

Methods matter: transformations in economic geography

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Introduction

In 1983, Doreen Massey invited a small group of industrial geographers to a workshop at the Open University (OU) in Milton Keynes to explore the connections between theory, policy, and method in their work. The participants, a mix of established and up-and-coming British researchers, were called upon to talk about not only *what* they were doing, but *how* and *why* they were doing it, their theory and their methods. While a consensus solidified around the *what* of economic geography – the problems of manufacturing job loss, the stubbornness of regional divides, the apparently inexorable decline of the British economy – little agreement emerged about the *why* and *how*, prompting a lively, albeit inconclusive, debate. For all the sense of a shared project, there remained marked differences in research methods. In *Politics and method*, the resulting book and inspiration for the present volume, the editors reflected: “There were points, certainly, in the debate where an impasse was reached, but as someone said at lunchtime, voicing the thoughts of us all, ‘I wonder why we’ve never done this before?’” (Massey and Meegan, 1985a:4)

In many ways, our book is an attempt to resume that lunchtime conversation. But it is a difficult task, running against the disciplinary grain. *Doing it* rather than *talking about it* has been the dominant intellectual culture. The immediacy and pressing nature of issues confronting economic geographers seem to militate against methodological talk. Even Massey and Meegan, writing within the eye of the storm that was deindustrialization and restructuring, hesitated on the threshold of their own book: “Time ... seems

too short for self-conscious reflection on the nature of the relation between theoretical, methodological and policy perspectives” (Massey and Meegan, 1985a:2).

But their conclusion was that methodological reflexivity was nevertheless indispensable. It should not be *either* politics *or* method—opting either for the study of industrial restructuring or “self-conscious reflection”—but both: politics *and* method. Tackling urgent political issues like deindustrialization and restructuring required appropriate methods, required serious talk. The earlier, brief flirtation of British economic geography with formal neoclassical theory and methods (regional science) was over, and it had since (re)turned largely to empirical analysis of industrial location, relying on orthodox data sources like censuses and surveys. But much of this work was theory-lite, or if there was theory it was loose and imprecise. Massey dismissed this “locational factors” approach as a mindless “search for a high R^2 ,” incapable of coming to grips with the scouring politics of contemporary industrial change (Massey, 1984:15). The book she wrote in response, *Spatial divisions of labour* transformed the field. It triggered one of the sharpest paradigm shifts in contemporary economic geography.

The OU workshop was at the cusp of two transformative moments—within the discipline of economic geography, and within modern industrial history and politics. It was an instant when politics and method both mattered intensely, and were conjoined. This was not the moment to skirt around methods, or to mouth accepted political nostrums. Both required scrutiny in the searing light of contemporary change.

More than two decades later, economic geography has passed through a series of far-reaching cultural, institutional, and relational “turns,” during

which time its objects, subjects, and means of study have been repeatedly overhauled. But despite these tumultuous changes, questions of method – the how and why of research – have been only fitfully (re)considered. With a few notable exceptions, methods went back underground. In this respect, the promise of *Politics and method* was never realized. Of course, research *practices* themselves changed. Partly prompted by Massey's work, economic geographers came to embrace intensive case studies and interview techniques, "finding the global in the local," and later they ventured into discourse analysis, participant observation, action research, ethnography, and much more. But, strangely, this produced little methodological talk, even though the methodological transformations were as significant as any theoretical ones.

The aim of *Politics and practice in economic geography* is to bring to the surface methodological issues that for too long have been buried. We have no expectation of consensus, but believe that conversation itself is important, and especially critical given the current variegated and pluralized form of economic geography. While there are reasons to celebrate this vibrant heterodoxy, the far-flung and varied character of the field means that it is all the more important to sustain, *indeed deepen*, intra-disciplinary discussion. Under such circumstances, the risk of methodological relativism, fuelled by mutual misunderstanding, is a real one. More methodological talk can help here. Not that it will resolve all differences over disciplinary purpose and practice, but it provides a language of engagement. On those occasions when methods talk rises to the surface, as it did in the wake of Ann Markusen's (1999) "fuzzy concepts" paper a few years ago, we are reminded how much remains unspoken in the field. While some saw the move towards intensive, case-study research as the early days of a "qualitative revolution," others interpreted these same developments as a

drift *away* from rigor and relevance (see Grabher and Hassink, 2003). For Markusen (1999:870), the slide into fuzzy conceptualization and slapdash theory development was accompanied by falling standards of evidence and an intellectual climate in which “[m]ethodology is little discussed.” Whether or not one shares Markusen’s conclusions, she is surely right to call attention to the serious consequences of eschewing methodological talk. Methods matter.

Here, we set the scene for the collection by providing a (necessarily selective) review of some key methodological developments in the field of economic geography during the period since the publication of *Politics and method*. We identify a number of crosscutting methodological paths, drawing attention to some of the landmarks, intersections, and dead ends. The coverage, of course, cannot be comprehensive, and inevitably it reflects the limits of our own Anglo-American cultural and linguistic reach. Moreover, the field of economic geography never evolved in a sequential, linear fashion, through a series of identifiable methodological phases. There is no grand narrative with a beginning, middle and end on which we can draw. And in many ways, we would not want one. But it does make telling the story challenging.

Lacking the frame of a single linear narrative, we pick up two parallel, but occasionally intersecting themes. One starts with political economy, where initial concerns with industry and employment have broadened into bodies of work on business networks, technology, knowledge, and transnational commodity chains. The other that starts with feminism and post-structuralism, with research on embodiment and work, extending out into explorations of social reproduction, the household, and social identity. Both strands are political, but differently political: in political economy, the

political looms large, it is about class and economic development, whereas in feminism and post-structuralism it is characteristically "small," about everyday possibilities for political action, including though the research method itself.

Methodological talk one: industry, place, networks

Economic geography's slow emergence from the cocoon of *industrial* geography coincided with its methodological awakening. By the mid 1980s, the taxonomic classification, "industry," the basis of so much in economic geography, was under severe strain. As the introduction to the definitive collection of the time put it, "the simple concept of a sector as a collection of establishments producing homogenous outputs is increasingly being called into question" (Storper and Scott, 1986:11). This was in part because of changes in the "real economy" were confounding industry classification schemes, together with the methodology of the "bounded" sector study. Not only did forms of vertical disintegration characteristic of "postfordism" muddy the singular category "industry," there was also growing awareness that service sectors warranted attention in their own right (see Walker, 1985; Storper and Walker, 1989).

If subcontracting, the rise of services, and more generally the emergence of flexible production called into question the reliance on *taxonomic* classifications of industry, other changes called into question sustained industrial production itself. Deindustrialization swept like a pandemic through the industrial heartlands of North America and Western Europe from the late 1970s. The subtitle to Bluestone and Harrison's (1982) influential book on American deindustrialization captured the moment: *Plant closings, community abandonment, and the dismantling of basic industry*. These

conditions engendered a new sense of political as well as analytical urgency in the work of economic geographers, radicalizing the field. Some joined the front line, defending jobs and working-class communities, and formulating alternative strategies in collaboration with labor unions, community groups, and municipalities.

The result was an unprecedented yoking of politics and method. A new and rapidly diffusing set of research practices focused on issues like workplace change and work reorganization (the "labor process"); international corporate strategy; and political alternatives (see Lovering, 1989; Peck, 2002). If workers and their communities were suffering then the focus, as Bennett Harrison explained, should be on the boardroom "perpetrators." Harrison and others insisted that research must be "explicitly value-driven and [should] consist of causal analysis combining strong logic and solid evidence to combat prevailing truisms" (Markusen, 2001: 40, 41). This ideologically-infused "activist intellectualism" was a far cry from the reserved and ostensibly apolitical industrial geographies that preceded it. After this radical turn, data would no longer speak for themselves and managers would no longer be blithely characterized as optimizers. Research was now understood as a *political* process, and managers were almost certainly up to something.

To establish exactly what, economic geographers delved *inside* the production process to figure out the particularities and consequences of deindustrialization. More than mere data-reporting units, firms were analysed as sites of class conflict and strategic decision-making. The challenges were to discover how companies really worked, how their markets were organized, what their competitors were doing, and the locational consequences. This prompted economic geographers to turn to

interviews with senior managers, to comb company reports, to talk to union representatives, and to draw upon increasingly sophisticated surveys.

The break with past economic geographical practice was marked, especially in Britain, where a largely atheoretical tradition was suddenly and comprehensively challenged by the arrival of Marxism and critical realism. The latter was especially critical of the 1970s "locational factors" approach. In this tradition, as Fothergill and Gudgin (1985:104) advised, "case studies should normally be avoided because it is difficult to know the extent to which the cases are typical, and the variability between cases often obscures trends that are clear when dealing with aggregate figures." Realists, on the other hand dismissed such aggregations as "chaotic conceptions" that lumped together causally-unrelated and qualitatively-variegated phenomena according to their co-location or descriptive characteristics (like company size or sector). Instead, realists favoured the close study of causally-defined groups (such as a group of firms linked through contracting relations or experiencing similar forms of technical change), delving beneath statistical artefacts like "net employment change" to identify the driving forces and mediating mechanisms involved in *specific instances* of restructuring. Explanatory penetration and political leverage, argued realists, stemmed from the careful weighing of the necessary and conjunctural conditions that shaped each "case." It would therefore be wrongheaded to imagine that a case might be more or less "representative" or "generalizable" (Morgan and Sayer, 1988).

This was not, as Sayer (1985:27) argued, the embrace of complexity for its own sake, or a turning away from abstract theory. Instead, it was a methodological counter to the widespread problem of "apply[ing] theory at the concrete level without due regard for contingent mediations." The

intention was to sharpen methodological practice within a radically inflected “new industrial geography.” In Sayer’s view, however, even the new wave of work was cluttered with misleading stereotypes, empirical blindspots, and poorly formulated abstractions. It often amounted to the reduction of the concrete to the abstract, in effect fetishizing some forms of restructuring as indicators of a supposedly singular development path, while ignoring or marginalizing others. Sayer’s realist injunction was not to question theory *per se*, but to utilize theory more carefully and reflexively.

The advice was to select case studies carefully, to cast the empirical net across a diverse range of circumstances (so as to *challenge* theory), and to engage in forms of *intensive* research marked by constant tacking back and forth between (provisional) theoretical explanations and cases, revising explanations along the way. This seductive formulation of the benefits of intensive research design was established by way of a pejoratively blunt, binary comparison with the “extensive” methods that were “far more common in economic geography” in the period prior to the realist ascendancy (Sayer and Morgan, 1985: 150). Extensive research designs that sought to identify recurrent or “typical” features of taxonomic groups, like firms in an industry or region, were damned with the distinctly faint praise of being useful at a descriptive level. The “extensive method lacks explanatory penetration,” Sayer and Morgan (1985:152-153) disparagingly observed, “not so much because it is a ‘broad-brush’ method and insufficiently detailed, but because the relations it discovers are formal ones of similarity, dissimilarity, correlation, etc., rather than substantial causal relations of connection.”

[Table 1 around here]

The summary table of the two research designs (Table 1), which was faithfully reproduced in the methodology chapters of countless PhD dissertations in subsequent years, might just as well have been labelled new wave/old hat rather than intensive/extensive. Advocates of orthodox, extensive methods within industrial geography apparently gave up without much of a fight. Intensive methods, with varying amounts of the supporting baggage of critical realism, became the new orthodoxy. This release of the methodological handbrake set economic geographers off on a search for theoretically informed case studies. Armed with new techniques like depth interviews, their mission was to divine causally meaningful, necessary relations and tendencies amidst the mass of contingent relations. No longer was there a singular empirical "reality" whose inner secrets could be apprehended by way of direct observation. The critical-realist world was a "layered" reality of structural/necessary relations, mediating mechanisms, and contingent outcomes.

While many, if not most, realist researchers drew their primary inspiration from Marxist theories, it was nonetheless an arm's length relationship. And when the criticisms of realism came, they were not from the positivist "old guard," but from advocates of more hardened forms of Marxism, some of whom thought they could smell an empiricist rat jumping from the ship. David Harvey, for one, railed against what he portrayed as a take-it-or-leave-it approach to theory, posing as "a convenient cover or as a transitional argument back to straight old fashioned and casual empiricism" (1987:**) . Sayer countered that while most of the protagonists in this debate were on the same side politically, this did not mean accepting Marxist arguments as a matter of "faith." Massey and Meegan, the Lancaster Regionalism Group, and other fellow travellers, he insisted, were elaborating theoretical explanations, not abandoning them. Sayer (1987:398) feared

that Harvey had begun to sound like “an Antarctic explorer, not daring to wander more than a few yards from the tent of *Capital* for fear of being lost in an empiricist blizzard.”

Convinced that they were heading the right direction, the undeterred British realists soon ended up straying even farther from base camp. Fine-grained analyses of restructuring became the order of the day. A special premium was placed upon what Sayer called the “hard work” of concrete, empirical research, typically at the local scale. In the UK, the Changing Urban and Regional Systems (CURS) program set out to elucidate the role of “place” in the process of economic restructuring through the intensive study of seven British localities. Each was conducted by a separate research team, working within a shared methodological framework. If the localities program set out to document some of the ways in which economic restructuring processes were “embedded” in the matrix of local social relations, it did so as an exploratory and relatively “open-ended” exercise (Cooke, 1986). There was a unifying concern with the effects of gender relations (reflecting the growing influence of feminism within British economic geography), with local political cultures, and with a determined inquiry of grounded relations. “The locality seemed to offer something concrete rather than abstract,” Savage *et al* (1987:28) reflected, “something specific rather than general, and a sphere where human agents mobilized and lived their daily lives.”

The localities initiative was immediately controversial. Neil Smith criticised this “empirical turn” for merely

documenting the minutiae of local change ... The signs are already ominous. If indeed the unique is back on the agenda, then it is difficult to see how we can avoid fighting the crude Hartshornian battle

between the ideographic and nomothetic, the unique and the general ... Should this debate emerge again, as indeed it threatens to do, it will be tantamount to admitting that we have learned nothing since the 1950s (Smith, 1987:62,66).

In retrospect, the methodological bark of the localities program was much louder than its explanatory bite. Scott (2000:28-29) subsequently characterized it as a "rather straightforwardly descriptive and empirical research program ... an amalgam of inductive studies of regional economic trends and of commentary on the cut and thrust of local politics." In his overview of economic geography's "great half century," Scott gave the localities program a place, but positioned it as an "interlude." David Harvey (1987) was less charitable.

While the localities initiative could claim some achievements—augmenting the standard operating procedures of industrial geography with more nuanced historical analyses (Beynon *et al*, 1994) and acute readings of gender relations (Bagguley *et al*, 1990) – much of the program looked more like an explication of extant theory, enriching the catalogue of cases, than an effort to generate new explanatory insights. The basic methodology of industrial restructuring studies was expanded "outward" to encompass a broader spectrum of local social relations, but this was achieved with the same methodological toolbox—semi-structured interviews and archival research, supplemented by secondary analysis of employment data. The locality studies lacked the ambition, depth, and salience of what in sociology were known as "extended case studies" (see Burawoy, 1991), and none rose to the level of "critical case studies," selected for the potentially definitive challenges that pose for extant theory (cf. Mitchell, 1983). Economic geographers were stepping outside the workplace, but the very richness of

these studies of contextualized industrial restructuring came at the price of explanatory traction and conceptual sharpness. And as diverting as tales of manufacturing decline in the Birmingham suburbs may have been, they paled in comparison with the new wave of studies emanating from California. Interlude, indeed.

In contrast with the narratives of loss that pervaded the locality studies, the upstart California school was much more concerned with the dynamics of gung-ho growth in what were styled as *new* industrial spaces (Scott, 1988b). Methodologically, this was back to the future. Again, the production process was seen, unambiguously, as the “engine” of regional transformation (see Storper and Walker, 1989). Allen Scott’s early 1980s studies of proactively restructuring sectors like printed-circuit production, animated film-making, and women’s garment manufacture drew on orthodox data sources like mail surveys and trade directories. Couched in an ambitious transactions-costs framework, Scott’s sector studies served the modest methodological functions of “corroboration,” illustration, and empirical “insight” (Scott, 1983: 248). His prime motivation was the “burning need to reopen macrotheoretical questions about the logic of capitalist society” (Scott, 1988a: 183), a consequence of the systemic shift from Fordism to flexible accumulation. The economic crises of the 1970s had triggered

a corresponding crisis of urban and regional theory. The old accounts of the forms of spatial development associated with the regime of Fordist accumulation and its cognate mode of Keynesian welfare-statist regulation were patently no longer very satisfactory as descriptions of underlying realities ... One of the evident responses in human geography to this predicament has been a certain disillusionment with theoretical work in general and a radical return to

empirical investigation, the multiplication of case studies, and an insistence on the significance of the local at the expense of the global and universal ... It is my hope [to find] potentially fruitful avenues of reconciliation between theoretical and empirical work in human geography (Scott, 1988a:182-183).

Scott's forthright claims yielded a sharp response from John Lovering, an advocate of critical realism and a researcher on the CURS program. Lovering (1990) took issue with what he saw as Scott's narrow, formal, and economistic theory of the firm—as a nexus of transaction costs. Scott underestimated the breadth of restructuring repertoire available to firms, focussing instead on "sectors that fit [his] stereotype of postfordist flexible accumulation" (Lovering, 1990:167). Scott's (1988a:181) confident identification of "a common underlying system of structural dynamics" was, Lovering insisted, a byproduct of a parameterized, firm-centric analytical gaze, one that seriously underestimated (often divergent) local circumstances. Local outcomes were not determined by inexorable capitalist dynamics, but were socially and politically mediated, requiring concrete study.

Scott's response was no less robust, demarcating where the methodological battle lines lay. Lovering was fixated, Scott (1991:133) countered, on "indeterminate complexity." The ultimate outcome of Lovering's "methodological ponderings," Scott said, was a conception of "empirical reality as just so much flotsam and jetsam, with theoretical ideas (so far as they play any role at all) reduced to inert atmospherics in the distant background." Lovering's wide-focus concern with the theorization of constitutive social relations, and with macroinstitutional and local cultural contexts, was likewise dismissed as a forlorn pursuit of "unstructured

empirical investigations as virtues in themselves,” bringing about a “chilling effect on high-risk theoretical ventures.”

In the end, relatively few economic geographers heeded the call to high-wire theorizing; most felt more comfortable nearer the ground. But Scott’s work on transactions costs nevertheless paved the way for the next generation of research, focused on a new object: the network. And soon, formalistic notions of industrial networks were giving way to more sociological treatments, turning on embeddedness and governance (Grabher, 2006). The insight here was that relationships among elements within a network are governed by, embedded within, a set of institutional norms, practices, and imperatives. Following Polanyi (1944, xlvi), embeddedness was the means to “grasp [institutions] in their concrete aspect.” This concern with the nitty gritty of institutional forms, functions, and associations, meant that empirical skimpiness was absurd. To stint the concrete would be to undermine the very justification and object of study. Methodological challenges must be confronted head on.

Network approaches seemed uncannily suited to the question of explaining high-tech growth, a key concern for “post-deindustrialization” economic geography. Animated by circuits of specialized knowledge, high-tech growth was evidently dependent upon embedded social networks of interpersonal relations and “trust,” and (re)produced through institutional governance structures, both formal and informal. Saxenian’s *Regional advantage* compared embedded governance structures and associated networks for two iconic high-tech regions: Silicon Valley and Route 128. These were indeed “critical cases,” capable of generating new theoretical insights, rather than merely illustrating extant theory claims. Breaking new ground for the discipline, the research strategy was overtly “ethnographic in nature,” with

the empirical data “accumulated over the course of nearly a decade living in and observing the two regional economies” (Saxenian, 1994:209).

Sustaining the book’s ambitious claims called for an up-close and personal engagement with sites and subjects. This was no quick and dirty study, nor could it be, given its comparative nature, and the variant of the network approach used.

Meric Gertler also derived conspicuous benefits from “being there,” from sustained and comparative case-study research. His work on advanced manufacturing systems explicitly begins with “deep differences in culture” between Germany and North America (Gertler, 2005: ix) – sufficient in some cases to bring work grinding to a halt. Like Saxenian, Gertler opts for a strategy of immersion.

Perhaps I am just a bit slow to figure things out. But I would argue that, from a methodological standpoint, there is considerable value in taking the time to immerse oneself in the different local contexts that ground business relationships in today’s global economy. There is also real value in following the evolution of relationships and practices over time, as I have been able to do in this extended study. The second or third interviews I have been fortunate enough to conduct with many of the firms included in this study have often yielded the most useful insights ... [There is no alternative to the] hard work of detailed, finely textured case studies: there is really no substitute for “being there” (Gertler, 2005: x).

Explanation here occurs through cases, richly documented in a fashion reminiscent of extended case studies. But in contrast to the work of both

British restructuring researchers and the California school, the theoretical commitments are mostly implicit.

The methodologically substantive contributions of Gertler and Saxenian seem much more likely to stand the test of time, however, than economic geographers' interventions in the related industrial districts debate. While rhetorically bold, these explorations of place-based networks were generally modest in terms of empirical substantiation. Not only did they remain within a tight productionist groove, they were also methodologically limited. Industrial districts could have been economic geography's finest hour—a construction that was constitutively geographical, and one that sparked interest across the social sciences. But in retrospect, the classic (or critical-case) studies of industrial districts of the 1980s and 1990s were carried out by economists, political scientists, and sociologists. Economic geographers missed an opening, and the sneaking suspicion is that methodological limitations were part of the problem.

Characteristically, economic geography moved on. Convinced that it was mistaken to sequester network relations to the local scale, economic geographers began to shift their attention to cross-scalar, *globalizing* networks. Again, much of this work was theoretically propelled, rather than empirically fuelled, though some of its enduring traces include work on Chinese business networks (Olds and Yeung, 1999) and distinctive “worlds of production” (Storper and Salais, 1997). In the process, however, there was barely a methodological pause, and rare exceptions to this rule were as much concerned with analytical as methodological frameworks (see Dicken *et al*, 2001). The exacting methodological debates around the parallel project of “global ethnography” (see Eliasoph and Lichterman, 1999; Burawoy *et al*, 2000; Gille and Ó Riain, 2002) represent a striking contrast.

Major impetus for economic-geographical work on networks came from actor-network theory (ANT). With roots in the sociology of science literature, ANT radically broadened the network optic. Now, anything could be an actor, from an INTEL micro processor chip to a post-it note or a stressed-out day trader. Nothing is too small, intangible, or simple. Indeed, the mundane and the everyday keeps the network functioning. For example, Leyshon and Thrift (1997) demonstrate the role of non-human agents, especially machines and texts, in their network account of London's financial center.

The point of these arguments is that it takes hard work to sustain a network. At any moment, it may unravel, requiring the continual shoring up of old relationships, and the making of new ones. It follows that actor networks make their own geographies. They are not pegged on a pre-existing Euclidean frame; they generate their own topologies and geographical forms. And because of interaction between an agent and the network ("translation" in ANT language), new objects are continually created—known as hybrids or quasi objects. A good example is "overseas" Chinese business networks, a hybrid object lying between traditional Chinese (*guanxi*) and Western business forms, but reducible to neither (Yeung, 2004). As a *methodology*, ANT approaches are liberating in the sense that they privilege following networks, wherever they lead, while staying close to the ground (see Murdoch, 1997). This is often seriously disruptive of extant explanations, and indeed this is partly the point: as Latour once gnomically pronounced, ANT is a "negative methodology." ANT's "stories" are made in the telling; they do not follow a methodologically prescribed path. Methodological guidelines themselves might even be considered somewhat contrary to the project.

More methodologically prescriptive is the commodity chain approach. At first blush, the commodity-chain concept seems clunkier than that of actor-networks. A chain implies linearity, and the focus on the commodity seems unduly restrictive. But as economic-geographical extensions of the commodity chain approach increasingly allow for dense, multifarious, nonsequential, and dynamic relations. For example, Hughes (2000, 178) describes a commodity chain as a “web of interdependence.” And since Marx, it is recognised that there is nothing restrictive about a commodity: “a commodity appears, at first sight, a very trivial thing, and easily understood. Its analysis shows that it is, in reality, a very queer thing, abounding in metaphysical subtleties and theological niceties” (Marx, 1976: 163). Economic geography’s best work on commodity chain analysis tries to recoup those subtleties and niceties, revealing social, cultural, political, and not least geographical processes along the way.

Given the potentially enormous reach of commodity-chain and ANT analysis—comparison across national borders, much travel, use of translators, knowledge of disparate actors connected through the chain, understanding of an entire business, from resource extraction to retailing—the methodological challenges are demanding. But yet again comparatively little has been written about its method. A notable exception is Ian Cook’s (2001, 2004) work on the papaya commodity chain, beginning with plantations in Jamaica and ending as dessert in a North London flat.

Cook follows his commodity from the field to the plane, to the London supermarket, and to the fruit bowl. Although the narrative appears linear, it is not, as crosscutting connections are drawn among and outside its constitutive stages: to colonialism, to the WTO, to Western middle-class

consumption aesthetics, to the diseases and pests of exotic fruits, to the character of international air-cargo transportation, and to developing-world labour control and surveillance. This is an unbounded, dense network of associations. And precisely because it isn't a simple chain, the commodity itself is no "trivial thing;" it is composite, defetishized, decrypted, reflecting all manner of trace effects. Methodology is hardly trivial, either. Cook reveals problems in just getting to Jamaica, of keeping informants on side, of taping interviews, of translating and transcribing them. Even after he returns to England, the problems continue. There are rows in his department over his research, a shortage of money, and a disastrous presentation of his results, involving accusations of paternalism and jingoism.

Reflexively wearing one's methodological heart on one's sleeve like this can be disconcerting. It certainly represents a radical break from economic geography's conspiracy of methodological silence, marking the distance the discipline has travelled since the days of the aridly dispassionate "sector study." Yet the legacy of methodological reticence remains, particularly in comparison with those social-science disciplines, like anthropology and sociology, that seem obsessively gripped by the need to reveal all. Surely, there lies something between exhibitionism and covering up?

Methodological talk two: praxis, feminism, post-structuralism

Political economy is one thread weaving through the last quarter-century of economic geography's methodological history. There is at least a second, sometimes frayed and broken, which starts roughly at the same moment, but which stresses different elements, draws on other intellectual traditions, emphasises alternative political ends and means, and deploys distinct

research methodologies. Especially at the beginning, it struggles to separate itself from political economy. But as it emerges, its theoretical sources become increasingly distinctive, primarily feminism and post-structuralism. Its foci are work, bodies, social reproduction, households, gender and ethnicity, and community. Interpreted through the lenses of discourse and power relations, analysis tends to be anchored in very particular geographical sites, both public and private. The nature of politics and political intervention is also different from political economy. Less couched at the scale of capitalism as a whole, it stresses active, local intervention, one means of which is the research process itself.

While there are these two separate thematic threads, they sometimes twist and knot, even in the lives and works of particular individuals. Doreen Massey, for example, made formative contributions to both. Indeed, she may not see any separation, but just a single thread. That said, for our purposes of trying to impose a narrative order on the disorderly methodological history of economic geography, it is useful to recognise the two traditions. Appropriately, then, we begin at more or less the same point ...

The economic dislocations of the early 1980s not only helped to generate abstract, academic political-economic schemes like the restructuring approach. They also stimulated hands-on political action, focussed on the immediate and the local--resisting plant closures, fomenting community action, serving on advisory committees and local councils. At least initially, praxis in economic geography seemed joined at the hip to political economy. It meant walking picket lines by day, reading Marx by night. In the United States, Bluestone and Harrison played a key role, influencing a generation of economic geographers (*Antipode*, 2001). Harrison's style of research was

characterized as both action-oriented and collaborative, “work[ing] closely with union and community-based organization leaders to craft research that would be useful to them,” thereby establishing a research agenda “not wholly shaped by the academy” (Christopherson, 2001:32). Indeed, Bluestone and Harrison’s (1982) *The deindustrialization of America* started life as a project commissioned by a labor-community coalition, both its “analysis and its strongly worded policy recommendations [owing] much to what Ben learned from labor leaders, rank-and-file trade unionists, and community activists” (Markusen, 2001:40).

In Britain, at more or less the same time, action-research projects were initiated by the Labour Government’s Community Development Programme (CDP). The CDPs spawned a series of community-based research projects, often triggered by events like factory closures or large-scale redundancies. It was in this context that “[e]conomic geography, and the questions of industrial location and labour markets in particular, took on a new political salience” (Lovering, 1989:201). Out of this ferment came, among other things, direct challenges to Thatcherism: the municipal socialist movement, and a closely intertwined intellectual project animated by the question, “In what sense a regional problem?” (Massey, 1979). There was also a significant, two-way movement—both of ideas and individuals—between the academic world and local-government research units, community-development organizations, and trade unions. One of the more notable outcomes of these efforts, alongside countless local jobs plans and municipal economic strategies, was the *London industrial strategy* (GLC, 1985), which “drew heavily on the new industrial geography to propose fresh, practical ideas for the reindustrialization of inner London” (Storper, 1987: 593).

Such work, both in the US and the UK, was predicated on the belief that the larger economic system could be fixed. Through a series of reformist measures framed often at the local level, sufficient band aids could be applied to patch up the body economic. These initiatives, originally proposed by communities, organised labour, and progressive local governments, were by the late 1980s onwards later systematised by economic geographers, as well as by others, as full-blown schemas, plans and policy initiatives. They went under such rubrics as flexible specialization, postfordism, local competitiveness and “cluster” strategies, and more recently, learning regions and creative cities.

J. K. Gibson-Graham—a student of Ben Harrison, involved early on with community organisation around plant closures—later reflected that the early 1980s was a period in which “most leftists struggled ... without a clear vision of the future.” They fell into the belief either that local strategies were justified on the grounds that the transcendence of the “system” was effectively beyond reach, or that “the only way to create a noncapitalist alternative is to create a prosperous capitalism first” (Gibson-Graham, 1996:164). For Gibson-Graham, however, it was imperative to have that vision, accompanied by an appropriate theoretical and methodological armature. The forms of activism associated with political economy, and which later morphed into policies of postfordism, cluster strategies and the like, were not achieving what they might. This was because of the baggage of essentialism and totality that political economy carried, limiting what it could accomplish. In Gibson-Graham’s view, political economy delineated “economy” by a set of definitive characteristics, established in opposition to the social and the cultural, and envisaged as total, unified and pervasive. Consequently, where activism could take place, how intervention could be carried out, at which sites and scales, and by which actors and agencies

were all narrowly determined by this essentialised conception. That conception needed to be overturned.

Gibson-Graham's project was to think about the economy, and thereby what it meant to fix it, in radically different ways. Their starting point was feminism and post-structural theory. Both offered a more expansive and porous conception of the economy, pointed to topics consistently underplayed or ignored, emphasised a richer, more varied palette of research techniques and questions, and indicated alternative forms of praxis.

Feminism first entered economic geography around the moment of deindustrialization, in the form of empirical studies of the spatial constraints women faced in the employment market. By the mid-1980s, those studies were joined by theoretical discussions around domestic labour and patriarchy taken from Marxism (socialist feminism). By the late 1980s, there was yet another break, stemming from changes within feminist geography and the introduction of post-structural theory (Bowlby *et al*, 1989). That theory was multifaceted and dynamic, but a central claim turned on the construction and power of discourse in shaping, permeating, and directing non-essentialised differences of all kinds: gender, race, subject identity, bodies, and, as we shall see, even the economy. Moreover, this was theory designed as Foucault said "to groan and protest" within concrete studies. It was no armchair theory, but gained legitimacy at ground level from the deployment of situated research methods. Finally, it was theory with a political point, to change the world. Unless strategies for political change were part of its furniture, it was irrelevant theory.

This combination of sensibilities began to transform economic geography from the early 1990s. Doreen Massey's (1995; Massey *et al*, 1992) work on

the high-technology complex in Cambridge (UK) was among the first to demonstrate the importance for economic geography of focussing on socially inscribed bodies. With Nick Henry, not only did she talk to economic geography's usual subjects—company directors, personnel managers, key workers—she also “interview[ed] the partners of the scientist/engineers in the cases where they were cohabiting” (Henry and Massey, 1995: 50). What they found was workaholism run rampant, which affected more than just the workaholics themselves. It impacted those around them, such as administrative assistants, secretaries and partners. Unsurprisingly, there was a clear division in the way in which the bodies involved were socially marked: more than 90% of the high-tech workers were white men, mostly under 40, and graduates, while most of the assistants, secretaries and partners were women.

Massey's point was that while scientists and engineers portrayed themselves as disembodied rational minds, ferreting out objective truth in the name of science, the organisation of their corporeal lives told another story. The inordinate hours that men worked as scientists was made possible by work of a different sort, done by their predominantly female partners and support staff (Massey, 1996). Male abstract rationality was achieved by female labour.

In many ways, Massey's study lay between political economy and feminism and post-structuralism. Still centrally concerned with class and the economy, making use of a traditional large-scale industry survey as well as interviews, it also pointed to a different trajectory, focussed on gender and work, the construction of masculinist discourses, and intertwined relations of power. In addition to asking conventional questions about production, it also problematised reproduction, asking “who irons the shirts?”

Yet more ambitious forms of methodological fusion were to follow. Hanson and Pratt's (1995) *Gender, work and space* married established concerns of feminist labour-market and transportation studies with post-structural theories. The book presented a complex layering and interlocking of different methodologies. There are certainly tables of numbers, of travel times, wage rates, gender employment ratios, and even the occasional chi-square statistic, but there are also rich, complex, contextual stories about identity, mobility, constraint, and local situatedness, gathered through focus groups, extended interviews and ethnography. The strength of the book lies in the braiding of these different methods into a coherent narrative about the nature and effects not only of gender differentiation, but differentiation within gender, along the fault-lines of class and race. Methods here are not mere instruments, external tools to achieve a particular end, but are integral to the end itself.

And the feminist injunction here, beyond recognising the often-concealed nature of "women's work," is to see how economic relations are themselves co-constituted through gender relations. In a radically different context, this was also a defining theme of McDowell's (1997) examination of the performance of merchant-banking work in the City of London. Here, the body itself constitutively entered into the economic product, in this case, financial services:

The theoretical focus on the body, sexualised performances and strategies of surveillance parallels material changes in the nature of work in service sector occupations. One of the key features of service sector work, compared with manufacturing jobs, is that the labour power and embodied performance of workers are part of the product in

a way that was not the case in the production of manufactured goods ... In service interactions, the body of the worker demand[s] an embodied and visible performance (McDowell, 1997: 32)

Conventional accounts of workers in the City of London tend to privilege their (disembodied) mental capacities—the ability to make split-second calculations about buying and selling. They appear, like Massey’s high-tech scientists, as minds without bodies. McDowell’s argument, however, is that bodies are integrally involved in the product: workers must talk the talk, but also walk the walk. They must perform appropriately, following the script, looking the part. Bodily acts constitute the reality of the “good” that service sectors sell. Economy incarnate.

Researching embodied practices is an embodied practice itself. McDowell’s first sentence is: “This book is the result of field work in the City of London” (1997: v). Interspersed with her “results” are accounts of what it was like to be in that field, how she operated, how her own performance as a socially inscribed body affected her results, what types of methodological strategies she deployed and their justification, and why it mattered politically that the focus was gendered bodies and feminist methods. Like Hanson and Pratt’s work, but unlike many economic geographies that preceded it, McDowell’s methodology is never separate, implicit or invisible.

The point, in fact, had emerged earlier in a debate between McDowell and Erica Schoenberger about corporate interviews—a rare outbreak of reflexive methodological debate in economic geography. Schoenberger’s (1991) defence of the technique of the corporate interview had been primarily aimed at a methodologically orthodox audience sceptical of such “subjective” qualitative approaches. McDowell’s criticism, however, came from a quite

different direction. It concerned Schoenberger's lack of recognition of gendered bodies, hers and those of her interviewees.

I wondered about her reason for not addressing one of the most significant factors in her own research – the fact that she is a *woman* and that almost certainly, the majority of her respondents were men. Earlier in her paper she suggested that in corporate interviews the investigator and the respondents were likely “to share the same language and social background.” So why did she omit to consider the one single important difference between them – that of their sex? Was she so certain that gender relations were irrelevant in these circumstances? (McDowell, 1992: 214)

In her reply, Schoenberger (1992: 217) said, “I agree with McDowell that gender makes a difference. The reason I did not talk about it was that I am not sure precisely what difference it makes, and I am not sure how I would know.” Her primary purpose was to expose cultural constructions of corporate culture, in contrast to the rationalist logic employed, not least, by managers themselves (Schoenberger, 1996). As she would later explain, “the corporation ... is both a site of capital accumulation and a stage for the playing out of powerful psychological and emotional processes. This suggests that we're not going to understand what corporations do in the world and why without analyzing this turbulent mixture of passion, power and rationality” (2001: 296). But both actors and analysts can have blindspots, and the purpose of McDowell's feminist intervention was call attention to the constitutive role of gender relations in sites where they are so often frozen.

Gibson-Graham's (1996, 2006) work is likewise productively disruptive, but in her case in explicitly political ways. Her action-research method sought no less than the transformation of the subjects of study, and their circumstances. Gibson-Graham suggested that, rather than wait around for once-and-for-all, systemic change, progressive transformation could be achieved piece meal, bit-by-bit. Furthermore, and this was the lesson of feminist methods, praxis could be achieved through the very research process itself.

Her site was of four coal mining towns in Queensland, Australia. The subjects were the female partners ("mining town women") of the male miners working there. Characterising prevailing household class arrangements as "feudal," Gibson-Graham argued that the bodies of women had been given over to patriarchal domestic service, "hewers of cake, drawers of tea." Because of declining markets and falling prices in the late 1980s, coalmining companies pushed to "remove restrictive work practices" (Gibson-Graham, 1996: 225). Instituting the "seven-day roster," companies made miners work more weekends, significantly increasing workloads of female partners. This was the moment at which Gibson-Graham began her research. She did not want to carry out standard social science, interviewing the women, treating their stories as simply data for analysis. Nor was she interested in another orthodox political economy, just another illustration of capitalist crisis and oppression. Rather, in the spirit of action research, she wanted to use the very process of interviewing mining-town women as a political opportunity for progressive intervention. Hiring mining-town women first to participate in workshops, and later to carry out interviews with other women in the community, the research project as carried out "in workshops and over the kitchen tables where one-on-one interviews were conducted ... created and cultivated spaces in which a feminist politics (the transformation

of gendered power relations) was performed” (Gibson-Graham, 1994: 215). In turn, this influenced male partners, ultimately making a difference in the mine itself.

This is a striking illustration of forms of political intervention in the economy made, as it were, below the waterline. The “formal” economy, constituted through wage labour in capitalist enterprises is but the tip of a much larger iceberg, Gibson-Graham insisted. A plethora of alternative economies exist in the mostly submerged ninety percent (see Figure 1). One of the tasks of progressive, action-oriented research is to enlarge the field of the visible, not least with the assistance of research subjects themselves. The purpose is as much political as analytical: when they believe there is only one essential economy, uniform, pervasive and impregnable, people give up before a hopeless task. But by pointing out the possibilities of diversity, that structures are not universally given, change becomes possible. Giving lie to the essential economy therefore exposes new futures.

[Figure 1 around here]

Geraldine Pratt’s (2004) *Working feminism* is also about trying to realise new futures through the research process. In this case it is for Filipina domestic workers in Vancouver, who she contends are abused both by their employers and the Canadian state. As a feminist, Pratt insists that it is necessary to make lines of connection with women who are otherwise very different from her; as a theoretician, she thinks it is necessary to interrogate abstract claims against the stubborn materialities of a prolonged case study; and as a methodologist, she is keen to explore how far one can take a single case study before repetition and banality take over.

At the heart of this extended case is collaboration with the Philippine Women Centre in Vancouver. She began the co-operative relation for political and methodological reasons. Politically, she wanted to be part of an existing organisation in which she could help facilitate progressive change for Filipino women predominantly employed as domestic labourers and caregivers. Methodologically, she recognised that

sustained collaboration ... forced a responsibility to theorise in concrete ways. Working with any community group has this effect ... Working with Filipino activists' groups is to be continuously reminded of the limits to generalization, and the value and necessity of theorising within the everyday rather than viewing the empirical merely as an illustration of a vehicle for abstract theorising (Pratt, 2004: 8).

So, like Gibson-Graham, Pratt uses both "extended" research methods and prolonged forms of engagement to realise political ends. But these ends entail her "continuously" having to rethink both methods and theory.

Both essentialism and masculinism run through the entire history of economic geography, shaping its content, delineating what counts as acceptable knowledge. First feminism and later post-structuralism substantially transformed the "subject matter" of the discipline. Not only did the topical concerns of economic geographers shift, their conceptions of subjects, and relations with those subjects, also changed. Feminists, in particular, have been much more open to making methodological talk, showing what the discipline might be if, rather than skirting around its methods, it opts to expose, celebrate and strategically deploy them. Such a

task is ever more urgent as the discipline multiplies and proliferates, respecting no boundaries, either substantively or methodologically.

Conclusion: talking methods

Feminist economic geography is the exception that proves the rule: for the most part, the discipline appears to operate a “don’t ask, don’t tell” policy with respect to methods. We all use them; we just don’t talk much about them. Sustained methodological reflection is exceptionally rare in published economic-geographical research. Tracking down even cursory information on methods involves reading between the lines, even in methodologically creative texts. Economic geography is methodologically opaque. This alone, surely, is reason enough for economic geographers to begin talking about methods.

In the period covered by our review, economic geography was a fast and fecund discipline: theoretical frameworks and topical concerns turned over quickly, and research orientations and practices also were repeatedly transformed. The analysis of censuses and surveys, once the methodological norm in the field, is now a minority practice; formal modelling and sophisticated quantitative analyses are almost lost arts (see Hamnett, 2003). Meanwhile, an array of new theoretical influences (feminism, regulationism, evolutionary economics, economic sociology and economic anthropology, nonrepresentational theory ...) and substantive concerns (networks, cultural embeddedness, embodiment, governance, social reproduction ...) have led economic geographers into new methodological territories. Case-study research, once the exception, is now standard. And a wide range of qualitative and intensive research methodologies—depth interviews, ethnography, discourse analysis, oral

histories, focus groups, participant observation, performance, activist engagement—all have a place. But many of these approaches would have been considered avant-garde in economic geography as recently as the mid-1980s.

This remarkable period of *understated* methodological proliferation has been a creative and productive one for economic geography as a field. A number of recent “state of the art” collections attest to the discipline’s rude health (Lee and Wills, 1997; Clark *et al*, 2000; Sheppard and Barnes, 2000). So what’s to worry about? Notwithstanding the progress made in the field in recent years, there has been a growing undercurrent of anxiety. Is economic geography losing its focus, its role and relevance? Is it asking the right questions? Is it connecting in the right way to policymakers and other audiences? What is its (appropriate) position within the academic division of labour? What is its distinctive message?

The general paucity of methods talk in the discipline, facilitated by conventions largely of its own making, has done nothing to alleviate these concerns. In published works, the analytical rationale for case-study selection rarely receives more than cursory attention. By the standards of extended-case methodologies, the extension of cases through time (entailing protracted temporary commitments to field sites, over space (via multi-locale case studies), and across scale (connecting “global” to “local” processes) is only sporadically achieved. Sustained theoretical development by way of case-study research is hampered by an apparent disinclination across the field to invest in corroboration, triangulation, and interrogation across comparative sites. The degree of codification and “sharing” of research practices, which would entail high degrees of transparency and reflexivity, lags the uptake of qualitative methods. Sampling strategies and

procedures are often glossed over, particularly in low-n studies. It is rare to see autocritical methodological reflection in economic geography papers. With a few notable exceptions, issues of researcher positionality remain unacknowledged and unexamined.

Not only is much methods talk reduced to footnotes, the field of economic geography may also be somewhat vulnerable to the charge that its aggregate level of methodological experimentation and creativity is not high. Interviews are the method of choice across large parts of the discipline. This is an area where there is productive, if sporadic, engagement with issues of positionality, ethics, and technique (see Clark, 1998; *Geoforum*, 1999), but venturing beyond interviews is as not commonplace as it might be. Ethnography and participant-observation techniques occasionally are used, but genuine ethnographic depth remains illusive (Herbert, 2000; Lees, 2003).

This does not mean that economic geography is bereft of good methodological practice. Rather, it is a comment on the nature of our disciplinary conversation. Across the field as a whole, a low premium is placed on methodological codification, reflection, transparency, and reflexivity. The consequences include: a narrowing of the bandwidth of our disciplinary conversation; a slowing of the pace at which the “tricks of the trade” are learnt by entrants to the field, who often must remake old mistakes and reinvent wheels as a result; and a reduced capacity to engage with other disciplines, which are less likely to extend the kind of methodological trust that we so freely share amongst ourselves.

It follows that, if economic geography is able to overcome its methodological reticence, there will be paybacks in a number of areas. The first and

perhaps most obvious is an enriched and “thicker” disciplinary conversation, in which research practices and processes are taken seriously. As the following contributions demonstrate, encouraging economic geographers to talk (again) about method is not self-indulgent soul-searching, it opens up *and sharpens* questions of politics, purpose, and priorities. Second, talking more about methods has the potential to increase the “social productivity” of the discipline. Methodological transparency facilitates learning from others. Both good methodological practice (“minimum standards”) and methodological creativity (pushing the envelope of research practice) are enabled by more methods talk. Third, the scrupulous articulation and defence of method both keeps us honest and aids rigorous conceptualization. Method provides a crucial “hinge” between empirical evidence and theoretical claims. And fourth, methods talk facilitates more effective and deeper forms of communication, improving cross-disciplinary engagement, and reducing the risk of balkanization.

A more methodologically reflexive economic geography will also be a *different* economic geography. There are certainly upsides to economic geography’s disciplinary velocity, including its adventurousness, novelty, energy, and creativity. But there is also a downside to a “don’t-look-back” ethos. There is little patience for corroboration and confirmation; there is a preoccupation with the moment of disclosure, rather than the slower processes of substantiation and extension; and arguments are rarely interrogated in depth before the field moves on. Martin and Sunley’s (2001: 153) commentary on the condition of economic geography, for example, reached the sobering conclusion that, “the majority of new concepts now go uncontested.”

In contrast, a more methodologically deliberative and reflexive economic geography would be a more *methodical* economic geography, focussing attention not just on staking claims but *sustaining* them. Our aspiration is an economic geography that is as effective at holding its ground as it is in breaking new ground. And the ground should also be methodologically pluralized and *contested*, in contrast to the present situation, in which different islands of practice in the discipline tend to be “mono-methodological.” Methods talk is not exclusionary, an official set of prescribed topics of dialogue, or even worse, a gag order prohibiting certain utterances. We are not trying to shut anyone up; quite the reverse. Methods talk liberates and deepens the conversation, allowing us more effectively to connect politics and practice.

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Table 1: Intensive and extensive research compared

	Intensive	Extensive
Research question	How does a process work in a particular case or a small number of cases? What <i>produces</i> a certain change? What did the agents actually <i>do</i> ?	What are the regularities, general patterns distinguishing features of a population? How widely are certain characteristics or processes distributed or represented?
Relations	Substantial relations of connection.	Formal relations of similarity.
Types of groups studied	Causal groups.	Taxonomic groups.
Type of account produced	Causal explanation of the production of certain objects or events, though not necessarily a representative one.	Descriptive, "representative" generalizations, lacking in explanatory penetration.
Typical methods	Study of individual agents in their causal contexts, interactive interviews, ethnography. Qualitative analysis.	Large scale survey of population or representative sample, formal questionnaires, standardized interviews. Statistical analysis.
Are the results generalizable?	Actual concrete patterns and contingent relations are unlikely to be "representative," "average" or generalizable. <i>Necessary</i> relations discovered will exist wherever their relata are present, e.g. causal powers of objects are generalizable to other contexts as they are necessary features of these objects.	Although representative of a whole population, they are unlikely to be generalizable to other populations at different times and places. Problem of ecological fallacy in making inferences about individuals.
Disadvantages	Problem of representativeness.	Lack of explanatory power. Ecological fallacy in making inferences about individuals.

Source: Sayer and Morgan (1985)

Figure 1: The economic iceberg



Source: J-K Gibson-Graham (n.d.)