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Regional science: back to the future?

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Abstract. After initiating a provocative discussion on "regional science in crisis" (Bailly and Coffey 1994; Gibson 1994; Plane 1994; Stough 1994; Anas 1994; Vickerman 1994; Casetti 1995), we now wish to present some additional thoughts on how regional scientists can simultaneously make their field more relevant scientifically and more useful for society. At a time when resources are tight, when the number of regional science students is small, when administrators are scrutinizing our budgets and our ability to generate outside money, we need to do something to regain (or is it simply to gain?) our place in the sun. In this paper, we argue that regional scientists will not reestablish their field by using classical approaches to regional analysis alone. It is essential that we look at new ways to answer questions raised by our social, economic, and political institutions. More specifically, we make some observations concerning the history of regional science, its role within universities, and its nature, as well as offering some suggestions concerning how regional scientists can attempt to improve the situation.

1. A brief history of regional science

The intellectual history of regional science is firmly anchored to a tradition of solving critical real-world problems. During the Second World War, the National Resources Planning Board developed innovative new approaches to regional problem-solving and rescued from obscurity methods and techniques already in the scholarly literature. The war was good for regional science; there were compelling strategic reasons to manage the location of productive capacity and to understand the spatial relationships between inputs and outputs.

This text is based on the individual presentations of the three authors during the Presidential Panel, held during the 34th Annual Meeting of the Western Regional Science Association, San Diego, California, in February 1995. Antoine Bailly is President of the WRSA for 1995-1996.

Cold war/hot times

The post-war period was also good. The transition from war-time economy to peace-time economy had substantial spatial implications. The Cold War brought a whole new series of questions for regional scientists. And an entirely different set of questions was being asked by those hoping to have a chance to plan for the post-military-industrial-complex economy. Not only were the questions requiring spatial analysis abundant and challenging, but the work of finding answers was also being generously supported by governments on both sides of the Atlantic. In absolute terms, the physical and biological sciences were certainly the major beneficiaries, but there was plenty left over for social scientists. One of the very first quantitative studies in geography (MacCarty et al. 1956) was funded by the Office of Naval Research. The National Defense Highway System, which produced the U.S. Interstate Highway Network, was probably a major reason that geographers and regional scientists were drawn into transportation planning studies of various kinds. The list goes on, but the point is that most of our best work in the 1940s and 1950s seems to have been tied to specific questions raised by government and, sometimes, by industry.

Happy days

During the 1960s and 1970s, the flow of funds continued, but the connections between inputs and outputs became increasingly blurred. The number of funding agencies proliferated and the number of recipients grew. The 1970s were, we think, a real turning point. Using 20-20 hindsight, we would assert that several traps were being set in the 1970s that are costing us dearly today.

First, money was flowing freely, both because we wanted to keep up with the Russians and because the Kennedy and Johnson administrations in the U.S., and national governments elsewhere, were willing to invest heavily in social programs. Mechanisms to disperse funds were more efficient than mechanisms to account for the value added. Recipients were left with the mistaken impression that research support was an entitlement for God's Chosen – not a simple transaction between buyer and seller.

Second, the market for Ph. D.-type researchers contracted as university enrollment flattened. The seller's market of the early and mid-1960s was gone. The over-supply in universities probably drove down salaries, but perhaps more importantly, it seems to have allowed for a reduction in teaching loads. Reducing teaching loads to encourage research was another signal that, no matter how esoteric our work, it was valued.

The wake-up call

The 1980s and 1990s have brought a rude awakening. These are the decades of the "reality check". The Cold War is over – and so too is much of the spending that it promoted. Nations are cutting back on spending for higher education as they are increasingly forced to assume responsibility for delivery of all sorts of programs. The bottom line is obvious – universities are feeling the pinch and are

increasingly being forced to reevaluate the ways that they internally allocate resources.

Times are tough not only because resources are scarce, but also because more senior colleagues who saw research support as a birthright are being told otherwise, and more junior colleagues are finding that if there is some implicit contract, it is far less generous than the one described by their mentors. To make things even worse, our friends in the physical and biological sciences are making us look bad in the eyes of university administrators, in the eyes of government and industry, and, perhaps, even in the eyes of a public who sometimes see us as champions of the arcane and inconsequential.

The physical and biological sciences perform in the two areas prized by university administrators who are faced by increasingly difficult resource allocation assignments: they teach large numbers of students and they attract extramural funds. Further, their work is seen as being socially useful (this notion is perpetuated by a regular flow of success stories). Finally, in the back of every research administrator's mind is the thought that just maybe his investment in basic or applied science will produce intellectual property that will yield cash returns. There may be tension between the applied and the basic approaches to the physical and biological sciences, but they have learned, apparently, how to minimize the disruption.

The regional paradox: rediscovering the region

In sum, in the 1990s the context has changed significantly; the Cold War is over and liberal approaches to economic development are proposed by policy makers. Inside our universities, regional science is increasingly viewed with skepticism; beyond the hallowed halls of academia, in the "real world", regional science no longer appears as a problem solving field and our models, or at least those building them, are unable to answer the questions raised by decision makers. The result is the closure of departments and research centers in both Europe and North America (e.g. S. P. U. R. in Louvain-La-Neuve in Belgium, the Department of Regional Science at the University of Pennsylvania, and the loss of C. N. R. S. status for research centers like the Bordeaux IER in France). Ironically, however, in the mid 1990s regionalization and regional questions have become hot topics among policy makers in certain countries.¹ Regions, in these countries, have become important components in the global approach to addressing new societal problems, as countries both dissolve into "regions" and join to form supranational blocks. The region has been rediscovered as the lowest common denominator on a global scale. It is not obvious, however, that regional scientists are aware of this!

The main questions raised by the political actors anxious that regional economic recession has consequences for their reelection prospects concern how

¹ For example, the formation of autonomous regions in Spain, Italy and Belgium; the growing importance of regionalism in France; the appearance of new regionalism in the ex-USSR and ex-Yugoslavia; the decentralization of political powers in the Netherlands; federalist policies in Switzerland; and so forth.

long it takes regions that suffer economic shocks to recover, and how they are best able to promote a speedy recovery. To answer these questions, regional scientists must think of how our regional concepts become integrated into plans for action. Indeed, we must ask ourselves whether we are really able to answer these questions, and whether we have any tangible solutions to propose to solve the problems of lagging regions? Do we really want to be the major advisors on regional policies, and to use our knowledge and methods to work on issues of unemployment, and geopolitical conflict and environmental problems? For the moment, many of the people to whom we assume we are addressing ourselves are not interested by our approach to regional problems; they are not listening to us because our best models do not give solutions to their problems and because we fail to convince them of the utility of our approaches.

Without doubt, many of these difficulties identified here result directly from the broad and amorphous nature of our field. In addition, to a significant extent, our difficulties are tied to our unwillingness or inability to position our field where the action is.

2. Of regions and science

It has often been said that, in science and philosophy as in everyday life, it is more important to pose good questions than to provide definitive answers. It seems to us that, in any discussion concerning the nature of regional science, its appropriate future directions, and its place in society, two interrelated questions must be posed *before* all others are considered: "what is regional science?", and "is regional science really distinct from other social sciences?" While we certainly do not pretend to possess the conclusive responses to these questions, we are able to advance some preliminary thoughts.

What's it all about?

What is regional science? This question is not intended to elicit stock textbook phrases involving "social and economic processes within a regional context" but, rather, to provoke reflection on some more fundamental issues that we often take for granted. While certain regional scientists (such as ourselves) often speak of re-orienting regional science, of "selling" it to university administrators and policy makers, and of making it more relevant to society, the nature of the phenomenon in question remains vague. Is regional science really a science? a discipline? an inter-disciplinary field of study (as we are most often told)?, a teaching and/or research agenda?, all of the above?, some of the above, or none of the above?

As students, we learned in our epistemology and philosophy of science courses that a field of knowledge is delimited by its object of analysis and by the theories and methods that it applies to that object. Does regional science have a distinct object of analysis? Our knee-jerk response is to say that its object of analysis is the region, although we know full well that regions do not exist in nature; they are, rather, intellectual constructs that exist only in the mind of the

regionalizer. We know, too, that the concept of region may be applied to an enormous range of spatial scales – from the neighborhood level to supra-national territories covering major portions of the earth's surface. In other words, regional science is about space. But then so is geography. And, for that matter, so are regional and urban economics, regional planning and urban studies, to cite only the most obviously related fields. Does regional science have a distinct and clearly defined set of theories and methods that it applies to its object of analysis? Clearly defined, possibly (to the extent that books have been written attempting to identify the elements of the set), but certainly not distinct. It is virtually impossible to determine at what point the above named fields end and regional science, in all of its eclectic glory, begins.

Through the looking-glass

So where does that leave regional science? In an intellectual, disciplinary and institutional no man's land; in its present form, it has (using the stock cliché) fallen between two chairs (although the actual number of chairs involved is more on the order of three or four). Alternatively, using Isserman's (1993) more eloquent term, regional science may be said to be "lost in space". More specially, regional science may be said to be simultaneously lost in *academic* space, in the sense of existing in very few institutions of higher learning; lost in *intellectual* space, in the sense of being neither a science nor a discipline, in spite of its pretensions; and lost in *terrestrial* space, in the sense of often having little applicability to real world issues and concerns. The problem of being lost in space is compounded by the fact that, as we attempt to demonstrate elsewhere in this paper, regional science is also lost in *time*, in the sense of largely having failed to evolve beyond its 1950s origins. (While the "Happy Days" theme is amusing in a TV series, it is much less so in an academic field that pretends to be useful to modern society). Thus, regional science is lost in time-space, which is roughly the equivalent of having fallen through the looking-glass with Alice or having embarked upon the yellow brick road with Dorothy and Toto.

In sum, regional science is neither a science nor a discipline. Nor is it significantly distinct as an academic field. Indeed, it is our view that there is very little that enables one to distinguish regional science from a certain number of its sister fields, neither in terms of its object of analysis nor in terms of its theories and methods. Having established what regional science is *not*, what can we say about what it *is*? At present, the best response to this question is that regional science manifests itself as a community of scholars that share common interests: in the location of social and economic phenomena in space, in urban areas and the regions that surround them, in applied mathematics, and so forth. In this sense, then, current regional science is more properly defined by the activities of its scholars – their associations, their conferences and their journals – than by its intellectual content. To paraphrase the old joke current during epistemological debates on the nature of geography during the 1960s: "regional science is what regional scientists do".

Clear and present danger

A real danger threatens the intellectual and institutional survival of regional science, or of any other field that consists almost exclusively of a community of scholars whose primary intellectual and institutional bases are in other, more "main stream" and more readily recognizable fields or disciplines. The danger is that regional science may become redundant – i.e., that its subject matter be considered to be sufficiently covered by other fields. Indeed, this may be why regional science programs have historically had such difficulty in being established and maintained in "academic space", in spite of the relatively impressive level of activity in the field, as measured by the number of scholarly discourses, books and articles that regional scientists produce annually. Regional science is at a disadvantage even in relation to other inter-disciplinary fields such as urban studies, which at least has the virtue of being identified with a clear object of analysis, if only in the minds of university administrators and the general public.

Science of regions; regions of science

In a recent guest editorial in the *Papers in Regional Science*, Nijkamp (1994) presents a "product life cycle interpretation" of regional science. His point of departure is that regional science may be likened to a product whose degree of market penetration regional scientists should seek to maximize. This is an interesting analogy, and one that we employ implicitly elsewhere in this paper. But given the observations that we have made above concerning the nature of regional science – what it is and is not – we wonder whether it is the most appropriate analogy to employ. Is it not more useful to consider regional science as a territory – a region, for example? (This analogy is actually highly appropriate when one considers that, like the boundaries of a region, the limits of regional science are both vague and fluid). We may then seek to identify the comparative and competitive advantages of regional science compared to other fields (the comparative advantages being based on regional science's utilization of its factors of production, and the competitive advantages being based upon its local conditions). Do these advantages exist? If so, how may they be strengthened? If not, how may they be developed? After all, the intellectual goods and services that we produce, and our success in creating markets for them, will in large measure be conditioned by the nature of regional science's advantages relative to other fields. Similarly, within regional science we may identify an intellectual "rust belt" consisting of sun-set research that is no longer of use to society, and a series of booming "new industrial spaces" that address current public policy concerns.

In this paper, we present a brief sketch of several factors that represent comparative and competitive *disadvantages* for regional science. Having a subject matter that is both relatively vague and non-distinctive in nature is one disadvantage. Having a product that is often of marginal utility to those seeking to deal with real-world problems is another. Each of these disadvantages is, in itself, relatively serious. Taken together, they represent a formidable challenge to the field and its practitioners – a non-tariff barrier that is not easily circumvented. On the other hand, however, in the fluidity conferred by its low degree of institutional

acceptance and its eclectic and synthetic nature, regional science may be likened to a flexible production system. In theory, it should become relatively easy to rapidly re-tool and to shift attention to new societal problems and issues as they arise.

3. Re-engineering regional science

Math and aftermath

Regional science, in a period of economic growth and postwar recovery, developed efficient techniques for analyzing certain types of spatial and economic phenomena. But most of these techniques are based on a neo-positivist paradigm, and regional science is often considered to be a very abstract field. The preeminence of mathematical theory can be severely limiting when the questions raised are those of unemployment, disparities between rich and poor, and social well-being. R. Stough (1994) writes in his comments on the paper "regional science in crisis": "it is ironic that just at a time when regions are becoming so important, the theoretical and practical bases of the field seem to be sedimenting around a rather narrow body of theory". Although we do not wish to return to the old debate on "scientificity", the boundaries between "sciences" and "non-sciences", and the role of mathematics as the necessary condition for scientificity, we think that the dogmatism of the neo-positivist school of thought is partly responsible for our incapacity to answer the main questions raised by society in the 1990s. Regional science, as a field, has not updated its way of modeling, and many of its conceptual bases are no longer relevant.

Changes are occurring, however, mainly outside of Northern Europe, Japan and the Northeastern USA; in Southern Europe, in Mexico, in Canada and in the Southern and Western USA, regional science is more often perceived as a discipline that is able to guide policy and planning. Many regional scientists are practitioners who have to look for research money; many of them are both academics and practicing consultants. They have not only to deal with issues of pure theory, but also must answer questions raised by policy makers and other clients. For these reasons, their approach to regional science has evolved away from the influence of the Walrasian tradition towards a more pragmatic one. L. Gibson (1994) raises a fundamental question about the links between these two approaches: "Can faculty reward systems evolve to accommodate the new realities of applied research?" It is a question that any regional science department has to solve if it wants to survive in the rough and tumble academic world of the 1990s.

Desperately seeking paradigm

If regional science wishes to be more than a positivist discipline that borrows concepts and methods from economics and geography, it has to find new epistemological foundations. Epistemology is the study of the nature and limits of our knowledge, and the study of the principles and methods that guide scientists in the elaboration, validation and use of concepts, models and theories. In

many disciplines the epistemological reflection is done in a permanent way (Lakatos 1984; Feyerabend 1979; Boudon 1990); this is not the case in regional science, in spite of a few presidential addresses in the 1970s and again in the early 1990s. The main reason is the multidisciplinary character of regional science; but it is also due to the fact that some regional scientists refuse to recognize that our field has its problems. By closing our eyes, we wish to avoid the harsh reality of the lack of relevance of our field in the 1990s and the tension between the goals of sophistication and relevance. We do not see that our narrow perspective, which emphasizes problems that fit our models and economic processes, rather than people, prevents us from contributing to the resolution of major societal problems. It is now time to reevaluate the foundations and goals of regional science, and clearly identify directions in which we want our field to move.

Similar issues were raised in a series of panel discussions held during several annual meetings of the Western Regional Science Association and the *Association de Science Régionale de Langue Française* and gave birth to a discussion published in 1994 in the *Papers in Regional Science* (vol 73, no. 1). A group of young regional scientists from Bordeaux², worried by the somber prospects of the field, and after reading the paper on "regional science in crisis", raised three important questions for the future:

1. Can we find basic concepts for a renewed regional science, concepts that are able to differentiate it from spatial economics and economic geography?
2. What are the most important subjects for regional science in the 1990s? Industrial and services location? Population well-being? Land rent? Environment questions? Pollution? Or something else?
3. How can we be more relevant, and become better integrated into the formulation of regional policy?

The way we were

Some answers to these questions were already proposed in the 1960s and 1970s. Lay Gibson (1994) tells us to go back to the early literature, and shows that the first issues of the *Papers and Proceedings of the Regional Science Association* and basic books like the *Methods of Regional Analysis* (Isard 1960) were full of applied work. The gap between academic regional science and policy matters was not so wide and regional scientists were addressing practical issues.

In an attempt, at least partially, to answer questions asked by the young regional scientists, let us recall several basic views of regional science advanced in the literature. The famous title, by Törsten Hägerstrand (1970), "What about people in regional science" represents a way to look at regional science as a field able to contribute to a more livable world. Stan Czamanski (1976) follows Hägerstrand's lead when he talks of a "better understanding of individual and societal motivations", and also of the "protection of the environment". Morgan Thomas (1977) proposes the study of "space society" rather than only "space

² This group, under the direction of Claude Lacour, calls itself "Columbus" since they are seeking the "new world" of regional science.

economy". In the 1990s Roger Bolton (1992) adds "place prosperity vs. people prosperity" and Rodney Jensen (1991) speaks of "responsibilities in terms of a service to regionalism in the form of practical responses to regional problems and concerns". In these quotations two common points or themes emerge: concerns about people and their welfare, and the desire to be relevant. But how many regional scientists share these views? Most journals are very specialized and papers are accepted more for their technical sophistication than for their relevance to problem solving in the real world. In a narrow academic world, where the jobs are scarce, it is difficult not to play by the rules of the game. But it is naive not to remember that these rules were established 30 or 40 years ago!

4. Build it and they will come³

If regional scientists are able to establish the foundation for a renewed regional science – one able to answer the questions posed by society in the 1990s – students, business and government (and perhaps even university administrators) will come knocking on our doors. If we are unable to do so, we may well be on our way toward extinction, at least on an institutional level. In order to succeed, we must learn from our failures in the 1980s and the early 1990s. In particular, we must develop a new and higher profile, as well as employment opportunities, both within universities and in the "real world". While we would expect many colleagues to question the specific details of our admittedly incomplete "three-minute history and diagnosis" of regional science's difficulties, we believe that few would contest our view that a problem does exist. What, then, can be done? We would suggest that regional science will be the beneficiary if we – individually, collectively and institutionally – focus our energy in several directions.

First, regional science must now cope with the challenges that face society at both global and local levels. It needs some adjustments in order to move away from the dominant positivist paradigm and to place more emphasis on "space society" issues such as people prosperity, place prosperity, environmental protection, unemployment, population migration, sustainable resources, regional equity, and social well-being. In countries losing their middle class (or failing to develop one), where the gap between rich and poor is becoming a major problem, where pollution is a daily issue, regional science needs to be able to address these societal questions. It is no longer a matter of measuring regional disparities or of locating productive capacity to respond to the concerns of a Cold War. We need new ways of looking at the regional problems of the 1990s, problems which are often very different from those of the 1950s.

Second, we must better establish regional science as a teaching field – not just a research field. This means not only expanding our graduate programs, but also, more importantly, aggressively moving into the world of undergraduate instruction. Only undergraduate programs can generate the sort of student numbers that

³ ... as the voice said to Kevin Costner in "Field of Dreams". (Thanks to Jon Miller, WRSA President-Elect, for the inspiration).

stand out on instruction reports. And expanded exposure to regional science among undergraduates should improve our access to advanced degree candidates.

Third, we need to spend more time dealing with the sorts of issues that business and government need to have solved. In other words, regional science must re-establish its role as a problem solving field, as a field that is relevant to policy makers in the 1990s. In order to do so, we need to rekindle our interest in practical issues of concern to society, and to strive to overcome "the deep ignorance among regional scientists of the nature of practical policymaking and implementation" (Breheny 1984).

Fourth, and equally importantly, we need to expend substantial efforts in "selling" what we do in explaining what we have done. It is already painfully obvious that almost nobody is willing to take our word for the fact that there is some intrinsic value to what we do. Nor, apparently, is the world willing to buy the assertion that we produce raw materials that can be harvested by the "lesser lights" who are charged with the implementation of regional policy. We also need to orient certain of our publications toward broader audiences; our publications should be able to inform the intelligent general public of what we are capable of doing.

Fifth, we need to be very creative when it comes to funding. The pace has been set by colleagues in the physical and biological sciences. We can no longer expect our institutions to provide support at even subsistence levels. If we are going to stay in touch with colleagues and be both producers and consumers of research, we are going to need to learn to leverage institutional resources or, perhaps, be fully self-supporting.

Sixth, we need to build regional science institutions and institutionalize what we build. We have not had good luck when it comes to building regional science departments and have had even worse luck when it comes to maintaining them. Perhaps the answer is in building programs – not departments – and in building associations with strong conferences and widely disseminated publications.

Finally, (and as discussed in Bailly and Coffey 1994), we must bridge the theory-practice gap. A certain amount of tension is, probably, inevitable. But one need only look at the fundamental relationship between biological sciences and medicine, pharmacology, and biotechnology, or between the physical science and engineering and information technologies to realize that although there may be a division of labor, the bottom line is a win-win-win outcome: basic research wins, applied research wins, and society wins.

5. Conclusion: to be, or not to be?

A final question needs to be asked: is it desirable (or, indeed, necessary) for regional scientists to work to change the status quo? Is all well in the kingdom of regional science or rather, to paraphrase Shakespeare, is something rotten? Could it be that the sole tangible problem besetting regional science is that some of us have developed inflated expectations concerning the status and utility of our non-discipline? It is clear from our comments where we stand on these issues. We believe that change is needed, change in the form of refinements to the intellectual

bases of regional science and of an enhanced capacity to address public policy concerns.

In our view, regional science is now at a critical divide. We can either adjust to the realities of a new environment or slip further into obscurity both within our universities and within the worlds of government and business. In this paper, we have attempted to identify several directions that hold promise for "fixing the fix we're in" (Gibson 1994). The trick, of course, is implementing the agenda that we call for while making sure that we hold the academic high ground. We want to broaden the audience that we teach to and we want to attract the best students. We want to be called upon to answer questions for business and government, but we want to do it as high profile experts, not as backroom staff workers. And we want to see our efforts valued and supported at levels more like those commanded by surgeons than by poets.

The cold truth is that if we are going to get what we want, we are going to need to sell ourselves to increasingly skeptical consumers in university administrations and to potential consumers of our services in government and industry, who usually don't even know that we exist. In the process, we will need to remind ourselves that "selling" is not the same as "selling out".

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