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Interventions on the 'moribund backwater' forty years on

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An unorthodox celebration: looking at political geography in historical perspective

Marco Antonsich

People often get together to celebrate anniversaries of events that have been important in their life. These celebrations help both cement the union among themselves and, as psychologists (Breakwell, 1986) would say, confirm a sense of continuity between the past and the present, so to offer an orientation for the future. As such, it is not surprising that political geographers have come together to celebrate, for instance, the centenary of the first academic book of political geography, Friedrich Ratzel's *Politische Geographie* (Antonsich et al., 2001), the centennial of Halford Mackinder's influential lecture (*The Geographical Journal*, 2004), or the centenaries of both the birth (Goblet, 1931) and the passing (*Hérodote*, 2005) of an important *géographe libertaire* of the nineteenth century, Elisée Reclus.

The interventions published here should also be regarded as a form of celebration, though an unorthodox one. In fact, they recall an event which does not reflect a positive image of political geography and its practitioners. In July 1969, in the midst of universal enthusiasm for the first man landing on the moon, the *Geographical Review* published a book review by an influential urban and economic geographer, Brian Berry (1969). The book in question (Russett, 1967) was a meticulous quantitative regional analysis of the international system, which, according to Berry, offered a "superb contribution to political geography" (p. 450). The only problem: Russett was not a political geographer, but a political scientist.

This prompted Berry to brand political geography as a “moribund backwater”, in need of borrowing the quantitative political science techniques in order to improve its scientific progress.

Forty years later, as I again came across Berry’s review, I suggested to the editors of *Political Geography* that they publish a short collection of interventions, which could comment critically on that piece. The invitation was addressed to two eminent political geographers, Julian Minghi and Ron Johnston, whose contributions to the sub-discipline are certainly outstanding, and whose careers span the entire period under review. Brian Berry also accepted the invitation to participate to this ‘celebration’ and I was invited to write the present introduction. I should point out that the present and following interventions focus intentionally only on the Anglo-Saxon literature, as this was the audience which Berry’s original intervention targeted.

Rather than summarizing the arguments put forward by each of the contributors, in this piece I want to look at Berry’s original comments in historical perspective, by locating them within the debate which has since characterized the sub-discipline. My argument is that Berry’s view, as formulated both then and now, is telling of three major shortcomings which have since characterized political geography and which, without finding definitive answers, have periodically emerged in debates among its practitioners. These three shortcomings are: 1) political geography’s lack of impact on the wider intellectual arena; 2) the lag between theoretical and methodological innovations carried out elsewhere and political geography; and 3) political geography’s inability to clearly define its core subject matter and its disciplinary boundaries.

As far as the first shortcoming is concerned, I shall not spend too many words here, as I have already addressed a similar point in a recent editorial (Antonsich,

2009). On the one hand, it is undeniable that, apart from few exceptions, political geography has had hard time making itself heard outside the geographic precincts, particularly among political scientists (O' Loughlin, 2000). Yet, on the other hand, it is also true that a great deal of contemporary critical political geography, particularly that informed by Marxist and feminist ideas, seems to privilege a less conventional notion of what 'impact' means. In fact, within this perspective, a political geography that matters is not one which competes in the marketplace of ideas, but one which is directly involved in forms of political activism and resistance (Sparke, 2008).

The 'lagging behind' was, and still is, Berry's major criticism of political geography. 'Moribund backwater' was indeed a critique based on political geography's inability to develop at the same pace as the one of other social disciplines. Looking in historical perspective, this is certainly not an isolated critique, since it has been voiced by political geographers themselves, thus contributing indirectly to portraying the history of the sub-discipline over the last four decades as one of 'missed boats'. Ten years after Berry's intervention, Peter Taylor (1977; 1979), a key-figure in the revival of political geography in the 1980s, confessed that, after being left behind by the quantitative revolution, the risk for political geography was then to be left behind by a politically based revolution. In fact, while other social and political disciplines were addressing the nature and role of the political in various domains of collective life, the research focus of political geography was still the state, often studied in a-theoretical terms (Dear, 1982; Hudson, 1982; Kirby, 1986). This avoidance of the political at various scales prompted Johnston (1980) to renew Berry's criticism of a discipline still appearing to be a 'moribund backwater'.

In the following years, though, political geography gradually emerged out of this backwater condition, first by incorporating a political economic approach, often cast

in Marxist or neo-Marxist terms, and then, more generally, by increasingly adopting methods and approaches from social theory. Yet, the critique of a sub-discipline in constant need of catching up to its neighbors did not abandon political geography. Whether it was gender, racism, or social movements (Reynolds, 1993) or, again, the cultural questions which should characterize any theorization of the state (Marston, 2004), political geography was viewed as a sub-discipline which was still “missing the boat”, as expressly mentioned by John O’Loughlin (1991) in relation to its failure in addressing the most pressing issues of contemporary world affairs. This issue came up again during a panel discussion among political geographers (*Political Geography*, 2003) at the annual conference of the Association of American Geographers held in 2002 in Los Angeles. Among others, Joe Painter, commenting on the call for a “new political geography” along the lines of cultural or economic geography, dismissed this hypothesis on the basis that he did not want to have “another example of political geography arriving late at the scene of new intellectual developments” (Painter, 2003: 637-638).

The third and last criticism refers to the content of political geography and who counts as a political geographer. In fact, during these last four decades, Russett has not been the only non-political geographer to be praised for his brilliant work of/in political geography. Back in the 1970s and 1980s, the revival of the sub-discipline came indeed also thanks to scholars who did not identify themselves as political geographers, like David Harvey and Julian Wolpert, who, according to Taylor (1982: 1), “[...] produced some of the best political geography of recent years” (see also Dear, 1982). Later on, other political geographers acknowledged that the “most exciting” (Kirby, 1986: 192), “most good” (O’Loughlin, 1991: 335), “most innovative” (Reynolds, 1993: 389), “most inspiring” (Sparke, 2008: 435) political

geography was produced outside the sub-discipline (see also Johnston in this issue). This admiration obviously questions the core and the boundaries of political geography. Reviewing the progress of the sub-discipline between 1988 and 1991 (see the yearly progress reports in *Progress in Human Geography*), O'Loughlin manifested a clear uneasiness at defining political geography, specifying its core subject matter, and agreeing on its theories, methods and aims. Yet, besides some authors (Kirby, 1986; Reynolds and Knight, 1989) sharing a similar concern for the lack of theoretical coherence, political geographers have, since the 1980s, increasingly accepted theoretical and methodological eclecticism, pluralism and heterodoxy as part of the identity of the sub-discipline (Taylor, 1982; Sidaway, 1995; Agnew, 2003; Mamadouh, 2003). Other political geographers (Ó Tuathail, 1996; Painter, 2003) have also explicitly questioned these very disciplinary boundaries in the name of a social theory aimed at deconstructing and challenging any type of boundary. It is significant, for instance, that the most recent progress reports in the *Progress in Human Geography* by Lynn Staeheli (2008; 2009) hardly mention "political geography" or "political geographers", preferring instead the generic terms geography and geographers. With the increasing politicization of all social disciplines, first with the Marxist turn of the 1970s and then with the cultural turn of the 1980s, it seems as if political geography has lost control of the 'political' (Marston, 2003: 633), being unable to claim any monopoly of concern in this field (Low, 2003: 625). The political is everything and everywhere, thus challenging the very existence of a discipline called political geography (or political science for that matter). To counter an 'anything goes' trend which has recently produced political geographies that are neither very geographical nor very political, John Agnew (2003) has suggested a refocus in the sub-discipline around specific concepts (territory, space, place,

network, and scale). This does not mean a call for a new orthodoxy, as Agnew himself actually praises theoretical and methodological pluralism, despite the fact that this pluralism, according to him, puts political geography in a constant risk of collapsing.

Forty years since his provocative intervention, it does not seem that Berry has changed his ideas. During this time, political geography has undergone a formidable revitalization, as testified by the number of its practitioners, the quality of their academic output, and the variety of their inter-disciplinary conversations. I am not sure whether political geographers should be particularly concerned if these do not emerge out of a quick online search or a chat with colleagues in a political science department. Yet, it would be erroneous to believe that Berry's criticism is simply off-target, since, as I have tried to argue, it actually points to some important issues which have not faded away during these years - rather the contrary. For the time being, though, I believe that political geographers have more than one reason to celebrate this odd anniversary: happy birthday, old 'moribund backwater'.

Half-Right on Backwater, Wrong on Moribund

Julian Minghi

On my arrival in Seattle to start graduate work in January 1958, an "old soldier" and advanced doctoral student gave me, a fellow-Englishman, some well-intentioned advice for success in the program – take academic work seriously, do not fool around,

and get into the new quantitative geography in which the Washington department was then establishing a strong reputation. As Brian Berry was clearly one of the star graduate students and held in high esteem by all, I followed his advice as best I could but lost his friendly oversight when, as one of the first “space cadets” to graduate, he went off to Chicago later that year. Without Berry as a mentor, I finished up working under the late Douglas Jackson, the Soviet Union and political geography specialist. Yet I still managed to include spatial analysis of survey data in my dissertation – indicating an impact of the US-Canada boundary on TV viewing behavior among the borderland inhabitants (Minghi, 1963).

Given my familiarity with Berry’s view of priorities in Geography, I was hardly surprised when a decade later he described the sub-discipline of political geography as a “moribund backwater” in his review of a 1967 book by political scientist Bruce Russett on international regions (Berry, 1969). Berry much admired Russett’s research in defining different world regions by clustering states according to a variety of factors. Regions of cultural and social homogeneity were identified by factor analysis of a host of variables; regions of similar political attitudes by analysis of UN voting patterns; regions of political interdependence by factor analysis of common membership in international organizations; regions of economic interdependence by factor analysis of trade; and regions of geographical proximity by factor analysis of airline distances. Berry asks rhetorically how political geography might greatly benefit from building upon this type of innovative work, especially if, as he suggested, Russett’s findings can be taken a further step in a geographical direction by a focus on location analysis. Russett was providing political geographers with a challenge and an opportunity and, Berry felt, political geography’s need for help was great – hence the “moribund backwater” comment.

My concern here is not to track the growth of the political geography over the past 40 years but rather to evaluate briefly the veracity of Berry's description based on the evidence available at the time.

Berry, an urban/economic quantitative geographer, had limited expertise in political geography. The resident political geographer at the University of Washington had been Jackson and those colleagues closest to political geography at Chicago had been Marvin Mikesell and Norton Ginsberg. All were primarily "regional" geographers (Mikesell – Western Europe and Ginsberg – South and East Asia). Such geography among the quantitative revolutionists was generally not highly valued and considered by some as "mere description".

There is no doubt, as Berry implied, that approaches to political geography had fallen behind economic and urban geography in applying innovative ideas and techniques developing among sister social sciences. Indeed, I had written an article in 1966 very critical of the anachronistic texts representing the sub-discipline (Minghi, 1966). Moreover, the inspiration for Roger Kasperson and me to propose a book in political geography in 1967 was our belief that we could wipe out this anachronism and thus bring political geography up to speed (Kasperson & Minghi, 1969). In the Introduction, we wrote that, among the many factors that had contributed to the decline of political geography, were the many synoptic and descriptive studies of the form of states. We felt that considerable confusion still existed as to the objectives, conceptual structure, and scope of political geography, a confusion demanding a rethinking of basic issues and the formulation of a clear focus and organization. Concern with theory, rigorous hypotheses formulation and testing, and comparative analysis had always been present but was emerging more strongly in the mid-to-late 1960s. Yet, for contemporary works in the areas of integration, growth and

development, international systems and voting patterns, we had to turn to non-geographers such as Karl Deutsch, Amitai Etzioni, Anthony Downs, William Foltz, Kenneth Boulding, and, yes indeed, Bruce Russett! In a review of our 1969 book, David Reynolds pointed out a basic dilemma – that the book may have been premature as in some areas defined as central to political geography, the relevant non-descriptive research literature did not yet exist, a point that certainly gives some support to the Berry backwater connotation (Reynolds, 1970). Reynolds did, however, also see some recent encouraging signs to suggest that the paucity problem would soon be rectified. Also, in a 1981 assessment about progress made in American political geography over the previous two decades, I had very few citations prior to 1969, again lending weight to Berry’s assertion (Minghi, 1981).

As the book by Kasperson and Minghi was published in late summer of 1969 and was submitted in final form about a year before, the chronology would be very close to submission and publication of the Berry review. So it is fair to say that the literature field Kasperson and I were viewing was identical to that which Berry could see and on which he presumably based his pejorative description.

In 1927 Carl Sauer branded political geography as a “wayward child”, a rather gentle filial metaphor from someone known for his candor (Sauer, 1927). And four decades later Kasperson and I were still cognizant of “the often peripheral position of contemporary political geography in research and pedagogy both in geography and in the social sciences generally.” Ironically, looking back 40 years again now, we have Berry making a much more negative assessment. Clearly the tremendous growth of political geography over this last period has been impressive, with strong contributions especially in electoral geography as documented by Johnston. Several excellent doctoral programs emerged. On the basis of this quality research production,

two leading English-language journals (*Political Geography* in the 1980s and *Geopolitics* in the 1990s) were launched, and political geography became one of the most active AAG specialty groups since the early 1980s. One could say that it has come to life and joined the main stream. But then it was already well on the way to recovery in 1969. For example, in the study of elections and environmental problems research was appearing using spatial analysis of electoral data and author-generated survey data as evidenced by works of Cox (1968a) and Kasperson (1969).

Was there in the late 1960s a paucity of studies in political geography using quantitative methods and the techniques of spatial analysis? Undoubtedly, yes! But did this mean the sub-discipline was in a moribund backwater? Certainly not! It lacked studies in some areas of research such as the Russett variety and that is why in our 1969 book Kasperson and I decided to select a number of studies from the IR (international relations) literature to demonstrate what could be done. But we also included a reading from Saul Cohen's 1963 book on global strategic regions - the first post-WWII work on geopolitics (Cohen, 1963). A pertinent point is that, in the longer run, the international relations community was less attracted to the types of analysis reflected in the works of Russett and Rudolph Rummel while taking a much fonder liking to the global modeling based largely on qualitative data of the Cold War era represented by Cohen's work. Indeed, global regionalization of states based on quantitative data was not then and has never been a main focus in political geography. Rather such studies have been just one aspect of a wide array of topics covered by the field at *all* levels in the political hierarchy.

The 1960s were known for their excesses and many of the attitudes held and comments made by both sides of the debate over the role of the quantitative revolution in geography were tinged with hyperbole. The discipline went through a

similar decade of trauma in the 1980s as it absorbed the new digital technologies of remote sensing, computer-assisted cartography and GIS. Perhaps it was fitting that at the end of the 1960s momentous decade dominated by the quantitative revolution, we got a final shot of hyperbole from Berry. The evidence partially supports the backwater metaphor in that political geography was slow to adopt relevant ideas from sister social sciences and to integrate the new technology into research activity. But clearly political geography was far from moribund in 1969.

From backwater to mainstream: reflections of 40 years

Ron Johnston

Rarely can a comment in a book review have been cited as often as Brian Berry's (1969: 450) question "What might political geography, that moribund backwater, become from an infusion of work of this kind?" – referring to Russett's (1967) monograph on international regions. (Or is that an over-interpretation? I checked on ISI Web of Knowledge to find that, according to its (undoubtedly incomplete) records it has been cited only 12 times – three of them by me!).

Berry was undoubtedly right in his largely unchallenged characterization of political geography then. It was not widely taught (certainly not in my undergraduate days) and little of the published work so classifiable had either intellectual sparkle or bite (on which see David Hooson's comments on the teaching of political geography at Oxford: Johnston, 2009a). Certainly, as John O'Loughlin (2009) records, the best textbook available in 1969 (published in the same year as Berry's comment: Kasperson and Minghi, 1969) contained nothing to suggest that the 'theoretical and

quantitative revolutions' then sweeping through Anglo-American geography was having an impact on political geography. Nor did other texts – including one whose 1989 edition maintained the outlook of its first, published in 1965 (Glassner and de Blij, 1989 - see also Taylor, 1977).

What has happened since? Is it still a backwater? The number of scholars who now identify as political geographers, the success of journals such as *Political Geography* (once *Quarterly*), *Geopolitics*, and *Space and Polity*, and a range of other indicators all demonstrate that political geography is part of the contemporary mainstream. Furthermore, after decades in the wilderness geopolitics has been successfully revived (Hepple, 1986; Dalby et al., 2001) and a new sub-sub-discipline of critical geopolitics created. The backwater has become an open tidal estuary, it seems!

But moribund? It depends on whether you think that what contemporary political geographers do is either 'not progressing or advancing; stagnant' (one of Dictionary.com's definitions of the term) and/or 'in a terminal decline; displaying such a lack of vigor and vitality that recovery appears impossible' (*OED*). Making that judgment means returning to the context of Berry's statement – a review of a book that used quantitative analysis to find order in the world, the leitmotif of the 'theoretical and quantitative revolutions' in which he was a key participant.

So did political geography join the revolutions, and has it matured along with other aspects of what became known as spatial science? Much political geography is not readily addressed using techniques designed for large data sets – it is concerned with the particular if not the singular, with contingency rather than generality. But some parts of the discipline offer massive opportunities.

By far the main example of this is electoral geography which – despite an attempt by Prescott (1959; see also Chapter 4 of Prescott, 1972) – had no accepted status or definition in 1969. Change was en route, however, largely due to two individuals working separately at Midwest universities. Kevin Cox's PhD at Illinois (1966) was on voting patterns in England and Wales and in 1968 he began publishing papers from both it and subsequent research – which included a pioneering sample survey. His 1969 *Progress in Geography* essay on 'The voting decision in a spatial context' (Cox, 1969; Pattie et al., 1998) provided a firm theoretical foundation for the nascent sub-discipline (drawing almost entirely from outwith geography, as Berry suggested was necessary), and his empirical studies illustrated the use of methods for analyzing both aggregate and individual data to test hypotheses about the geography of voting patterns.¹ (Cox's – 1968: 2 – first argument for the “mathematization of political geography”, following Haggett and Chorley's - 1967 - applied to other areas of the discipline, quoted Bunge's – 1966a: 267 – claim that “political geography is a subject rich in mathematical applications”).

The other electoral geography hearth was at the University of Iowa, led by Dave Reynolds who applied spatial interaction modeling to voting patterns and involving students such as Clark Archer (who with Reynolds wrote an important – though only published in a discussion series – paper on the nature of electoral geography: Reynolds and Archer, 1969). Peter Taylor visited the Department in 1970-1971 (on Kevin Cox's recommendation) and with the group initiated investigations of sectional patterns of voting in the USA using factor analyses, set within a firm theoretical framework derived from political economy – best exemplified in Archer and Taylor (1981).

Of the two hearths, the first soon cooled. Cox shifted his research interests and published no new substantive studies after 1972; no doctoral students had worked with him on electoral topics.² And although Reynolds too shifted his substantive research interests, the Iowa group that he fostered continued, multiplied and diffused, however, retaining its focus on sectional voting patterns. Work in that genre ignored much of Cox's agenda and failed to embrace the wide range of data sources (especially social surveys) and analytical methods that Cox had been exploring. Meanwhile Peter Taylor returned to the UK and continued work on redistricting, based on his earlier explorations of classification and regionalization which was linked to Bunge's (1966b) seminal paper on gerrymandering – a practice he had experienced first-hand while in Iowa. This led to his seminal book with Graham Gudgin (Taylor and Gudgin, 1979) on *Seats, votes and the spatial organisation of elections* (which indirectly span-off into his pioneering work with Stan Openshaw on the modifiable areal unit problem: Taylor and Openshaw, 1981) and he began a fruitful collaboration with Ron Johnston (Taylor and Johnston, 1979). Like Cox and Reynolds, after a few years his interests shifted elsewhere, but Johnston continued pursuing the Iowa-Taylor-inspired work on redistricting, combining it with a renewed interest in Cox's agenda regarding voting patterns, exploring – with a range of collaborators – aspects of the importance of place in British electoral behavior. (Much of this work is synthesized in Johnston and Pattie, 2006; for a personal view, see Johnston, 2009b.)

That long stream of British work emanating out of Ohio and Iowa, employing increasingly sophisticated quantitative applications, remained in the mainstream of spatial analysis – indeed introduced it to the UK political science community. That has not been the case in North America, on the other hand, where with a few

exceptions – notably John Agnew and John O’Loughlin and their collaborators, mainly working on non-US topics using modern quantitative methods to analyze aggregate data – much of Cox’s agenda has been largely ignored and work in electoral geography has not developed in parallel to that in the UK, where his seminal studies have been much more influential (on which, see Johnston, 2005).

Within political geography, therefore, electoral geography has emerged out of Berry’s moribund backwater in directions that he was signposting in 1969 – but movement has been faster and farther on one side of the Atlantic than the other. But it has to a very considerable extent been marginalized within political geography. For example, its outputs get but very brief mention in Cox’s later text on political geography (Cox, 2002) and none at all in his earlier introduction to human geography as spatial science (Cox, 1972); there is also only a limited discussion of electoral geography in Agnew’s (2002) historical survey of political geography. Recent *Handbooks* (Cox et al., 2007) and *Companions* (Agnew et al., 2003) have one or two separate chapters on electoral material (the latter also has one by O’Loughlin – 2003 – on spatial analysis in political geography which draws mainly on electoral studies for its examples), but only Flint and Taylor (2007) integrate findings from studies in electoral geography into a theoretical structure. Only by doing that, they claim, can electoral geography be considered a ‘success story’ and not just ‘a bitty and uncoordinated pattern of researches which has produced a large number of isolated findings but few generalizations ... It is not at all clear where electoral geography has been leading’ (p. 195).³

But what of the remainder (bulk?) of political geography? Overviews – whether in the annual reviews commissioned for *Progress in Human Geography* or in such volumes as *Geography in America* (Gaile and Willmott, 1989, 2004) – indicate that

only the first half of the ‘theoretical and quantitative revolutions’ has substantially affected the entire sub-discipline. Theoretical perspectives abound – just check the contents of *Political Geography* – but few are associated with quantitative analyses. This is partly because of the subject matter, but also because of an antipathy to not only quantification but also the sort of theory with which it has always been (often wrongly) associated: as Peet (1998) argued, spatial science taught us the need for theory – but (for him and many others) the wrong sort of theory. But this need not have been/be the case: an early edited volume (Cox et al., 1974) illustrated how formal evaluations of hypotheses could be deployed in a range of studies, but the leads were rarely followed. Three decades later, O’Loughlin (2003: 43) concluded that quantitative political geography could either be more firmly integrated into the wider sub-discipline or ‘become further isolated’; the former would only occur if political geographers used the sophisticated tools now available – ‘political geography theory has raced ahead of empirical tests and statistical expertise over the past 20 years, [but] the gap can be narrowed and many untested theoretical propositions can be checked’.

Such non-quantitative work is certainly not moribund, however. Nor is it any more the case, as Berry expressed it (1969: 451), that ‘The experience of other parts of geography is that a little opportunistic borrowing never hurts. In political geography a lot might help; for the need is great’. Political geography has – in effect – followed his lead. It may still be a net importer (Laponce, 1980) and probably needs to import more, as suggested above with regard to electoral geography whose links to political science, in the USA at least, are largely one-way, weak and ill-developed, but it has learned the lesson. Theoretical developments on topics such as territoriality – especially by Bob Sack, John Agnew and Peter Taylor (see Johnston, 2001) – are

closely associated with political geography, but have much wider resonance. And of course, there are always the questions ‘what is political geography?’ and, perhaps more importantly, ‘who is a political geographer?’. Almost certainly, neither Derek Gregory, nor David Harvey, nor Neil Smith would identify as political geographers – but *The colonial present* (Gregory, 2004), *The new imperialism* (Harvey, 2003), and *American empire* (Smith, 2003) are highly significant, theoretically sophisticated contributions to political geography (however you might want to define it).

Much of Brian Berry’s research during his long career has faced the issue of whether correlation implies causation – handled by setting empirical analyses within clear theoretical structures. But can causation be implied from the post-1969 changes in political geography? Berry challenged those who identified themselves as political geographers at the time to sail out of the moribund backwater and enter the braiding streams of theoretical inquiry and robust analysis. Some did – and many others have since joined them. The directions they have followed may not all have been those that he anticipated – some may have meandered, even got stranded in an oxbow? – but the 40 years since he urged political geographers to join the flow have seen a vibrant flowering of research. Moribund backwater now – certainly not.

Is there a ‘new political geography’?

Brian J.L. Berry

Had political geographers been comfortable in their skins in 1969 my characterization of their subfield as a moribund backwater would have been dismissed

and soon forgotten. What could a young upstart know of their work? But they were not comfortable and the characterization still raises hackles, hence this intervention.

I was not as naive about political geography at the time as Julian Minghi would have us believe in his somewhat defensive remarks. As a lad I grew up just two blocks from Halford Mackinder's home in a small river town in Lincolnshire. The geography master at the local grammar school which we both attended, albeit decades apart, made sure I knew about the great man and his ideas. I also was well aware of political geographers' retreat into regional description after World War II, running as quickly as possible from Friedrich Ratzel's and the Haushofers' Social Darwinism, as expressed in their concepts of *Lebensraum* and *Rassenkunde* and put into practice by Germany's National Socialists. Nor was I naive about political science. As an undergraduate at the University of London I had made a weekly trek to the London School of Economics to attend political philosopher Michael Oakeshott's lectures, not long after he left Cambridge to succeed Harold Laski. Oakeshott (1962) was working on *Rationalism in Politics*, and I was struck by his ideas of the pluralism of experience and his insistence that the dominating principle of scientific thought was quantity, in contrast to that of historical thought, which was being in the past. By extension, I thought, the dominating principle of regional thought had to be "being in the region," dealing with the particular and the contingent, not the pursuit of order in the world that lay at the heart of the spatial science to which I was attracted. The search for order was central to emergent quantitative social science and became the hallmark of the "quantitative geography" movement that formed at the University of Washington.

By 1969, at the University of Chicago, I had formed working relations with a range of political scientists who shared the pursuit of general principles and the

deployment of formal analysis: at nights in the university's computer lab with Duncan Macrae Jr. coaxing the UNIVAC I to produce direct factor analyses, in my case of cross-national data sets, with results transmitted off campus to Rudolph Rummel and an international relations group that included Bruce Russett; over several years developing the university's multidisciplinary programs in urban studies, first with James Q. Wilson and later with David Greenstone and Paul Peterson; exchanging mail and telephone messages over coffee each morning with Brian Barry; and along the way sharing ideas with a vibrant and frequently contentious group of political sociologists and with Theodore Lowi.

I was often asked why political geography was so lackluster. I argued that it need not be so: political geography should be to political science what economic geography already was becoming with respect to economics – part of an interdisciplinary interchange that began in regional science and subsequently produced the lusty hybrid now billed as the “new economic geography.” But “should be” was a long distance from what was. As Ron Johnston confirms in his intervention, political geography was not widely taught in 1969 and little of the published work so classifiable had either intellectual sparkle or bite. Hence my remark, which was deliberate, designed to elicit reaction, which obviously it did. Hackles were raised, but much to my surprise it was not forgotten; rather, it appears to have festered.

Has there been change since 1969? How does the field describe itself today? During the last quarter century my career has taken me far from geography and so to update my understanding, to use one of the new verbs, I “googled” for insight. A description of political geography is posted on *Wikipedia* (<http://en.wikipedia.org/wiki/Politicalgeography> dated 7/27/09)

“Political geography is the field of human geography that is concerned with study of both the spatially uneven outcomes of political processes and the ways in which political processes are themselves affected by spatial structures. Conventionally political geography adopts a three scale structure for the purposes of analysis with the study of the state at the centre, above this is the study of international relations (or geopolitics), and below it is the study of localities. The primary concerns of the sub-discipline can be summarized as the inter-relationships between people, state, and territory.”

Then, following a reference to my “moribund backwater” remark:

“From the late-1970s onwards political geography has undergone a renaissance, and could fairly be described as one of the most dynamic of the sub-disciplines today.”

“In part this growth has been associated with the adoption by political geographers of the approaches taken up earlier in other areas of human geography, for example, Ron Johnston’s work on electoral geography relied heavily on the adoption of quantitative spatial science, Robert Sack’s work on territoriality was based on the behavioral approach, and Peter Taylor’s work on World Systems Theory owes much to developments within structural Marxism.”

It was fashionable for each subfield of geography to have its “revolution” in the mid to late twentieth century, and political geographers evidently believe that they had theirs and now are mainstreamed within geography. Johnston makes a carefully qualified case, however. He cites significant progress in British electoral geography making full use of modern methods of spatial analysis, but of the simultaneous marginalization of electoral studies by political geographers in the U.S. There are also, he says, new theoretical developments dealing with territoriality together with borrowed world systems formulations that ultimately have Marxist foundations. Julian Minghi (see his intervention in the present issue) is more upbeat not only about the state of the field in 1969, but also regarding new strengths in geopolitics centering on global modeling using qualitative data.

But somehow this doesn’t seem to add up to mainstreaming within geography, unless it is a geography in which spatial analysis has been marginalized, borrowed Marxist formulations have moved to the fore pushing aside other types of theory about national policies and international relations, and modeling is qualitative. So then I decided to ask whether political geography had developed closer ties to political science – whether research by political geographers was important enough to have an influence on the work of political scientists. I posed the question to the faculty in my political science program. They were familiar with the work in electoral geography (*Electoral Studies* is edited in my school, and two of the faculty are principals in the British Electoral Study, based at the University of Essex). Several also knew that geographers had made important contributions to redistricting methods and practice and a couple also were aware of papers that addressed problems of spatial interdependence analytically. However, the work on territoriality drew blank stares, and the international relations group was scornful of the Marxist

foundations of World Systems Theory. Mackinder, they said, was the last political geographer to be recognizably a “big thinker” – no political geographer has since displayed the breadth of view or the influence of, for example, Samuel P. Huntington (1996) in his “Clash of Civilizations”. There was little sense among the political scientists that a “new political geography” was emergent in the manner of the new economic geography. That would require a conceptual breakthrough powerful enough to draw together researchers across disciplinary boundaries. It was clear that most of these political scientists were focused inward, on their own disciplinary concerns, by and for political scientists, and it was equally clear that political geographers had similarly territorialized world views and had been quite willing to push certain topics to the margins of that territory.

My own work (Berry, 1991; Berry et al., 1998) on the borderlands of geography, economics and political science has involved time – macrohistorical processes – rather than space and has clearly been far beyond these margins, yet I have found the cross-disciplinary relationships that were essential if this research were to be successful to be both stimulating and productive. This has led me to conclude that even if political geography is mainstreamed within geography as political geographers claim, it really is but to a tributary in the wider arena of social inquiry: that only with passage into that arena will political geography produce the thinkers whose ideas shape world views. But then, as Minghi says, I had “limited expertise in political geography” in 1969 and I am sure he will say that I know even less today...

Notes

¹ A volume celebrating Kevin Cox's career is to be published in 2010 (Jonas and Wood, 2010) in which I have a chapter with Charles Pattie on his electoral geography that discusses these papers in some detail.

² John Agnew was a graduate student with Kevin Cox at Ohio State University at that time, however, and although he later wrote many important contributions to electoral geography his PhD thesis was linked to Kevin's growing interests in territorial conflict and power. Andrew Bodman had also been attracted to Ohio State by Kevin's electoral work.

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