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Theory and policy in regional science

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Abstract. These remarks were ones I made as the discussant of Koichi Mera's presidential address to the meeting of the Western Regional Science Association, Rio Rico, Arizona, February 2003. Rather than dwelling on the many points of agreement between Mera and myself, I offer a few thoughts of my own on the role of theory and policy in the field of regional science. Regional science can be seen as an agglomeration of a theory community and a policy community, and both are important to the success of the agglomeration. I describe some ways the two communities should relate to each other.

I know I speak for a great many readers in thanking Koichi Mera for his stimulating address. The relationship between theory and policy, and the reflections of a mature scholar on his discipline, are always of interest, and are topics suitable for this occasion. I share his concern that regional science include work that is relevant for policymaking. I think it will be of interest to most readers, however, if I add some observations of my own rather than simply dwell on the fact that I agree with most of his remarks.

Mera has followed a familiar practice by studying the content of academic journal articles as evidence on the essential character of a discipline. However, I urge some caution on this method for inferring the nature of regional science. There's much more to our field than journal articles. A full understanding of the regional science community requires knowing something about other things regional scientists do: policy analysis, consulting, and ... teaching! We need to study the selection of topics in courses, the assigned readings, teachers' comments on readings, and the papers students are asked to write. And of course the relevant departments are economics, geography, and planning departments, since there aren't many courses actually labeled "regional science." Unfortunately, that kind of research is more time consuming than reading and classifying journal articles and abstracts, and as far as I know, has not been attempted.

This is an important point because journal articles are inherently a biased sample. In the nature of things, they must appeal to a wide readership, thus they must convey research that is readily *generalizable* across regions. At least

that's true of the journals that have the largest circulation. As a result, journals tend to specialize in theory and related subjects, such as application and testing of theory. Many would say generalizability is achieved only by excessive abstraction. But my point here is that the essential function of major journals is dissemination of theory. (This does reinforce Mera's point that the *Annals* is an exception, and certainly I agree that editors make a real difference.) Policy analysis is usually more idiosyncratic, region-specific, region-contingent, and thus of less general interest to a major journal's readers, no matter how important and interesting it is to residents of some region.

I'm pretty sure that if we had a more complete picture of our field, not just articles in leading journals, we would find both theory and policy well represented, even in the field's youthful days.

On the general topic of theory and policy, it occurs to me to apply one of our favorite tools to the study of ourselves. A discipline is an *agglomeration,* as a city or region is; it is an agglomeration of scholars and teachers and subspecialties, as a city is an agglomeration of firms and households and neighborhoods. *Diversity* is valuable in both cases. One of the most important things "we have in common is that we don't insist on having too much in common" (Bolton and Jensen 1995, p. 143). A major value of diversity is having both "clean models and dirty hands" as some sociologists have put it (Hirsch et al. 1990).

Being regional scientists, we know the basic theory why an urban agglomeration can be more efficient than any of its parts. There are advantages of common labor pools, common infrastructure, and rapid transmission of ideas when people have similar problems and experiences and when they use the same language. The agglomeration can be effective even if the separate parts are specialized and differ significantly. A discipline is similar: there are common labor pools of academics, practitioners, and graduate students; a common infrastructure of journals, publishers, and professional meetings. There is a rapid transmission of ideas, in a dynamic process of social learning, because we share language and experiences.

For simplicity, think of the disciplinary agglomeration as having two subgroups, a theory group and a policy group. This bifurcation is of course a drastic simplification, but I hope it's forgivable here since we all deal with oversimplified models. Obviously many of us move back and forth between the two, from month to month and even page to page. As convenient shorthand, I'll use the labels "theorists" and "policymakers," recognizing that the latter group includes many regional scientists who don't actually *make* policy but merely advise on policy or analyze policy options.

Now, even if the two groups are very separate and specialized, the combination of the two can be effective. The two benefit each other, and regional science includes both and ought to include both. What both groups need to do is keep the common labor pools and infrastructure strong, and the transmission channels open.

How can they benefit each other? Both have responsibilities: to communicate effectively and to respect the other group – there's no communication without listening as well as talking. Obviously policymakers need to be receptive to new ideas from theorists; that is a commonplace. In the flow from theory to policy, the most important impact is probably not so much the impact of big ideas, one at a time – blockbusters, as it were – but rather the cumulative impact of many ideas related to each other in a few general

modeling traditions. For example, policymakers and practitioners respect recent theory on agglomeration, but, I suspect, not so much because of their regard for any one article or book, but because of the sheer volume of work on agglomeration. It's similar with theories of social capital. Every application will cite James Coleman's pioneering work, but it would not have been sufficient by itself. To have an ultimate effect, *a theorist must first attract the attention of other theorists*, who attract other theorists, until the cumulative weight has an effect. Consider a third example, this time the *decline* of a theoretical idea, namely the simple economic base multiplier. Policymakers have come to respect it less, as they realize the importance of elasticity of supply, regional competitiveness, and income distribution effects. This change was not so much due to any one or a few critiques, but rather to a large collection of critiques and the cumulative impact.

There's an aspect of this accumulation of theoretical weight that I find especially interesting. Many critics of regional science, and the same is true in economics, complain about so much theory being minor twists on some standard model – inconsequential "tweaking" of assumptions to produce results of little meaning, it is said. Such complaints are heard often in economics departments during the season for hiring new faculty. There are some strong academic traditions that help explain what's going on – standards for dissertations, publish or perish demands for tenure. Without agreeing or disagreeing with the criticism, I note that "tweaking" does add to the cumulative weight of theory, which I've suggested eventually has an effect on policy. As I said, theorists must first convince other theorists, *in fact they must attract tweakers*. Thus tweaking is not without value. There is a question, of course, whether the process, as it now plays itself out in job talks, conference papers, and journals, is really an efficient way for theory to affect policy.

What about the other feedback, from policy to theory? Here, theorists have a responsibility to listen, to respect the policymaker's experience with real places. Frankly, I think this feedback has been less successful than the other one, and less successful than the corresponding feedback in economics. I think that regional science theory doesn't have enough appreciation for real places – "real communities in real natural, historical, and cultural settings with real people – an incredibly diverse lot of people – who, as Alfred Marshall put it when he explained what economics is about, are going about the 'ordinary business of life'" (Bolton and Jensen 1995, p. 139). Theory in regional science has been less successful in *complicating* its models of places than economics has been in complicating its models of households (*ibid*, see also Bolton 1995). I use the term "complicating" in the good sense, in Hirschman's sense of "moving against parsimony" (1984). Regional science theory, I think, has not moved energetically enough to analyze "the ordinary business of life as it is affected by the *places* where people live, work, and consume" (Bolton and Jensen 1995, p. 140, emphasis added).

Regional science has produced work on how location in space affects behavior, but location is not the only important characteristic of a place. Real places are challenging to model because they are produced by a dynamic sorting of people into groups that are not homogeneous but on the other hand are similar enough to allow social interaction and cooperation, and places shape preferences at the same time as preferences shape places (*ibid*, p. 143).

Of course, feedback from policy to theory has not been completely absent. Policymaking throws up challenging questions, theorists rise to meet the challenge. Policymakers express frustration at theory, theorists try to defend themselves. Policymaking often involves working with unusual data, perhaps region-specific data. Theorists want to test theories against new data, so policymaking stimulates empirical testing of theory. Then the empirical testing has a feedback effect on theory itself. Sometimes this flow from policy to theory is especially interesting because it is a flow across disciplinary boundaries. Theorizing by economists about social capital offers an example. The concern with social capital originated in real places, with statements like "It's hard to define and measure, but we know it when we see it," by other social scientists who had ... well, dirty hands if not clean models. Economists, sensitive to criticism that they ignored social interactions outside the market, got interested and developed their own theoretical models of social capital investment.

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