Editorial
Remaking Economic Geography: Insights from East Asia

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In 2003, George Lin and I co-edited a special issue of Economic Geography that aimed to situate and encourage economic-geographical research in the context of rapidly “Rising Asia”. We argued for new ways of theorizing economic geographies of Asia (Yeung and Lin, 2003) and included four empirical papers grounded in different theoretical perspectives – industrial clusters, regulation theory, politics of scale, and corporate embeddedness. Five years have lapsed since these papers were originally presented in the annual meeting of the Association of American Geographers in Los Angeles in March 2002. What we now witness is the rapid proliferation of economic-geographical research on dynamic Asia, particularly East Asia comprising both Northeast Asia (e.g. Greater China, Japan, and South Korea) and Southeast Asia (e.g. Malaysia, Singapore, and Thailand). During this period, several major events dominated news of the day, including the infamous outbreak of SARS (Severe Acute Respiratory Syndrome) in China, Hong Kong, and Singapore in the first half of 2003. Despite this major set-back for Asia that was coupled with terrorism and its counter-military operations on a global scale, economic growth and transformation in East Asia has continued unabated at unprecedented rates.

Critical engagements: how does East Asia matter (again)?

This phenomenon of “Dynamic East Asia” provides a great research opportunity for (re)producing economic geography – a discipline with considerable research interest in change and transformation in specific locales ranging from macro-regions and nation-states to sub-national regions, cities, and rural districts. As economic geography is grappling with the perceived lack of new innovative ideas and/or empirical research foci, “Dynamic East Asia” may well serve as one of the key sites for critical engagements among economic geographers and their allies in other social sciences. How does East Asia matter then? There are clearly material and discursive dimensions to the growing significance of East Asia in our economic-geographical imaginations. In terms of material reality, our conceptual and empirical understanding of developed regions in OECD countries may have reached a certain level of “theoretical saturation” (à la Glaser and Strauss’ 1967 grounded theory) and empirical replication-cum-repetition. Engaging our research with East Asia can potentially help us break away from the genuine possibility of such theoretical saturation becoming a theoretical stalemate.

Let me give one brief example from Japan to underscore the importance of this critical engagement with East Asia and our need for significant intellectual investment in relation to

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1 This editorial represents solely my personal opinions and in no way does it reflect the opinions and positions of my fellow co-editors and the journal itself. I would like to thank Yuko Aoyama and Amy Glasmeier for their insightful comments on earlier drafts.
other major social science disciplines. Economic geographers’ interest in Japan during the period between its meteoric rise in the 1960s and the 1970s and the collapse of its “bubble economy” after 1989 represents a brief interlude in our research trajectory. Whereas scholars from other social sciences such as economics (Masahiko Aoki and Kiyoshi Kojima), political science (Chalmer Johnson, Walter Hatch, and Peter Katzenstein), and sociology (Ronald Dore and Ezra Vogel) made significant stride in theory development by engaging head-on with the rise of Japan Inc., economic geographers were ironically rather reticent and held back their theoretical “experimental sites”. Of the limited research on Japan’s economic geographies, it was mostly about the spatial organization of Japanese trade and investment in the host regions in North America and Western Europe; there was not much concerted and theoretically grounded economic-geographical research on Japan itself. With the usual caveats on simple database analysis, a quick search of Economic Geography shows that between 1925 and 2001, there were only 27 articles with “Japan” or “Japanese” in their titles: 15 of them published after 1960, three of them in the 1960s, and none in the 1970s(!). Of the remaining 12 articles published after 1980, only two were not about Japanese trade and/or manufacturing transplants in North America and Western Europe (http://www.jstor.org, accessed on 7 August 2007). If we include other leading geographical journals, the Japanese economy was the analytical focus of a handful of articles published mostly in the 1990s (e.g. Patchell, 1993a; 1993b; Peck and Miyamachi, 1994; Webber, 1998).

True enough, the other social sciences often had specific geopolitical imperatives and overtures in their research; their strong interest in the rise and transformation of Japan itself had the political underpinning of “Japan Learning” (corporate restructuring towards new production systems) and “Japan Bashing” (trade protection and reversing regional decline) in their home countries. They consequently made a significant streak of solid conceptual advancement and empirical findings that ranged from developmental state configuration and networked business organization to corporate cultures and labour market dynamics. Still, these social sciences fell prey to the law of “contingency research”: massive research on a phenomenon only when it became a major geopolitical-economic issue. By the early 1990s, as Dicken (2007: 44) aptly observes, the “United States’ fear of the Japanese threat receded; the ‘bash Japan’ literature virtually disappeared”. What did we do in this first golden research opportunity arising from “Dynamic East Asia”? With hindsight, I think economic geography could have ventured more significantly into the “Rise of Japan” and engaged seriously with its subsequent decline and stagnation. In short, we could have been more actively involved in this phase of “Dynamic East Asia” through a deeper understanding of Asia itself.

While we clearly cannot turn back the clock, economic geographers need not miss the current second chance of partaking of “Dynamic East Asia”. In fact, I would argue that the sheer pace and rapidity of this regional dynamics provides an extremely fertile ground for new conceptual development, methodological innovation, and empirical insights. Our successful understanding of “Dynamic East Asia” will almost certainly help us move forward the subject matter. This conviction is increasingly evident in our academic discourses. For example, my personal recollection from the experience of organizing both Global Conferences on Economic Geography in Singapore (December 2000) and Beijing, China (June 2007) points to the tremendous interest among hundreds of participants in “Dynamic East Asia”. These conferences also symbolize the crucial role of East Asia in hosting discursive exchanges among economic geographers (see also Olds, 2001; Olds and Poon, 2002). With these positive material and discursive contexts, we can now engage critically with “Dynamic East Asia” in order to “theorize back” at the dominant discourses in economic geography. Economic-geographical research on East Asia increasingly helps the advancement of new
knowledge and reproduction of our discipline. In the next section, I will detail some of these recent efforts in remaking economic geography through new insights from East Asia.

(Re)made in East Asia: how to “theorize back”?

This section seeks to revisit some of the key concepts and ideas in economic geography that can be made and remade in the context of research on East Asia. In the limited space of this editorial, I can only provide a cursory re-reading of these key concepts and introduce the following four theoretically grounded empirical papers on East Asia in this issue of *Economic Geography*. To me, “theorizing back” at Anglo-American economic geography can be understood as either making original theory that emanates from research on sites outside Anglo-American countries or remaking key economic-geographical concepts in light of new insights from East Asia. This challenging task of “theorizing back” is particularly important in today’s global economy where Made in X (X=an East Asian economy) has become so ubiquitous in our everyday life. So exactly how does “theorizing back” work in practice? Let me start with Jim Glassman’s (2007) insightful analysis of Thailand’s eventful recovery from being one of the worst hit economies during the 1997/1998 Asian financial crisis. In this account based on original field research and official statistics, Glassman mobilizes and adds significant value to one of the best known concepts in Marxist geography first developed by David Harvey (1982) – capital’s spatial fix. In retrospect, this now-classic geographical concept has been extremely influential in Anglo-American geography during the past two and a half decades, stimulating several generations of Marxian scholarship that offer a solid geographical take on capitalist global restructuring. In his original formulation, Harvey (1982) argues that capital actively seeks to mitigate the problems of accumulation (e.g. decreasing profits) through two forms of spatial fixes: new investment in the built environment (temporal deferral) and geographical expansion into new production sites (spatial displacement).

To many Marxist scholars, the rise of East Asia as a global factory can often be “read off” as an empirical proof of the working of these spatial fixes, particularly in relation to the rise of the new international division of labour (cf. Fröbel et al., 1980). Indeed, East Asia is an after thought in Harvey’s original conceptualization, as he was not particularly concerned with the empirical specificity of these spatial fixes – a messy research task happily relegated to “area studies” specialists. This dominant epistemological approach to theorizing in Anglo-American geography – abstraction without empirical specificity and validation – is often not that helpful when it comes to understanding “Dynamic East Asia”. Glassman’s work shows clearly that the “fixing” of spatial fix is indeed a highly contested phenomenon contingent as much on internal political alliances and social struggles. As a sequel to his earlier paper published in *Economic Geography* (Glassman, 2001), Glassman wants to develop new “ways of reading spatial fixes politically” (p.xx; original italics). This is a very significant step towards the remaking of an influential concept. The paper illustrates how this fixing is not only driven by abstract capitalist logic(s) often emanating from the home economy, but equally important enabled by context-specific factors in the host of this capital – Thailand. To Glassman, the case of Thailand therefore points to the fact that “the variants of the spatial fix that often hold the greatest sway are not those classical Marxism and Left Keynesianism have most emphasized” (p.xx). In particular, Thailand’s post-crisis recovery has taken what Glassman characterizes as “the low road fix” of spatial relocation within Thailand to reduce production costs and continual export of low value-added products (see also Webber and Rigby, 1996).

Glassman’s work represents a very useful demonstration of how research on sites outside Anglo-American countries can help “theorize back” at key concepts, as the
internationalization of capital as a form of spatial fix necessarily entails both source countries and recipient countries. Since the concept was originally developed through research on these source countries, particularly in the US and the UK, it does not offer much into the grounding and local specificity of capital’s global reach. As evident in Glassman’s paper, fresh theoretical and empirical insights can now be gleaned from economic-geographical research on capital’s recipients that are almost always going to be such developing economies as those in East Asia. In fact, I will argue that this host perspective offers as much insight into the internationalization of capital as the home perspective. A complete analysis of capital’s internationalization as a form of spatial fix clearly entails both perspectives; this is exactly when “theorizing back” becomes such an important and critical link in the entire project.

Almost parallel to Harvey’s (1982) classic work and the ensuing “Marxist turn” that came to dominate economic geography for the next 15-20 years, there was another major theory development in the field of international political economy (IPE) during the 1980s and the early 1990s. This IPE literature sought to explain differential capitalist development in the global economy primarily at two spatial scales: national and global. At the national scale, some IPE scholars identified distinctive pathways to industrialization in East Asia (and elsewhere in Latin America) that were driven largely by state-centric policy initiatives and institutions. The developmental state, as evident in Japan, Taiwan, South Korea, and Singapore, emerged as the key concept in this rich strand of theoretically informed empirical research (Johnson, 1982; Amsden, 1989; Haggard, 1990; Wade, 1990; Hatch and Yamamura, 1996; see also Evans, 1979; 1995). As argued in Yeung and Lin (2003) and earlier in this section, this theory development is a good example of “Made in East Asia” concepts. Into the 1990s, the IPE literature experienced another leap in theory development when its analytical focus was shifted to the global scale. Here, crucial insights from earlier nationally specific studies fed into the wider debate on the nature and diversity of global capitalism(s), commonly known as the “varieties of capitalism” (VoC) literature (Whitley, 1992; 1999; Berger and Dore, 1996; Crouch and Streeck, 1997; Hall and Soskice, 2001; Lane and Myant, 2007). In this literature, we see “theorizing back” with a vengeance. Theories of capitalist states not only were made in East Asia and now continental Europe, but they also destabilized the conventional notion of Anglo-American capitalism often taken for granted as the “default” form of global capitalism. A global mosaic of different varieties of capitalism was said to exist.

Where are the economic geographers in this burgeoning and influential IPE literature? As Peck and Theodore (forthcoming) have passionately argued, economic-geographical research has been mostly missing in this important IPE literature (see also Dicken, 2004 for another example of geographers missing the boat – globalization). Whilst East Asian political economy is featured prominently in this IPE literature, economic geography has gone into post-Fordism, regulation theory, and, more recently, neoliberalism. Indeed, few economic geographers have directly engaged with this VoC literature (e.g. Dicken, 1995; Yeung, 2000; 2004; Peck and Theodore, forthcoming). To Peck and Theodore (forthcoming), geographers’ reluctance to engage with this literature might be linked to their perception that the VoC approach is limited by its methodological nationalism and a tendency towards static analysis and latent institutional functionalism. While there is some validity to this criticism, I think economic geographers are well positioned to move the VoC literature forward beyond these inherent limitations. This state of mutual exclusion between the IPE community and

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2 Interestingly, one of the key proponents of regulation theory, Robert Boyer, is heavily involved in the VoC literature (see Boyer and Drache, 1996; Hollingsworth and Boyer, 1997).
economic geography does not have to persist in future. As exemplified in the papers in this issue, there are at least two emerging possibilities of developing further theoretical insights from economic-geographical research on East Asia. First, our research on the geopolitics of East Asian development can shed light on the changing spatial configurations of territorial development. For example, since the early 1990s, political scientists and political geographers alike have called for new ways of (re)thinking about the state beyond the national scale. In a highly influential article, John Ruggie (1993) advocates for an analysis of the modern nation-state in international relations and IPE beyond the boundary of state territoriality. His work is strongly echoed in the work of leading political geographers such as John Agnew (1994; 2005; see also Agnew and Corbridge, 1995) and Peter Taylor (1994; 1995; 2007). These geographers have conceptualized this theoretical bias in international relations theory towards the state as a container as a form of “territorial trap”.

Here, East Asia can provide very useful insights to help us get out of this “territorial trap”. Glassman’s (1999) earlier work shows how Thailand’s post-War development was directly linked to the internationalization of its state that in turn paved way for the internationalization of capital into Thailand (see also Glassman, 2001; 2007). Another Southeast Asian state, Singapore, represents perhaps the most extreme form of state beyond the “territorial trap”. Since its inauguration on 9 August 1965, Singapore’s developmental state has not only actively internationalized itself in the global arena, but also embraced international capital in ways that no other open economies in the modern era have endeavoured (see Rodan, 1989). What distinguishes Singapore from Thailand, however, is its active outward internationalization of capital and transborder economic cooperation strategically promoted by state policies and initiatives. Here, we witness state beyond the “territorial trap” par excellence.

Extending earlier geographical work by Willis and Yeoh (1998), Yeung (1998a; 1999), Olds and Yeung (2004), Bunnell et al. (2006), Nick Phelps’ (2007) paper in this issue demonstrates how the state, in this case Singapore, can script globalization in ways that potentially bring economic benefits to its citizenry. His research on Singapore’s state-sponsored industrial parks in China, Vietnam, Indonesia, and India shows that while the state may gain from its active “authoring” of globalization’s multi-faceted tendencies (see also Panitch, 1996; Yeung, 1998b), this gain might well be limited and difficult as its extra-territorial strategies are often not easily replicable in other geographical contexts. Phelps’ paper thus contributes to a more nuanced reading of the state’s efforts in transcending the “territorial trap”. To him, some of the apparatus deployed by the Singaporean state in its strategy of extra-territoriality “would be largely unthinkable and probably impossible for most western developed nations in which the maturity of national territorial demarcations has left states ‘trapped’ geographically” (p.xx). Coupled with Glassman’s (1999) work on Thailand, Phelps’ important finding points to the possibility that the concept “territorial trap” might be much more rooted in the Westphalian system of states commonly associated with advanced industrialized economies outside East Asia. Again, their work helps “theorize back” at this literature of state territoriality and provides the necessary correction to its possible conceptual bias.

The second emerging possibility of economic-geographical research on East Asia refers to potential theoretical contributions to the VoC literature. By grounding varieties of capitalism in different East Asian national and sub-national economies, this research offers a much more geographically nuanced understanding of not just different forms of nationally based capitalisms in the global economy and thereby helps us transcend the limit of methodological nationalism in the VoC literature. More importantly, it shows how these different capitalist
forms can intermesh and morph into a new form of hybrid capitalism at different spatial scales, transcending another weakness of the VoC literature in terms of its tendency towards latent institutional functionalism. In particular, transborder flows of economic activity among East Asian economies are critical to this process of hybrid capitalism. While my earlier work shows how ethnic Chinese capitalism becomes increasingly hybridized in a global era (Yeung, 2004), Chun Yang’s (2007) paper in this issue zooms into a specific locale, Dongguan in southern China, and examines how varieties of capitalist practices embodied in manufacturing investors from Hong Kong and Taiwan can be reproduced at the sub-national scale in mainland China. Her paper complements Phelps’ argument for state’s extra-territoriality and showcases how business systems and political-economic alliances in Hong Kong and Taiwan have left strong “home” imprints on their domestic firms, even when they operate in a transitional socialist economy characterized by Lin (1997) as “red capitalism”. Akin to the work by Glassman and Phelps, Yang’s paper offers a useful geographical analysis of the divergent processes and outcomes of transformation towards market economy in Greater China. It connects well with the recent initiative in “theorizing back” at the VoC literature by drawing upon research sites outside North America and Western Europe (Lane and Myant, 2007).

As noted earlier, economic geography during the early 1990s was heavily involved in developing what was subsequently termed an “institutional turn”, even though it did not connect directly with the IPE literature (see MacLeod, 2001). Taking its antecedents from the heated debate on post-Fordism and flexible specialization, this institutional economic geography focused explicitly on industrial districts (Amin and Thrift, 1992), regional restructuring (Grabher, 1993; Amin and Thrift, 1994), and learning regions as a form of new regionalism (Asheim, 1996; Morgan, 1997). This literature has grown massively during the past ten years and it is certainly beyond the scope of this editorial to provide even a brief review (see Lagendijk, 2006; Harrison, 2007). Suffice to say that with hindsight, this literature might have focused too much on the triumphal stories of successful regions such as Silicon Valley and Third Italy, neglecting possible lock-in effects and subsequently painful restructuring experienced by most regions. The literature has also underestimated the crucial role of trans-local forces in the dynamics of local and regional development.

In this context, the fourth paper in this issue by Dennis Wei and his Chinese colleagues (Wei et al., 2007) showcases the rise and fall and rise again of a regional development model in east China in wake of its transition to a socialist market economy. In what is often termed the “Wenzhou model” in the China studies circle, small-scale and often family-owned businesses spearheaded local development in collaboration with the dense networks of local institutions. As Wei et al. argue, this model very much resembles Alfred Marshall’s well known description of an industrial district. What is particularly interesting in the case of Wenzhou is that in times of changing institutional environment and intensifying regional competition, these firms have gone through two rounds of restructuring in order to break out of the regional lock-in effects – organizational change and delocalization to metropolitan areas. Both processes of spatial restructuring have transformed the Wenzhou model into one increasingly dominated by large and multi-regional firms. Their paper thus “theorizes back” at the new regionalism literature in economic geography by pinpointing the conceptual naivety of an overt emphasis on small firms and local relational assets as the pathway to sustainable regional development. Their study also echoes recent efforts by some economic geographers to “globalize” or “scale up” our understanding of local and regional development (e.g. MacKinnon et al., 2002; Coe et al., 2004; Yeung, 2005).
Globalizing economic geography: why bother?

I have so far explained briefly how “Dynamic East Asia” matters in economic geography’s critical engagements with new research sites and how these engagements, as evident in the four papers in this issue, might help us “theorize back” at economic geography and ally disciplines. Let me end this editorial with a tentative assessment of the key issues and prospects for future economic-geographical research on East Asia. My overall impression is that the prospects are excellent because of the fortuitous juxtaposition of three important factors: strong cadets (people), supportive intellectual environments (institutions), and growing social needs (communities). As evident in our 2003 special issue and this issue of Economic Geography and recent publications in other leading geographical journals, a new generation of economic geographers with strong research focus and interest in East Asia has clearly emerged. These cadets have gone well beyond the application of existing theories to their East Asian examples. More importantly, they are capable of “theorizing back” at these different strands of literature and making original contributions in ways that did not quite exist before. At the same time, institutions in East Asia and elsewhere where these cadets are based are becoming much more aggressive in promoting academic research that leads to high impact results. As theoretical work tends to be more impactful because of its wider readership reach, I expect much more original theoretical work on the back of solid empirical research in East Asia to emanate from this supportive institutional context. Finally, all sort of communities in East Asia have newly found social needs that go well beyond the analytical capacity of their conventional wisdoms and social norms. As many East Asian economies are becoming much more integrated into the global economy, these communities and people have urgent social needs to understand their changing contexts and environments in order to make better informed decisions for themselves and others. Here, economic geographers’ well-meaning attention to local specificity and contextualized details tends to give us a much stronger mandate and legitimacy to help these communities through knowledge development and mutual understanding.

It is one thing for me to paint such a rosy picture; it is however quite another thing to relate back to economic geography “proper”. I think there are real and significant issues related to East Asia that need to be addressed by our collective endeavour. This may require us to return to big ideas, ambitious agendas, and large-scale projects. Recent transformations in East Asia alone provide a very serious reality check. If we bring in other important and yet overlooked regions such as South Asia, the Middle East, much of Africa, and South America, I think our plates will almost certainly be very full and our relentless desire for and pursuit of the mundane and the idiosyncratic in our “backyard” can be put into an appropriate perspective. In this regard, I can think of three very significant issues in the economic geographies of East Asia (and elsewhere) that merit our attention both in terms of theory development and empirical investigation: (1) mega economic transformations; (2) East Asia’s impact on global politics and social change; and (3) environmental challenges. The first issue comes naturally as we, economic geographers, are dealing with the economic as our default subject matter. East Asia’s continuous economic transformations, though impressive as it often seems, can pose significant risk to the entire global economy. Just imagine China dumping all its US Dollars and Treasury Bills holdings and the global financial market will be in serious chaos much worst than the current sub-prime woe in the US. Here, we need economic-geographical research that helps us understand better these intricate interconnections between East Asian economies and the global economy. The experience of the Asian economic turmoil tells us that we do not necessarily get from economics science good and appropriate answers to these questions.
Equally, the second issue is far too important to be left to political scientists and international relations scholars. As evidently shown in both Glassman and Phelps’ papers, geopolitical overture can impact seriously on economic processes and vice versa. Their work and others in economic geography have pointed to our capability and responsibility in resurrecting both the political and the economic in our conduct of economic-geographical research. After an interlude of several centuries, the re-emergence of Asia in global politics – a phenomenon termed ReORIENT by the late Andre Frank (1998) – might just wake us up from our comfortable positions in the global order. Finally, economic geographers are perhaps quite good with economic processes; but we are generally incapacitated when it comes to the environment – clearly one of the weakest links in our intellectual endeavour. The rise of East Asia poses one of the most significant environmental challenges in human history. According to some estimates, China has now overtaken the US as the largest polluter in the world and its environmental record is not at all impressive. In Southeast Asia, annual haze arising from the slash and burn method of forest clearance in Indonesia has consistently engulfed the region year after year. Where are the economic geographers again? Perhaps we are too inhibited by our very own intra-disciplinary boundaries. My own belief is that such boundaries, by their virtue of existence, are meant to be crossed! The future of economic geography hinges on us bringing the environment back into the economy and vice versa. Doing so requires us to develop conceptual frameworks that bridge both fields (e.g. Taylor, 1996; Robbins and Sharp, 2003; Schoenberger, 2003; Mansfield, 2004; Angel, 2006; Gibbs, 2006). In other words, there is much “theorizing back” within different subfields of Geography that needs to be done. Again, East Asia provides a fertile research ground for such epistemological engagement to take place.

By raising the above challenging issues in the context of East Asia, I hope to provoke a greater concern with “theorizing back” in our future work in economic geography. At the end of the day, I believe it is not just a question of doing economic geography for fun and personal satisfaction (see some reflections in Tickell et al., 2007). As this admittedly narrow editorial shows, economic geography should provide answers to very challenging issues and questions that are simply too important to be left to other disciplines. We need to get out of the mindset of self-chastised reluctance to engage with big theory and large questions. We need to remake a kind of economic geography that takes a globalizing perspective without fear. “Theorizing back”, as practised in the papers presented in this issue, is but the beginning of this not-so-modest enterprise of globalizing economic geography.

References


