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## **Economic Geography and the Financial Crisis: Full Steam Ahead?**

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*The Professional Geographer*

### **Abstract**

This article considers whether the growing theoretical and methodological diversity or pluralistic nature of economic geography contributes to its lack of engagement outside the discipline and academy. While we are enthusiastic about the vibrancy this pluralism brings, we also speculate that it contributes to the discipline's tendency to fall short of significantly impacting key debates in the social sciences. In particular we consider the disciplinary challenges to influencing mainstream debates over financialization and the recent financial crisis, and the recurring lament that economic geography "misses the boat" by failing to significantly impact key scholarly and policy issues. Specifically we suggest that methodological and theoretical diversity, local contextualization and relational analysis, all of which we support as vital to the discipline, make it difficult to isolate a disciplinary core. We conclude that pluralism produces a vibrant discipline with unique explanatory power, but that it also has important impacts on the design, execution and influence of geographers' research outside the discipline.

**Key words:** *crisis, economic geography, finance, pluralism*

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"Although the book was fundamentally geographical, I took the decision at the outset not to use the word 'geography' in the title nor even to divulge my identity as a geographer. In some ways, I now feel a little ashamed of having done that. On the other hand, at that time – and, to some extent, this is still the case most people would not have taken such a book written by a 'mere geographer' very seriously. So it proved."

--Peter Dicken (2004a, 514), reflecting on his book, *Global Shift*, 18 years after publication of the first edition.

### **Introduction: Diverse engagements**

It is likely a coincidence that the same year Peter Dicken discussed his hesitancy to identify as a geographer, he also made the case that geographers had "missed the boat" of

scholarly debates about the concept of globalization (2004b). Dicken's two observations, coincidence or not, are indicative of a key disciplinary challenge debated at the 2010 Summer Institute for Economic Geography (SIEG)—that between *diversity* and *engagement*. Diversity, because regardless of our quick development into a friendly community, it was a community incapable of, or unwilling to, agree on a common theoretical, methodological or topical core to unite us. Engagement, because while we passionately debated the strategies, scope, nature and relevance of our work on the most pressing contemporary issues, we found no common definition of, or strategy for, engagement with them. Ten years after Amin and Thrift's (2000) oft cited suggestion of a "turning point" in economic geography, the discipline is more theoretically diverse than ever, but is it also less engaged? While we generally applaud increasing pluralism, we wonder if economic geographers in their expanding diversity have inadvertently sacrificed some of their capacity to make an impact outside the discipline and the academy. In particular we are interested in pluralism in geographers' work on the recent financial crisis. In that context we question first, what constitutes the pluralism of the discipline and second, whether geographers ought to be concerned that this pluralism further denies economic geography the benefits of a disciplinary core?<sup>1</sup>

The authors were all drawn to economic geography by what seemed a heady mix of theoretical vibrancy, methodological openness, and clear (if varied) political commitments. Now that we are on the "inside", we see that this heterogeneity has implications for early-career academics. As discussed at the SIEG, we find this especially important in areas such as

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<sup>1</sup> By "disciplinary core" we refer to a theoretical and methodological cannon, something we believe economic geography lacks. However, we also realize that while economic geography might appear *from within* to be loosely constituted, heterogeneous, and plural, the external view is of a relatively closed and exclusive milieu with rigidly policed processes of interpellation. This applies to all academic disciplines. Our view of economic geography as pluralist and lacking a core thus *means in relation to other disciplines*, in particular other social sciences disciplines such as economics and economic sociology.

positioning within a tight academic job market. While we may be more comfortable than others conversing outside our discipline, we also find it more difficult to identify our scholarly niche, and thus our unique contributions *qua* geographers. This raises key questions about our disciplinary response to, and engagement with, the recent financial crisis and its reverberations. Nevertheless, even though it often seems like a potential solution, we believe that now more than ever, Amin and Thrift's (2000) warning holds true: we should not seek legitimacy through rapprochement with orthodox economics, no matter how much we might envy that discipline's apparent coherence and clout, seemingly undiminished, like the power of investment bankers post-2008. In other words, even though our ultimate motivation is to do "urgent" research (Samers 2001), we suspect a direct engagement with rational actors, efficient markets, and neo-classical pricing theory would quickly squeeze out what we are most interested in: unevenness, inequality, historical contingency and alternative futures. In fact we needn't look further than economic sociology, always a potential disciplinary partner, to see the unfortunate effects of an attempted reconciliation with mainstream economics<sup>2</sup> (Peck 2005).

In particular we are reacting to Engelen and Faulconbridge's (2009) "Dickensian" warning that the financial crisis could be geography's "next missed boat". Two years later our qualified response is economic geographers have responded to the crisis with all hands on deck, but we are nevertheless interested why there is a recurring lament that economic geographers are insufficiently engaged in key debates. In fact there is a growing body of geographic work related to the economic and financial crisis that began in 2007 (Aalbers 2009, French, et al. 2009, Mann 2009, Harvey 2010, Castree 2010, to name only a few). Other than for calls for more

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<sup>2</sup> While we are oversimplifying the argument, Peck suggests that by attempting to engage with neo-classical economics, economic sociology, or at least the "new economic sociology" (NES), was "disciplined" into accepting some of the key neo-classical axioms such as rational actors and efficient markets. This made it difficult, if not impossible, to arrive at significantly different conclusions. You might say, following Amin and Thrift (2000), that NES lay down with the lion, and ended up as prey.

geographically minded work (Engelen & Faulconbridge 2009, Lee et al. 2009, Wojcik 2009), however, there is a distinct lack of consensus in economic geographers' reactions. So while we are generally sympathetic with Barnes's and Sheppard's (2010) promotion of "engaged pluralism" in economic geography, we also want to suggest there is a trade off between pluralism and engagement. In other words, we believe that economic geographers' hesitance to "discipline" each other contributes to the inability to pinpoint just what economic geographers "do" and thus, how they enrich larger debates. At a time when there is citational evidence that economic geography may have growing influence outside the discipline (Foster et al. 2007), this is an important consideration.

All of this said, we will not attempt a comprehensive survey of economic geographers' contributions to discussions of the 2008 financial crisis. Instead, we have chosen three examples of emergent geographic work on finance and crisis that, in their methodologies, help explicate three components of our broader argument about the trade-offs between disciplinary diversity and engagement. The first example, focused on the politics of post-crisis pension provision, considers the remarkable analytical benefits, but also the disciplinary tradeoffs, to using a heterodox collection of methods and theoretical frameworks that are common in economic geography. The second example, focused on post-crisis industrial clusters, considers the long-standing challenge to geographers of making the contingency of local geographies relevant to other disciplines and policy makers. The third example, focused on post-crisis economic recovery in Latin America, considers the necessity of relational analysis to economic geography, but also the challenge of making the often "messy" explanations relevant to "outsiders". Considered together, these examples demonstrate that much of the explanatory power of the discipline comes from the same things that contribute to the perception that it fails to engage.

Far from simple character flaws, these idiosyncrasies are fundamental to the fields' commitment to broaden economic discourses. In the conclusion we question whether contemporary economic geography is well suited to produce the sort of pithy responses necessary to engage outside the discipline and academy without jettisoning what we consider some of its most important characteristics.

### **The politics of pension inequality**

The work by economic geographers on pensions and economic decision-making is illustrative of the issues, challenges and opportunities of a pluralistic discipline, especially at a time when reductions in public employee pension contributions have become a key component of post-crisis austerity movements in the UK and U.S. A significant strand of the research agenda of the Centre for Employment, Work and Finance directed by Gordon Clark and Linda McDowell at Oxford focuses on the evolution of occupational pension systems and institutions, and processes of economic decision-making of participants, trustees and investors. Within this broad program some themes have been explicitly – or perhaps “traditionally”--geographical in the sense of exploring the spatial dimensions of differentiation and inequality (Clark and Knox-Hayes 2007, Strauss and Clark 2010). Others have been explicitly theoretical, engaging critically with increasingly influential behavioral economic research on pension decision-making (Strauss 2006, 2009; Clark 2010). Perhaps tellingly, however, it is the empirical, mostly quantitative explorations of under-researched issues (e.g. attitudes to choice and risk, and the role of socio-economic status in decision-making in UK defined contribution pensions), often grounded in new primary datasets, that have garnered the most attention in policy circles and outside the field of geography (see for example, Clark, et al 2008; Clark, et al. 2010; Clark and Strauss 2008). In terms of diversity, the broader pension project--like much in economic

geography--draws on other disciplines, engaging geographical concepts and theories in (often critical) conversation. This means that it is not always easy to distinguish a geographical “core” to the research, unless one counts the critique of a-spatial models and modes of theorizing. Nevertheless, to those on the “inside”, the ways in which this research is grounded in an understanding of “the economic” that encompasses the importance of context, and a fundamental concern with economic inequality and social justice, to us reveals its lineage in critical and feminist economic geography.

The question is, then, if economic geographers undertake engaged and diverse research projects—why is there a sense that we are prone to “miss the boat”? For example, what have been the contributions to date from geographers in analyzing the unprecedented attack on the post-war model of public sector pension provision in the U.S. and UK? From our perspective there are a number of contributing factors. Early career academics must focus on publishing papers in peer-reviewed journals if they wish to be employable in an increasingly competitive and international job market. And, as in other disciplines, the realities of academic publishing mean that many papers will take between 12 and 24 months to be published. A paper that first explored some of the implications of the financial crisis for pension funds, for example, was initiated in 2008, accepted for publication in 2009, and as finally published in mid-2011 (Strauss, 2011). This slowness is intensified by geographers’ commitments to both collect primary data *and* interrogate a wide range of theoretical frameworks. The latter is important for the longer-term strategic process of “an alternative opening up of economic analysis”, and former is important for “engaging” with economies and economic problems in all their “real world” complexity” (Peck, 2005, 161). Together these commitments can divert effort away from ongoing and sustained analysis of secondary data (e.g. government statistics on pension fund



deficits, rates of coverage, and pension inequalities.) This analysis, however, is what garners the immediate attention of policy makers and is likely to be of help to groups engaged in campaigning on issues related to pension inequality especially post-crisis. Sustaining a research agenda that combines these foci ideally requires sound training in quantitative and qualitative methods, the latter now encompassing participatory, visual, restorative and ethnographic methods, as well as media training. This is a hugely challenging pedagogical issue.

Thus, the vibrancy, openness and political commitments that initially attracted us to economic geography have now become challenging goals we aspire to reproduce and expand in the context of our own academic praxis. The attempted critical engagement with behavioural economics, for example, remains stubbornly one-sided, but is that an indication of the limits to heterodoxy in the mainstream of the economics discipline? Pensions are deeply political, a lens through which to view processes and institutions of the economy and the welfare state, including--or perhaps especially--during times of economic and financial crisis. Orthodox economics and the strong model of economic rationality co-construct the ideology of individualism that is undermining collective approaches to welfare provision. This is the political logic of its disciplinary--in the double sense--power. Economic geography, to paraphrase John Holloway, should change the world without seizing power.

### **The local context of crisis**

We find similar challenges in economic geography's ongoing attempts to explain the impacts of crisis on development trajectories in industrial clusters. Economic geographers have enjoyed substantial influence on the development of theories and policies relating to clusters.

However, while classically geographic works like Saxenian (1994) have no doubt contributed to the proliferation of Silicon Valleys, Alleys, Harbors, Islands, and Glens, the work of

management scholar Michael Porter (1998) remains the most influential across disciplines and outside the academy. Porter is influential because he focuses on policy outcomes -- regional economic growth and development - as opposed to geographers who have been “more diffuse in [their] aims” and therefore less useful to policymakers (Martin and Sunley, 2003, 9). Because management is a less theoretically diverse discipline than geography, Porter can focus on “practical” effects and spend less time synthesizing his work with numerous theoretical projects. Meanwhile geographers, often occupied with the fundamental messiness of local clusters and wary of generalization across space and time, find it more difficult to engage in policy debates.

For economic geographers, local context and contingent evolution in the formation and reproduction of clusters take precedence over improving competitive strategies (e.g. Feldman et al. 2005). It is the mingling of institutional, cultural, and organizational influences that creates a specific context from which place-based practices and tacit knowledges emerge (Gertler 1995). While there has been some debate whether economic geographers should concentrate on local contextualization or the creation of generalizable models (see Markusen 1996), generally the trend among economic geographers has been to focus on case studies of specific clusters rather than model building. But in the aftermath of a global financial crisis we need to ask how geographers’ specific, contextualized case studies contribute to a greater understanding of how clusters survive and respond to global or national economic downturns. The challenge is to apply what we have learned from our individual case studies to create a broader understanding of how clusters experienced the crisis without surrendering our “more diffuse aims”.

The Kitchener-Waterloo region (an area better known for high-tech companies like RIM, the maker of the Blackberry cell phone) provides a useful example of this problem. During a study of industrial innovation in the region carried out during the crisis, it quickly became

evident that the present crisis was simply the latest in a series of crises the region had faced over the past decade (Bathelt et al., forthcoming). The region's industries are heavily dependent on both suppliers and customers in the US, and have been hurt by the strengthening Canadian dollar since 2002, which made their products more expensive. While the 2008 financial crisis certainly hurt many of the local manufactures who supplied the auto industry, other firms diversified their markets in response to the previous rounds of crisis and were successfully weathering the storm.

In other words, regardless of the global character of the financial crisis (French, et al. 2009), Kitchener-Waterloo's experiences are unique to the region. Economic geographers, however, need to make clear that these individual case studies speak to the wider study of clusters and regions. Kitchener-Waterloo illustrates that a cluster's response to one crisis is, in part, a reflection of previous crises. Other industrial clusters, such as those throughout the Rust Belt and the English Midlands, are in the same position as Kitchener-Waterloo. Still others, as diverse as high tech firms in Silicon Valley, small textile shops in Northern Italy and assemblers in Guangzhou, experienced the present crisis differently, but still through the context of previous crises.

When addressing the needs of clusters during the crisis, economic geographers need to emphasize the importance of understanding the cluster's particular history. This does not necessarily imply a strictly evolutionary perspective, but rather an understanding that the past influences the future. Storper and Scott (2009) argue that regional economic dynamism lies in the harmony between local resources, institutions and skills. Economic geographers' qualitative, contextual research methods and paradigms give us the tools to investigate a cluster's history and dynamism to identify potential matches between local resources and talents. Cluster researchers, like Michael Porter (2008), who largely lack this perspective, are instead left to argue for large-

scale national re-regulation to preserve competitiveness, often overlooking clusters' and regions' particular needs. Economic geographers may indeed be challenged to engage development policy at the scales that Porter promotes due to our lack of a clear disciplinary core that would allow for a unified, universal response to current events. However, we argue that geographers are eminently prepared to offer relevant economic development analysis to actually existing localized economies. The challenge is convincing local partners in regional governments and institutions to pay more attention even though we rarely offer the same neatly packaged solutions as our colleagues in economics and management.

### **Latin America, crisis and the space of flows**

We lastly want to point out that most discussion of the 2008 global financial crisis in economic geography and proximate disciplines as well as the mainstream press, has been centered on the United States and Europe. But how and where did the crisis spread? While the initial crisis caused a widespread economic shock and subsequent slowdown in growth, distinct global regions have not experienced the crisis in the same way. Realizing it is a large and diverse region, we briefly consider why Latin America generally experienced a much less severe crisis compared to the U.S. and Europe. In particular we suggest it is only through considering both the grounded and interconnected nature of Latin American economies at the same time that a full conception of that crisis can be grasped. In other words, we suggest economic geographers' tendency toward *relational* analysis (Yeung 2005) provides a crucial and unique perspective on financial crisis even though it sometimes results in conclusions that are "messier" than more straightforward methodologies. This is crucial since the only way one might successfully grasp the complexity of the relationship between inter-regional financial flows and particular place based economies, is through a diverse and pluralistic discipline capable of

juggling analysis of a grounded “real” economy with unorthodox economic ideas such as “the space of flows” (Castells 1996).

In the case of Latin America, reliance on maritime trade and primary commodity exports protected it from the long term downturn experienced in the U.S. and Europe. International trade did contract in the wake of the crisis, and this contributed to a significant economic slowdown in the region, but it turned out to be short-lived. Latin America’s previous unfortunate experience with financial crisis (World Bank 2010, Jara et al. 2009), coupled with expanding partnerships with China, Russia, Brazil and other ‘emerging markets’, meant it was also one of the first regions to return to robust economic growth after the crisis. The rise of maritime containerization, much like the rise of financial markets themselves, represents just such a shift in contemporary society toward a world of flows facilitated by information technologies (Taylor 2004). It is precisely the non-centered nature of maritime shipping networks that has contributed to the uneven distribution of gains and losses across the global port network as a result of the global financial crisis (Eyerdam 2010). In fact, some shipping companies conducting trade with ports in the Latin America have been “ordering more capacity than is reasonable and ignoring the (generally negative) market signals that began in late 2007” (United Nations ECLAC 2009, 3). In spite of the negative impact that the financial crisis has had on the shipping industry overall—2009 was the worst year for the industry since the invention of modern cargo transport—sea lanes between Latin America and ‘emerging markets’ in Russia and China for instance, continued to flourish (Eyerdam 2010, 55).

Any analysis of the financial crisis in Latin American, especially South American states, however, must also consider the macroeconomic policy approach undertaken in the wake of the 1997-8 Asian financial crisis and the 2001-2002 Argentinean currency crisis. Among other

things, intense capital flight encouraged many regional governments to more closely monitor and regulate foreign currency flows, asset price bubbles and excessive credit expansion all of which “arguably moderated the risk of a boom-bust cycle” (Gallego, et al. 2010). In addition, Latin American container shipping ports are still commonly described in relation to their host countries’ economies (Ellis 2009). Eyerdam (2010) observes that the Latin American port sector was “down 11% less than the shipping industry overall”, which suggests one cannot look exclusively at shipping networks, but must also consider the grounded economies, in this case based on primary commodity exports, where those networks are embedded.

As economic geographers, we are ideally situated to analyze the distinct placed-based nature of different institutions existing as widely dispersed nodes within a broad network of world commerce. It is the confluence of globe-spanning trade networks with the placed-based nature of economic trading hubs (such as seaports) which we see as a key opportunity for economic geographers to make important contributions. This type of global economic organization – a mix of economic flows across large distances and fixed territorial economies – leads us to better appreciate the realities and relations of economic organization and as such, reactions to crisis. As John Agnew states, “it is not a question of either/or but of how one relates to the other” (2005, 166). Considering so many variables and relations between them, however, does not make for neatly packaged policy prescriptions. The challenge for economic geographers who strive to make the discipline relevant to a broader and non-academic audience, is how to translate relational and network analyses into language that policy makers and other disciplines find useful.

### **Conclusion: Rocking the boat**

“If economic geography is to play a part in challenging these entrenched orthodoxies [neoclassical economics and neoliberal politics], then it will need to find and make common causes, even as it remains a pluralist project” (Peck 2005, 166).

If the SIEG proved anything to us, it is that economic geography is a vibrant, exciting, and creative discipline at the cutting edge of social theory and rethinking “the economic.” But not unlike other instances of disciplinary contemplation, we also sensed a bit of frustration with the seeming infrequency that good geographic work is “impactful” with broader audiences. This takes on even more urgency at a time when tenure-track jobs continue to be sparse, anti-intellectualism is rampant (at least in the U.S.), and economic recession and austerity movements continually shrink higher education budgets. It seems now more than ever we ought to consider economic geography’s relevance.

This is the context in which we authored this article, and our main motivation was to explore both the up and downside to expanding pluralism. To state it simply, our conclusion is there are trade-offs to the expanding diversity of the discipline. Most notably it is increasingly difficult to pin-point the discipline’s contributions to contemporary debates. The “missing the boat” accusation is just one of many related critiques about relevance. While we certainly agree, in this particular case, that more geographers ought to find common causes in researching finance, financialization, and financial crisis, we do not think the accusation is a fair one. If economic geography was less diverse and more “disciplined”, it might be possible to more quickly and succinctly engage issues like the causes and consequences of the financial crisis. But we suspect it would also be more difficult to square our political and intellectual commitments. After all, the challenges of theoretical and methodological diversity in economic geography should not be reduced to a choice between over-simplifying our explanations or giving up on potential engagements. Rather it should be about expanding intellectual

conversation, questioning dead-end assumptions, and engaging a wide range of contemporary problems related to finance and crisis. Perhaps a better question is what boat we want to be on? If “catching the boat” requires quick, definitive answers to complex socio-spatial issues, answers that all too often leave crisis-causing orthodoxies untrammelled, we may embark, but only on a collision course with neo-classical orthodoxy that we suspect will leave economic geography sunk. On the other hand, we believe using innovative methodologies to help drive forward debates about the type, role and accountability of financial institutions as well as the part that finance capital plays in processes of globalization, de-industrialization and economic restructuring are worthy disciplinary goals, even if they take decades to articulate. The speed may be slower, the launches less dramatic and the seas rougher than some, especially early career geographers, prefer but we think the benefits justify the risks.

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