the division between geography and regional science is David Harvey's *Social Justice and the City* (1973), an impressive book that has four broad claims. First, any distinction between methodology and philosophy must be rejected because theoretical verification is only achieved through social practice. Second, different versions of space are created and maintained by human practice, but largely for the benefit of the few. Third, all conventional views of social justice are problematic; furthermore, justice can never be achieved in a market-exchange society. And fourth, the embedded processes of urbanism, which serve to concentrate wealth and power, shed light on many wider issues of social and political concern. Written especially for geographers, the book calls for the dismissal of status quo and counter-revolutionary ideas and the adoption of revolutionary theories – those that are dialectically formulated and therefore offer each person the prospect of creating truth.

Harvey makes some very good points in this book and it is a wonderful introduction to the topic of urbanism, one that raises important questions regarding the distribution of power and nature-society relations long before most other social theorists. He also makes the reader think a lot about housing, the nature of public goods, and the role of spillovers in dense, urban settlements. But clearly the book was largely designed to be an indictment of economic geography and regional science, as practiced at that time, for Harvey goes out of his way to assail the foundations of neoclassical thought and the contributions of many highly-regarded social scientists. Among a litany of charges he asserts that: market exchange is largely responsible for scarcity; welfare economics is useless because it addresses neither space nor time; and stylized Thünen-type models only impede society's ability to address its various ills. More generally, Harvey calls for the creation of an entirely new economic-geographic paradigm – one that replaces “value-neutral” positivism with value-led activism, and one that substitutes materialism for idealism in the interpretation of scientific advancement. In short, Harvey refutes the theory, the underlying

empiricism, and the policy applications of Alonso, Beckmann, Berry, Dacey, Isard, Mills, Muth, and Stevens, among others.

What have human geographers been doing in the past 30 years?

Since 1973 human geographers have slowly but surely shifted most of their attention to the various concerns outlined by Harvey. This process was spearheaded by a number of prominent thinkers – many fondly recalled by Barnes – who initiated debate about such matters at the meetings of the AAG in the late 1970s and early 1980s. A flash point for this debate was in the Industrial specialty group, which became hopelessly splintered. As a result a number of geographers – apparently of no consequence to Barnes – began to shift their allegiance from geography to regional science. Since that time geographers have only become more hostile to the neoclassical paradigm, as practiced by economists and others found in business schools. And they have become increasingly receptive to the ideas of many other disciplines, particularly those found in the humanities. So today there is much talk in the discipline about how to practice the new economic geography – or is it economic geographies? One large group seems to have accepted responsibility for finally putting the old economic geography to rest. Here excitement has arisen, at least for now, about such matters as the embeddedness and contingent features of economic systems; the shifting identities of economic actors; the relations of validity, reliability, and reflexivity; and the needs of in situ and qualitative research to complement or supplant quantitative research. It is safe to say that a good number of these people know very little at all about neoclassical economics (or any of its relatives, like rational choice theory). A second group, clearly acquainted with the ideas of Marshall but also with Polanyi and Geertz, has rejected the modeling of space-economies for a “thicker” or “deeper” understanding of how economic regions evolve – especially those areas that have turned out to be economically successful. In some ways, though, this second group has reified the region by returning to the idiographic tradition that constrained much of the thinking in human geography prior to the late 1950s. A third group, entirely comfortable with mathematical formalism but highly sympathetic to Sraffa and Kaldor, has simply found the approaches, assumptions, and axioms of neoclassical modeling to be far too restrictive. These geographers have shown, among other things, how space influences the behaviour of embedded agents, how markets can move out-of-equilibrium and the fortunes of regions can diverge, and how uncertainty and contingency both affect the trajectories of evolving space-economies. Although I obviously simplify matters, it is safe to say that

most – if not nearly all – economic geographers have eschewed the traditional “objective” and “scientific” mode of research where neoclassical theory informs the creation of models, data are collected and analyzed, and these models are either verified or falsified.

But all of this is clearly fine. Disciplines change directions, sometimes very rapidly, as paradigms or tools shift and old problems are replaced by new concerns. Young investigators bring different interests and skills to the table and often address topics that were either cast aside or improperly examined by their older colleagues. And this finally brings me to the one troubling issue at the very heart of the paper delivered by Barnes. Why do geographers, increasingly confident that they are practicing the “right” sorts of economic geography, now feel the need to demean the work of regional scientists who are also keenly interested in issues related to location, space, and the region?

To some degree, of course, this growing resentment is entirely understandable because so many economists – not just Fujita, Krugman, and Venables – have refused to acknowledge important contributions made by geographers to regional science as voiced through the old economic geography. Apparently, these contributions were viewed as being unsatisfactory because the models were static but not dynamic, the equilibria were partial but not general, and the processes were first-order but not second-order. However, to anyone genuinely interested in the history of ideas, the debt of the new geographical economics to the old economic geography should be very clear. But there is something else going on here. Clearly many economic geographers are not just uncomfortable with the approaches and methodologies of regional science, but they are increasingly hostile to its institutional and professional manifestations as well. In my mind the reasons for this are twofold. First, human geography, at this point in time, is less concerned with the production of knowledge than it is with the uses of that knowledge. To some degree this follows from geography’s inherently synthetic approach but it also follows from increasing concerns about social justice. And second, some geographers are simply unhappy because they have not been successful at all in gaining entry to, and then reproducing themselves in, regional science. Regional scientists have simply not been receptive to either the ideas or the activism recommended by David Harvey 30 years ago.

Regional science today

Regional science has changed a lot since the 1970s and, unfortunately,

geographers now play an increasingly marginal role in its practice. It has very much become a cooperative enterprise that is not dominated by any one scholar and is not constrained by any one set of ideas. In fact, scholars sharing an interest in location, space, or the region now come from surprisingly diverse backgrounds and advocate a wide range of approaches and methodologies. These people invariably have stronger professional allegiances to home disciplines but they come together to share ideas in an environment of intellectual tolerance and mutual respect. Theoreticians are informed by empiricists and empiricists learn from theoreticians. Furthermore, many people consult or are actively engaged in formulating public policy. Concerns, issues, and problems are addressed at a variety of spatial scales – not just the local and the regional. Global research on development is not immediately rejected for being too simplistic or for introducing environmental determinism through the back door. Scholars investigate overt behaviour to discern human motivation rather than gather attitudes or opinions regarding this motivation. Ideas are rarely contested the way they are in the home disciplines because turf battles are not seen to be so important. Who maintains institutional power is apparently not of widespread concern to regional scientists, at least not in the ways it is of concern to those in their home disciplines. Professional direction in regional science comes, for the most part, from respected research-active scholars housed at many of the world's finest universities. The agenda for a NARSC business meeting is much more focused on narrower issues related to research and scholarship than the agenda for a typical AAG council meeting. And, by the way, regional science has been holding its own in America (but not in Canada) and flourishing throughout many parts of Europe and Asia since the early 1980s. Global memberships are way up and most journals are getting plenty of good submissions. I suggest that Trevor Barnes attend a future meeting of NARSC to discover this for himself!