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Review

Stuck in a mess (again): A response to Johnston, Fairbrother, Hayes, Hoare and Jones

Trevor Barnes

Department of Geography, University of British Columbia, 1984 West Mall, Vancouver, Canada BC V6T 1Z2

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ABSTRACT

A response to Johnston et al.'s criticisms of my earlier paper about the Cold War and geography's quantitative revolution that I am overly deterministic and fail to prove my case.

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1. Introduction

I am grateful for the well-crafted, useful and knowledgeable commentary on my paper (Barnes, 2008) by Ron Johnston, Malcolm Fairbrother, David Hayes, Tony Hoare and Kelvyn Jones (Johnston et al., 2008). Over the years I have gained much from Ron Johnston. My first contact with him was as an undergraduate. In spring 1978, Jacquie Burgess, then at University College, London (UCL), led our history and philosophy of geography undergraduate class around the back ways of Tottenham Court Road to the Polytechnic of Central London to hear Ron Johnston speak at a conference about new developments in geographical thought. The conference was on ideology, a topic then making its way into British geography, and deployed by its proponents in part to counter the object of our debate here, the quantitative revolution and its philosophical base of positivism. I learnt from Ron Johnston that day, and I continue to do so, including from this paper.

Johnston et al.'s big points, although they make several smaller ones, are that the quantitative revolution has multiple causes and multiple geographies. This, they argue, contradicts first my mono-causal representation focussed on the Cold War, and second the constricted geography I offer. This second criticism is linked to a critique they make of their University of Bristol, School of Geographical Sciences colleague Peter Haggett (2008) and his recent paper in *Geographical Analysis* that describes the geography of the British quantitative revolution. They argue that Haggett's portrayal of that geography pivoting on the Cambridge–Bristol axis is like my own portrayal too pinched and narrow. Instead, Johnston et al. offer an alternative in the last part of their paper which is more variegated and fulsome. I will not answer for Peter Haggett, although I will later broach the general issue of the diffusion of geography's

quantitative revolution. Specifically, my intentions in this response are first, to clarify the origins of my project; second, to address Johnston et al.'s two most significant criticisms which are that I am "overly deterministic" (p. 1801), and that my evidence fails to make the case; and third, in the conclusion, to suggest a geographical resolution to our dispute.

2. A backward beginning

I backed into the topic of Cold War geography rather than going into it front ways. The project I started more than ten years ago was to understand geography's quantitative revolution. I had caught its last gasps as an undergraduate during the mid-to-late 1970s. When not in Jacquie Burgess' class I was in Bob Bennett's (also no longer at UCL) struggling to understand single-, double-, and triple-lagged variables, spatial autocorrelation equations, the Kalman filter, and the ultimate, Box-Jenkins (and uttered in revered tones, or so I remember). When I went to America as a graduate student, it was clear that the Revolution was not on its last gasp at all, but was in its pumped-up, late-adolescent prime. My second ever conference was the annual meeting of the North American Regional Science Association at the Ambassador Hotel in Chicago where in the autumn of 1978 an evangelical Walter Isard stood up to explain the whole world in 10 multiregional models, and 25 sub-models (discussed in more detail in Barnes (2003)).

It took me many years before I felt ready to tackle what had gone on between the Central London Poly and the Chicago Ambassador Hotel. In autumn 1997 I finally started interviewing some of the pioneers of the quantitative revolution and their students to ask them what they thought they had been doing, and what they actually did.

Informing my work by then was the literature in science studies. It seemed made for my study with its supposition that

E-mail address: tbarnes@geog.ubc.ca

knowledge was produced by social and material practices, best understood by empirical, often historical case studies. My first exposure to science studies was appropriately at Bristol when I began reading Bloor's (1976) book *Knowledge and Social Imagery*. I admired the book for its chiselled clarity, its historical sensibility, the argument through example, the focus on detailed actions, the rejection of any super ordinate rationality, and the willingness to take on seemingly the hardest epistemological case, scientific knowledge. Bloor's work ostensibly offered me an understanding of human geography's own hardest epistemological case, its quantitative revolution. Later, perhaps even more useful, were Latour's (1987) writings that were dishevelled and wild compared to Bloor's, but boundlessly fecund and creative. While I drew on Latour's particular schemes and concepts, he primarily gave me a license to take risks, to think expansively, to be in a mess but not to be despondent about it. Scientists, as Latour continually demonstrated, were themselves in a mess, stuck in various muddles, embedded within complex settings of a particular society, culture, politics and geography, never pulling off the "god trick," the "vision from everywhere and nowhere" (Haraway, 1991, p. 191), but nonetheless sometimes achieving (literally) earth-shattering results.

Bloor and Latour helped me to represent and make sense of the practices of US-based pioneering quantitative geographers that I heard first-hand in interviews (reported in Barnes (2004)). Nevertheless, in listening to them, I often felt I came into their stories half-way through. I mean that although my interviewees were scrupulous in telling me their stories from the beginning, no one reflected on the historical conditions that enabled their narratives to begin as they did. I wanted to know the conditions that made the lives of quantifiers possible, and by making them possible, understand how they were then brought about (see Deleuze and Parnet (1987, p. 96)). Haraway's work (1997), and especially Pickering's (1995), that I later read, provided the missing beginning, the instigative role of the military-industrial complex ("technoscience" in Haraway's (1997, p. 15) terms, "the World War II" regime in Pickering's (1995, pp. 234–46)). The start was still messy and tangled – Haraway (1994) uses the metaphor of "Cat's cradle," Pickering (1995) "the mangle" – but it was a start nevertheless.

3. "Overly deterministic" and "unproven"

Johnston et al. make two main charges: that I am "overly deterministic" (p. 1801) as a result of my "adopting something... akin to that of structural Marxism" (p. 1801); and that "the evidence is not there," rendering my "case,... at best,... unproven" (p. 1802).

3.1. "Overly deterministic"

I must admit no one has ever accused me before of being "akin" to a structural Marxist. I suppose it is a case following Charles Peirce that everything that might happen, will happen. One of the reasons I was drawn to both Haraway's and Pickering's works is precisely because they are *not* mono-determinists. Moreover, and ironic given Johnston et al.'s argument, one of the reasons Haraway is not a mono-determinist is because she draws on structural Marxists like Althusser. Althusser's (1969) essay on "Contradiction and overdetermination" develops Freud's notion of overdetermination, the idea that everything determines everything else. By definition that essay recognises multi-determinism, an end I would have thought shared by Johnston et al. Certainly it is by me.¹ In this sense, maybe we are all structural Marxists together.

¹ There is that dreaded Althusserian phrase, "determination in the last instance," but Althusser (1969, p. 113) makes clear that "the lonely hour of the 'last instance' never comes."

Johnston et al. charge me of being "overly deterministic". I am presuming by that they mean "mono-deterministic." After all, everything is determined in one way or another, even if it is by chance. Specifically, they think that I use "Cold War" as a single essential cause like Duncan and Ley think that structural Marxists use economy as a single essential cause (Johnston et al. p. 1801). Gibson-Graham (1996) have brilliantly demonstrated, though, that the economy is no single, essential cause, and in the same way neither is the Cold War. Johnston et al.'s depiction goes something like:

Economy → Social Consciousness [= Marx]

Cold War → Geographical Consciousness [= Barnes]

Pickering's metaphor of the mangle on which I explicitly drew in the paper, however, rejects mono-causality, and indeed, the very notion of traditional causal explanation.² Pickering (1995, p. 24) writes: "The world of the mangle lacks the comforting causality of traditional physics or engineering, or of sociology for that matter, with its traditional repertoire of enduring causes (interests) and constraints." Instead, the mangle operates through "a dance of agency, and the dialectic of resistance and accommodation" (Pickering, 1995, p. 24). As new entities like US geography enter the Cold War fray, a complex two-step goes on, with US geography making one move, the Cold War making another, with the final result emergent and unpredictable. Rather than the Cold War as sealed and static, a black box imbued with causal force, it is always in process, heterogeneous, changing as new elements enter, mixing with existing ones, producing new patterns and effects. Consequently, there is no mono-causality and maybe no traditional causes at all. What is generated is precisely messiness and diversity, seemingly the ideals desired by Johnston et al.

Johnston et al. may say though that I still privilege the Cold War (however it is connected to human geography). If the Cold War is conceived as porous and emergent (just like Gibson-Graham's conception of the economy), then yes I believe it was a central assemblage during 1950s and 1960s. That was the life that was. Consequently, US geography became mangled with it, just as so many other entities did, changing and being changed. It was there at every turn. It could be ignored, it could be resisted, but as I tried to show in my paper there was significant accommodation as well that eventually took the form of the quantitative revolution.

My thesis, then, is that because Cold War militarism was central, US geography necessarily interacted with it, in the process moving geographical knowledge towards modelling and quantification. Given this argument, there are three potential strategies of rebuttal Johnston et al. could have pursued against my argument.

First, they could have claimed that Cold War militarism was historically not central. That would cut the ground from under my argument, rendering any mangling with geography small beer. This line of attack, however, was not Johnston et al.'s, with them providing neither contradictory arguments nor evidence against my claim of the Cold War's militarism's centrality.

Second, they could have said that even though the Cold War was central in 1950s and 1960s America, there is a separation between knowledge production in the academy and the larger world outside (cast in my terms this would be the argument that academic knowledge is not the kind of thing that is caught in the mangle). There are shades of this argument colouring their paper. It is partly found in Johnston et al.'s seemingly dismissive characterization of contextual histories as Marxist and "overly determinist"

² Andrew Pickering gave a wonderfully lucid account of his idea of the mangle in a radio interview to the CBC programme *Ideas*, and downloadable at: <http://www.cbc.ca/ideas/features/science/index.html#episode4>.

(p. 1801).³ It is also seen in their characterisation of alternative UK geographies of the quantitative revolution lying outside the Cambridge–Bristol axis (pp. 8–11). In Johnston et al.'s account, that revolution is not connected to any events or movement outside of the discipline. It is as if the discipline pulled itself up by its own quantitative boot straps, resulting in the scattered geography of calculative accomplishment described. If their position is that knowledge production and the larger context in which it occurs are disconnected, then I and the larger science studies literature on which I have drawn would fundamentally disagree. Moreover, it would go against the tenor of particularly Johnston's own previous writings found, for example, in his excellent *Geography and Geographers* that asserts "the study of a discipline must be set in its societal context" (Johnston and Sidaway, 2004, p. 23).

Finally, they could have argued that while the Cold War was important, and while social context played a role in disciplinary production of knowledge, war and the military happen not to be elements of the context that make a difference. Johnston et al. make no general argument for dismissing the role of war and the military in influencing geographical thought, however, and the historical studies that exist in the discipline document an inextricably tight relationship between the two (Driver, 2001). Indeed, a case could be made that the very academic discipline of geography emerged exactly because of its potential contributions to war and the military. In this sense, what occurred in the Cold War was simply another twist in an ongoing gyrating saga.

The point is that of the three potential avenues of rebuttal to my argument, Johnston et al. explore at best one of them, and then not really. To use their phrase, the case against me remains "unproven."

3.2. "Unproven"

But Johnston et al. think it is me who presents the unproven case. My strategy in making the argument was to focus on the lives and histories of three individuals. Clearly, they are not any old three individuals, but individuals who played key roles in shaping US geography through the late 1950s and early 1960s. They influenced especially graduate students who would take up the movement in what Johnston et al. rightly recognise as a small discipline, and consequently relatively easily swayed. I worked my arguments through the lives of these individuals precisely because it would enable me to make the strongest case. By following the three around, you see clearly how they undertook their various accommodations. This is the injunction of the mangle, and more generally science studies: to be specific and concrete rather than to rely on abstract syllogisms. Latour (1987) subtitles his book, "How to follow scientists and engineers around society." That was what I was trying to do: to follow Waldo Tobler, William Garrison, and Arthur Strahler around Cold War American society, and to gauge its impress.

Johnston et al. did not think I did enough following. Or at least in the following I did, I never managed to capture the "Ah ha" moment when my thesis was "proven." They suggest instead I should have written a counter-factual history, imagining the history of geography if there was no Cold War. Would that have really satisfied them, however? Counter-factual histories by their very definition are made up, leading Michael Postan famously to say, "the

might-have-beens of history are not a profitable subject for discussion" (quoted in Gould (1969), p. 195). Johnston et al.'s (p. 1802) own ventures into counter-factual history are interesting, but what do they prove? They suggest that without the Cold War, "some sort of revolution would have happened" (p. 1802), but of what kind? And how, when, and where? I think it is better to argue about the known rather than the unknown, or that ultimate Rumsfeldian nightmare, the "unknown unknowns, the ones we do not know we do not know."

That said, I am mindful of the difficulties in connecting individual lives and larger events like geography's quantitative revolution, like the Cold War. In part this is why I began and ended my paper with Don DeLillo's epic *Underworld* that wrestles exactly with this issue. Johnston et al. are right to remind me that lives are complex and messy, at times contradictory and opaque, with peoples' motives hidden even to them. Following people around, though, I believe is vital to constructing geography's histories (see Powell (2007), who also follows the same tack in his work on the history of post-war Arctic geographical research). But it is hard. There is always the temptation, one maybe I failed to resist, to impute motives that were not there, for your subjects to dance the two-step to your tune and not to their own.⁴

4. Conclusion

Johnston et al. do everyone a service by emphasising the messiness of geography's practices and thus its history. But it is not free-for-all anarchy either, where anything goes, and by merely wishing for a new kind of geography you bring it about. There are larger forces at work, interacting with individuals. That is what the mangle is all about. While the outcome is contingent, mixed up, even messy, it is made up of what was fed in at the beginning. With computers, hard science, quantification, interdisciplinary research teams, gobs of military money, and Cold War geopolitical ends entering the mangle, it was always likely that as geography interacted with them there would be a quantitative not a social revolution.

But of course this was the mangle in the United States. In Britain it was a different story because different elements were fed into its rollers. Clearly there were some linkages where parts of the US story enter into the UK narrative. Richard Chorley is one tie, and Haggett (in Browning (1982, p. 47)) has said how influenced he was in writing *Locational Analysis* (Haggett, 1965) by what had happened in America. The significance of the second half of Johnston et al.'s paper is in providing an elaboration of the local elements entering the UK mangle, and the messiness that eventuated. I am happy to be stuck in this mess with Johnston et al.

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³ There is slippage in Johnston et al.'s characterisation of Marxism and contextual history. On p. 1801 they imply that my essay "follows the line" of Marxist contextual history. But then later (p. 1802), they imply I am doing something else in following a strategy "akin" to "structural Marxism." Why is not structural Marxism part of Marxism (I am sure Althusser thought he was a Marxist)? And in any case why are Marxism and contextual history joined? Does everyone who carries out contextual histories have to be a Marxist? There needs to be more precision in Johnston et al.'s definition of terms, and in their postulated relation one to another.

⁴ Gross's (2007) *Richard Rorty: The Making of an American Philosopher* that I read after completing my paper explores the complex relation between biography, in this case Richard Rorty's, and the larger social context in which his life unfolded. Gross wants both to recognise individual agency, and larger social forces that he does by developing what he calls a "new sociology of ideas" (Camic and Gross, 2004).

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