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# “Not Only . . . But Also”: Quantitative and Critical Geography

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This article argues that the binary between quantitative and critical geography is pseudo rather than real. The duality arose, the article suggests, because of the peculiar postwar intellectual history of human geography in which the critical approach followed the quantitative one. Accordingly, for internal sociological reasons, it was necessary for the critical approach to excise everything that went before in quantitative geography. In contrast, the article argues that there is no inherent contradiction between critical and quantitative approaches, and indeed there are good reasons to join them. The article makes its argument by suggesting, first, that Marx, the ultimate social critic, was sympathetic to mathematics in his own work; second, that this fact was lost to the radical geographers of the late 1960s and early 1970s because of their desire to distance themselves and ultimately to overthrow the dominant quantitative approach; and finally, that the supposed binary of quantitative and critical geography might be dissolved by engaging in what Galison (1998), the historian of science, calls “trading zones.” **Key Words:** critical geography, Marx, quantitative revolution, trading zones.

**M**y UK undergraduate geography curriculum of the mid-1970s was schizophrenic. We pored over Harvey’s (1969) *Explanation in Geography*, and our lecturers took us to the leading edge of British quantitative geography: 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). It is hard to believe that I was sufficiently obsessed by Box–Jenkins that I got up very early one Saturday morning to go to the college library to stare until my head hurt at their ARMA and ARIMA family of forecasting models. This was half of my undergraduate education.

Then there was the other half. It was learning that quantitative methods were flawed. They had their time in the limelight, but now we needed instead to be social critics. We read Harvey mark II (or “Marx II” as we said), single issues of *Antipode* in vibrantly colored covers locked away behind the library counter, and most shocking of all, a ravaged private copy of Olsson’s (1975) Michigan Geographical Publication No. 15, *Birds in Egg*. It was owned by a postgraduate student with an unlikely research interest in London’s double-decker buses, their routes, and their timetables, who one day came to one of our classes “to disturb us.” He did, and not only by telling us about the inner workings of London transport but by reading from Olsson.

Missing from our curriculum was how the two halves fitted together. In my final year, I took a seminar on geography and philosophy. One of the topics was the ostensible opposition between quantitative and radical geography (and represented by the dyspeptic Berry–Harvey debate in *Antipode* that recently had made its way into the locked library cabinet; Berry and Harvey 1974). Someone in that seminar said, and drawing on the name of a well-known Peter Cook and Dudley Moore BBC television show of the time, “Why can’t it be ‘Not only . . . But also?’” This is my question in the remainder of this article.

My contention is that the opposition between quantitative and critical geography is pseudo rather than real (and recognized, for example, by Kwan 2004). I argue that to understand why quantitative and critical geography have been portrayed as antagonistic (as they were in my seminar in the mid-1970s and continuing to the present), it is necessary to pursue intellectual history both inside geography as well as outside.<sup>1</sup> Scrutinizing that history, I suggest, makes it clear that there is no inherent contradiction between mathematics and critique but also shows why such an erroneous opposition was accepted within human geography in the first place.

By quantitative geography I mean the movement that began in the late 1950s concerned with systematically applying mathematical forms of reasoning and formal statistical methods and techniques to geographical problems (and that also provided the spark for what later became GIScience).<sup>2</sup> My argument is that the

quantitative approach came to geography relatively late, primarily by way of economics. That particular route, I suggest, tainted the approach for critical geographers, even though, as I also argue, there is nothing inherently anticritical about mathematical forms of representation.

Critical geography is now a variegated and diffuse set of perspectives including feminist, postcolonial, post-Marxist, antiracist, antiglobalization, and queer theory.<sup>3</sup> It is not possible to deal with all these different theories in an article as short as this one, but I do not think it is necessary either. My suggestion is that critical geography’s aversion to quantitative geography originated during the early 1970s in the encounter between quantitative geography and the discipline’s first systematic critical approach, radical geography (and from which contemporary critical geography later emerged). It was that initial encounter, and as much sociological as intellectual, that poisoned the well of a quantitative approach and established the putative binary. For these reasons, this article focuses on the past rather than the present, and the comparatively narrow radical geography rather than the contemporary broad critical geography.

The article is divided into three brief sections. First, by reviewing Marx’s relation to mathematics I make the argument that mathematics is not innately antithetical to a critical approach. Marx is possibly the critic of all critics, and his writings were inspirational for radical geography. In spite of his own limited mathematical acumen, however, Marx consistently favored a quantitative approach to social analysis. Mathematics and Marxism were never antonyms. Second, this fact was lost when the critical approach was introduced into the discipline during the 1970s as radical geography. My focus is on Harvey, the key developer of radical geography during this period, who I argue partly contributed to producing the binary that this Focus Section attempts to overcome. Finally, to help to break that dualism, I suggest drawing on work in science studies, particularly Galison’s (1998) inquiry into twentieth-century microphysics that shows how similar blockages in that field were circumvented by the construction of “trading zones” and trading “pidgin languages.” It represents yet another argument, along with others made by the articles in this Focus Section, for the binary to be dissolved,

for geography to be not only critical, but also quantitative.

### Marx and Mathematics

Although Marx is perhaps best known for rousing one-liners—“Workers have nothing to lose but their chains,” “Workers of the world unite,” “Religion is the opiate of the masses”—he also penned less riveting but more exacting prose:

Consequently  $u$  and  $z$  as mere names, as symbols of functions of  $x$ ; therefore as well only the general forms of this ratio of dependence (*Abhängigkeitsverhältnis*):

$$\frac{u_1 - u}{x_1 - x} = \frac{f(x_1) - f(x)}{x_1 - x},$$

$$\frac{z_1 - z}{x_1 - x} = \frac{\varphi(x_1) - \varphi(x)}{x_1 - x}$$

is generated immediately by the processes of taking the derivative. (Marx [1968] 1983, 19)

Marx wrote nearly a thousand pages like this beginning from the late 1850s and continuing almost until he died in 1883 (Marx [1968] 1983, vii). The pages are a combination of notes made from mathematical works he read and an original study of the history and nature of differentiation. Marx was intent especially on “tear[ing] off the veil of mystery” (Marx [1968] 1983, x) that surrounded “quantities which are used for calculating the infinitely small—the differentials and infinitely small quantities of variable orders” (xi). To do so he used the same method he applied to tearing off all “veil[s] of mystery”: dialectical materialism. The preceding equation was part of that unveiling. It represented one element in Marx’s revised history of mathematical differentiation told “as a particular case of [the] dialectical law of ‘negation of a negation’” (Smolinski 1973, 1194; see also Kol’man [1968] 1983).

Not that it was easy for Marx. Mathematics was not in his intellectual marrow, or as he put it, he was “never on intimate terms” (Marx, quoted in Smolinski 1973, 1197). In 1858, at age thirty-eight, as he was preparing the manuscript for the first volume of *Capital*, he wrote to Engels: “I am so damnedly held up by mistakes in calculation in the working out of the economic principles that out of despair I

intend to master algebra promptly. Arithmetic remains foreign to me” (Marx [1968] 1983, viii). Consequently, many of the thousand pages of Marx’s mathematical manuscripts are redrafts, and even redrafts of redrafts, as he struggled with the obstinate complexities of differentiation. He continued with the project of learning mathematics, however, going back to original sources by Newton, Leibnitz, d’Alembert, and Lagrange, studying in the Reading Room of the British Museum but also in libraries at the Universities of London and Cambridge as well as at the Royal Society (Marx [1968] 1983, xxiv).

He channeled so much effort into the task because he thought mathematical or analytical forms of reasoning were central to his larger project. They were central because mathematics was a component of science, and he thought of his work above all as a science, a science of society. As reported by Lafargue, “Marx believed . . . ‘a science is not really developed until it has learned to make use of mathematics’” (quoted in Kol’man [1968] 1983, 220).

More specifically, Marx thought mathematics necessary to represent and understand the laws of motion of capitalism. Commodity values, rates of exploitation, the falling rate of profit, and crises of overaccumulation—the very conceptual furniture of his revolutionary science—needed to be expressed formally, certainly numerically, preferably algebraically, and if possible in terms of the mathematics of calculus. In fact, Chapter III of the third volume of *Capital* according to Engels ([1894] 1967, 12–13) was to be fully mathematical, “dealing with the relation of the rate of surplus value to the rate of profit in the form of equations.” Marx never lived long enough to see Volume III completed, so his chapter never came to be, but what is taken as a draft of that chapter exists and was analyzed by Smolinski (1973, 1195–98). It utilized the full array of Marx’s mathematical arsenal, including first-order differential equations, algebraic manipulation, and, admittedly Marx’s weakness, arithmetical calculation, including horrendous multiplications in the calculation of prices and involving fractions like  $209\frac{21}{71}$  and  $172\frac{25}{63}$  (neither of which he successfully performed; Smolinski 1973, 1197).

The upshot is that although Marx’s mathematical acumen might be questionable, his belief in and willingness to use mathematics are

not. Smolinski (1973, 1198, 1200) concludes his study:

For all his lack of skill and the crudeness of his methods, Marx uses mathematical reasoning not only as a shorthand notation à la Alfred Marshall, but as a means of making explicit the implication of his initial assumption and of deriving new propositions pertaining to economics. . . . His failure to apply advanced mathematical methods in his economic writings maybe attributed to a variety of reasons, but certainly not to the belief that such methods are inapplicable to the study of economic phenomena.

The larger point is that in Marx's work there was no binary between the analytical and the critical. Marx might be the all-time, ultimate critic both in terms of ideas and also on the ground. Hundreds of millions of peoples' lives were changed because of Marx's critical impulse, but that critique was pursued partly through the analytical. It was not either-or (i.e., the binary) but both-and ("Not only . . . But also").

Of course, this did not stop Marx from being interpreted as laying at only one end of the binary, the critical, and the opposite end to the quantitative. This one-sided interpretation began early on in 1896 when the Austrian economist von Böhm Bawerk ([1896] 1949), in his extended essay *Karl Marx and the Close of His System*, excoriated Marx's mathematics, implying that his scheme was incapable of formal rigor.

By 1930 in the Soviet Union it was a different story. It was not that Marxist economics could not be quantitative but that it should not be quantitative. Stalin believed twinning mathematics and Marxist economics made it "the most reactionary brand of bourgeois economics" (Kol'man, quoted in Smolinski 1973, 1202). Consequently, those associated with that brand, including Nikolai Kondratiev of long-wave fame, were purged along with their mathematical models (Kondratiev was charged as a "kulak professor" in the 1938 Moscow show trials and executed the same day his guilty verdict was handed down).

In Cold War America, it was different story again, with Marxist economics shown up because it was not mathematical like "bourgeois economics," and which, under Paul Samuelson's contributions, especially his *Foundations of economic analysis* (Samuelson 1947; see also

Samuelson, 1998), had upped the mathematical ante, making economics resemble a branch of physics (Mirowski 2002). In fact, Samuelson (1962, 12) initially was one of the masters of the put-down, calling Marx in his 1961 presidential address to the American Economic Association "a minor post-Ricardian," and Marxian economics "a not uninteresting precursor of Leontief's input-output."

In human geography there was yet a different twist. As discussed in more detail later, after holding out against formalism until the late 1950s, first a trickle then a surge of human geographers took up mathematics and formal theory in the quantitative revolution (Barnes 2004a). In part, the move in geography to spatial science was a mimicking of the developments in economics, with that discipline held up as a paragon, with some of its abstract theory and mathematics transferred wholesale. Whereas Samuelson's iconic *Foundations* continues even now to define economics as an intellectual style within only ten years or so of the start of the quantitative revolution spatial science was contested by "radical geography" that attacked its mathematical formalism, presenting Marxism as the critical (nonmathematical) alternative.

### Radical Geography and Mathematics

To understand this fissure that opened between the quantitative and the critical in human geography one must go back further in the story to the emergence of the discipline's quantitative revolution. That revolution began in the mid-1950s at selected sites in the United States, initially the University of Washington at Seattle and the University of Iowa in Iowa City. Later it spread to the University of Chicago, University of Michigan, Northwestern University, and Ohio State, and, in addition, there were European centers such as at the Universities of Cambridge, Bristol, and Lund (Barnes 2004a).

Geography's quantitative revolution was part of a larger move that occurred in the U.S. humanities and social sciences toward a "new rigorism" (Schorske 1997) generated by the Cold War and the rise of the military-industrial-academic complex (Barnes and Farish 2006). Neoclassical economics of the

Samuelson kind was front and center in this move (Mirowski 2002), and accordingly some geographers within the quantitative revolution turned to it for theories and techniques (especially those linked to the allied movement of regional science, and also neoclassical and formal in its sympathies; King 1979; Barnes 2004b).

Neoclassical economics was the enemy of Marxism, however. It was “bourgeois economics,” justifying the status quo. Further, from the beginning neoclassicism was mathematical, based on the equations of energetics, a mid-nineteenth-century branch of physics (Mirowski 1989). Forces modeled mathematically in the physical world were metaphorically redescribed by neoclassicism as economic relations. Early neoclassical economists such as William Jevons, Carl Menger, and Léon Walras substituted utility for energy in the constrained maximization equation of the physicists and made the rationality postulate the equivalent to the physicist’s principle of least effort (Mirowski 1989). Samuelson (1947) later increased the mathematical stakes in his *Foundations*, but it was still within the same neoclassical game of mathematical formalism introduced eighty years earlier.

Given the relation between geography’s quantitative revolution and neoclassical economics on the one hand and the historical antagonism between Marxism and neoclassicism on the other, it was not surprising that when radical geography first appeared it disparaged not only neoclassicism but the quantitative revolution as well. Such a position was clearest in Harvey’s writings, and I focus on them because they are both so lucid and so central to the radical geography agenda (Castree and Gregory 2006). Having advocated for the “other side” during the 1960s (i.e., for quantitative methods), Harvey (1972b) dramatically changed stripes, making a powerful statement against them in an essay that appeared in *Antipode*, “Revolutionary and Counter-Revolutionary Theory in Geography and the Problem of Ghetto Formation.”

That essay was an attack on market-based theoretical accounts of urban land use that began with the Chicago School and culminated with the formal mathematical models of William Alonso and Richard Muth that were “built on neo-classical marginalist principles”

(Harvey 1972b, 8). The problem, however, was that Harvey cast his critical net wider than was warranted, going beyond the specific critique of the Alonso–Muth neoclassical formulation to include quantitative methods and formal theory more generally. Harvey contended that first, quantitative methods in geography were at the end of their usefulness:

[Geography’s] quantitative revolution has run its course and diminishing marginal returns are apparently setting in as . . . [it] serve[s] to tell us less and less about anything of great relevance. . . . There is a clear disparity between the sophisticated theoretical and methodological framework which we are using and our ability to say anything really meaningful about events as they unfold around us. . . . In short, our paradigm is not coping well. It is ripe for overthrow. (Harvey 1972b, 6)

Second, in response to a series of commentaries on his paper, he further suggested that formal theory was “counter-revolutionary,” and defined as:

A theory which may or may not appear grounded in the reality it seeks to portray, but which obscures, be-clouds, and generally obfuscates . . . our understanding of that reality. Such a theory is usually attractive (and hence gains currency) because it is logically coherent, easily manipulable, esthetically appealing, etc., but it is in fact divorced from the reality it purports to describe. (Harvey 1972a, 41)

This was where the die was cast. Harvey linked the “logically coherent,” the “easily manipulable,” and the “esthetically appealing,” that is, formal theory, with irrelevance. In fact, worse than irrelevance because “attention [was diverted] from fundamental issues to superficial or non-existent issues” (Harvey 1972a, 41). This is what made it “counter-revolutionary theory,” and consequently anathema for any self-respecting radical geographer to pursue.

Harvey’s general argument, then, runs something like this: Neoclassicism is quantitative and mathematical; radical geography is against neoclassicism; therefore, radical geography should be against the quantitative and mathematics. Such an argument is flawed, however. Although its premise might be right that neoclassical economics is quantitative and mathematical, the conclusion does

not follow. You can do quantitative methods and mathematics without necessarily doing neoclassical economics. The two sets of pursuits are separate, which the conclusion fails to recognize.

So, on logical grounds Harvey (and radical geography) was wrong to reject mathematics, producing a false binary between critique and the quantitative. Historically, however, it was wrong too. Marx himself, as already argued, was sympathetic to mathematical forms of presentation. In addition, a number of the theorists invoked by geography's quantitative revolution, and indeed some of its active participants, were radical social critics who developed their formal geographical theories partly in the name of progressive social and political change. This requires elaboration.

One of the bulwarks of the quantitative revolution was locational analysis and referring to the logically and mathematically rigorous investigation of the "spatial arrangement of phenomena . . . and related flow patterns" (Johnston 2000, 464). As a pursuit it was associated with German location theory and included works by Johann von Thünen in the first half of the nineteenth century and Alfred Weber and Walter Christaller in the first part of the twentieth century. As a body of work it was relentless in its use of mathematical and numerical methods (Barnes 2003).

Each of these three locational theorists, however, intended his mathematics for politically progressive ends, to forward social critique. Von Thünen was concerned with "all of those evil conditions which sicken the social situation of Europe" and believed that his land use model, including its calculation of the frontier wage, would be a just remedy (von Thünen, cited in Dempsey 1960, 327).<sup>4</sup> Weber was appalled by the conditions of sweated labor in the turn-of-the-century German clothing industry and wrote his dissertation that included his famous locational triangle to counter what he thought was an "unexampled barbarism" (Aron 1964, 113). Finally, Christaller, although he could be confused and contradictory, was for most of his life a socialist and sometimes a communist, which caused him to go to France temporarily in the early 1930s because of fears about his personal safety in the wake of the rise of Nazism, and then at the end of his life, to

be refused a U.S. immigration visitor entry visa for a valedictory speaking tour of America. He saw no contradiction between his mathematical and numerical presentation and his critical politics.

Further, even within geography's own centers of calculation, several of the revolutionaries saw no contradiction between their quantitative methods and their critical political ends. At the very cradle of the quantitative revolution, the University of Washington, Seattle, two of the original "space cadets," Richard Morrill and William Bunge, were both on the left. Morrill, in remembering that period said, Bunge "and I were the two most radical. I was not quite as radical as he was. . . . He was a member of the Communist Party. I was a member of the Socialist Party and founder of SDF and all of those things here on campus" (Morrill, interview with author 1998). Morrill's academic work on transportation models, however, fully upheld "the idea of formal theory that could be quantifiably expressed in mathematical terms, [that] was the goal" (Morrill, interview with author 1998).

Bunge was even more passionate about both politics and mathematics. He attended so many political demonstrations that he bought a suit dedicated for wearing on such occasions, and his PhD dissertation completed under Bill Garrison, and published as *Theoretical Geography* (Bunge 1966, xi), was the most evangelical written by any of the space cadets in its commitment to "modern mathematical geography." His belief in the power of formal theory, and geometry in particular, continued after he was further radicalized (if that was possible) following the beginning of the American bombing in Vietnam and the events at Selma, Alabama (Bunge 1979). By the early 1970s, Bunge was genuinely confused when Marxists such as Harvey began "to purge the quantitative-theoretical gains of the sixties" (Bunge 1973, 33). For Bunge, quantitative methods and formal theory were the means to achieve critical aims, and if they had not done so yet, it was only because they were inadequately pursued, not because they were counterrevolutionary; indeed, they were one means for forging revolution.

Even after Harvey's statement, some geographers during the late 1970s and 1980s, especially within economic geography, tried to join

both critical and mathematical perspectives. Allen Scott's (1982) work was formative, combining the chiseled linear production equations of political economist Piero Sraffa with at first a von Thünen model to examine land rent, and later, manufacturing clusters forming new industrial spaces. Eric Sheppard and some of his students including Paul Plummer, Claire Pavlik, and myself, during the 1980s drew on the highly mathematical formulations of analytical Marxism associated with the writings of John Roemer (Sheppard and Barnes 1990). Likewise, Michael Webber and his students, especially David Rigby, deployed Emmanuel Farjoun and Moshé Machover's stochastic Marxist theoretical approach to present an original rigorous statistical empirical assessment of postwar Western capitalism, especially in Canada (summarized in Webber and Rigby 1996). All of these works upheld a critical impulse, but all of them did so by using a formal and numerical vocabulary and tools of analysis. By the end of the 1990s, such work had largely dissipated in part because economic geography was increasingly taken with qualitative methods (Barnes et al. 2007) and partly because economic geographers were less trained in formal analysis, possessing neither the skills to write such work nor the mathematical literacy to read it.

In sum, I tried to identify the moment when the binary was created between quantitative and critical geography. Pivotal, I suggested, was when Harvey rejected the quantitative revolution, and accepted "revolutionary theory." At that juncture, either—or emerges. On logical grounds, however, Harvey's argument is shaky, and in practice, it is widely contradicted. The discipline is littered with examples of geographers, as well as honorary geographers, who, although they adhered to a critical sensibility (often a radical political one), at the same time effectively used quantitative methods and mathematical theory. I am not suggesting Harvey did anything underhand. As he said in his paper, "all of us were involved" (Harvey 1972b, 3). It was a brilliant maneuver, but it had larger consequences, setting first radical geography and then later critical geography on a particular intellectual trajectory, defined by a binary that was hard to shake, stubbornly holding to the present.

## **Beyond the Binary?**

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Consequently, although the critical and the quantitative are not intrinsically in opposition, in human geography they are rarely presented together, and even when they are, the bond does not seem robust. Indeed, for Kwan (2004, 756), the relation is of "mutual indifference and the absence of dialogue." So, how can people be encouraged to speak to one another across this divide that, although artificial, acts as no less a real barrier?

Several suggestions have been made (and reviewed by Kwan [2004], which includes her suggestion of hybridity). Mine derives from science studies, an interdisciplinary literature concerned in part with how scientific knowledge is forged from the divergent elements that enter it. Galison's (1998) work on trading zones is especially germane based on his epic study of the interaction of different types of scientists engaged in the problems of detecting, recording, and theorizing microparticles within the history of twentieth-century physics.

Galison (1998) uses the notion of a trading zone drawn from economic anthropology. It is used in that field to understand how exchange occurs between societies with very different values and meanings, objects of importance, and languages. In spite of these dissimilarities, exchange still proceeds, fostered by the development of a pidgin trading language, an intermediary between the trading parties' parent languages. Neither fully dependent nor independent of those languages, pidgin is betwixt and between both (it is a hybrid to use Kwan's vocabulary). The development of a trading language is a first step in dialogue and the gaining of mutual interest.

Galison (1998) works through the idea of a trading zone by studying three different groups of physicists—theoreticians, experimentalists, and instrumentationists—as they deal during the twentieth century with a variety of issues—technological, experimental, metrological, mathematical, and conceptual—around the recognition and understanding of microparticles. Each of the three groups hails from a different intellectual tradition, speaks a different language, mobilizes

different methods, and holds different objects in esteem. At specific moments, though, each is willing to engage in trading, to improvise pidgin, and to set aside their differences to work together. Galison's prime example is the serendipitously established Rad-Lab at MIT during World War II that engendered cooperation and trading among very different scientific partners (Buderi 1996). No superordinate principle emerged. "Quite the opposite, . . . trade . . . [was] coordinated . . . without reference to some external gauge" (Galison 1998, 803). As Galison shows, though, in the end common purposes were nevertheless achieved and some dazzling accomplishments realized.

My suggestion is that to break the impasse between critical and quantitative geographers, trading zones should be constructed and new hybrid vocabularies developed. So, rather than a single large solution applied all at once everywhere, my proposal is for piecemeal change, to take place at particular sites where there are opportunities for trading. Afterward, the parties may go back to their respective corners, but the hope is that the very interaction makes a difference to both, and something new and creative, albeit as yet undefined, eventuates.<sup>5</sup>

Of course, in part this Focus Section, and the five sessions at the 2007 San Francisco annual meeting of the Association of American Geographers on which it was based, are about achieving just such an end. They attempt to create a trading zone, to construct hybrid vocabularies, and to point to novel solutions that, although they might not work in the end, nevertheless should be vigorously pursued until exhausted. If this is the project, one consequence should be less concern with epistemologies and ontologies, talk of which usually divides. Instead, following Galison's study of twentieth-century physicists, the task should be defined concretely and specifically, with well-defined ends. Hybrid languages are pragmatic instruments for achieving particular goals. If interrogated under the harsh light of ontological and epistemological theory, their sharpness can be blunted, causing disengagement. Whereas what we most desperately need in the relation between the critical and the quantitative is keenness, engagement, and common purpose. ■

## Notes

- <sup>1</sup> Kwan (2004, 756) says "the division [between the two camps] seems deeply entrenched . . . and the rift seems to have magnified over time."
- <sup>2</sup> The quantitative revolution is reviewed by Barnes (2004a). The relation between the quantitative revolution and GIScience is discussed by Chrisman (2006) and Barnes (forthcoming).
- <sup>3</sup> There is no definitive history and definition of critical geography, although there exist Painter's (2000) *Dictionary of Human Geography* entry, Blomley's (2006, 2007, 2008) progress reports in *Progress in Human Geography*, and arguably a house journal, *ACME: An International E-Journal for Critical Geographies* (<http://www.acme-journal.org>). It is possible that by its very constitution and rationale such definitiveness is oxymoronic, making any general claims about the movement necessarily fraught. This is another reason for this article's focus on a narrower set of historical issues.
- <sup>4</sup> Barnbrock (1974), and subsequently, Harvey (1981), recognized the political radical sentiments of von Thünen.
- <sup>5</sup> Such an approach is based on the philosophical pragmatist conviction that hope is best given by pluralist conversation and involving many parties who may well disagree with one another. As the pragmatist Oliver Wendell Holmes wrote, given "all life is an experiment . . . we should be eternally vigilant against the attempts to check the expression of opinions that we loathe" (Holmes, quoted in Menand 2001, 430).

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