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Special Issue: Methodological Issues in Macro Comparative Research

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METHODOLOGICAL ISSUES IN MACRO COMPARATIVE RESEARCH: AN INTRODUCTION

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This special issue of JWSR is the offspring of an ASA Political Economy of the World System session that I organized in 2007. My thanks to Andrew Jorgenson, co-editor of JWSR, who moderated the session and proposed that I put together a special issue on this topic. In turn, I asked Timothy Moran to join me as co-editor of this issue. Tim is one of the foremost quantitative macro-comparative sociologists in the country, and was the discussant on the PEWS panel. Tim provides a summary and discussion of the contributions in the conclusion. As it turns out, only two of the panel presentations are included in this issue. The other two were submitted in response to a general call for papers. All four manuscripts were peer reviewed.

Three of the articles in this issue overlap in ways that might not readily be apparent by their respective titles. Pablo Lapegna's article, "Ethnographers of the World...United? Current Debates on the Ethnographic Study of 'Globalization'," explores the ways in which field studies of individuals and locales can do more than inform our understanding of global processes. Lapegna argues that these studies are necessary to counter the notion that local processes are simply the homogenous outcomes of macro-level dynamics. Lapegna rejects the "local-global" duality, arguing that social organizations at any level, be it a small village or megalopolis, need to be considered in their own right. Ethnographies provide a "view from below" that understands these hierarchical linkages as a dynamic interactive ones, where actors at the local and national levels modify global "projects" in light of area specific conditions and cultures. In the second half of the paper, Lapegna provides a detailed discussion of the current debate surrounding two types of ethnographic study; multisite versus global perspectives. Lapegna does not take sides on this issue, leaving it instead to the reader to navigate these issues.

In "Puzzles in the Comparative Study of Frontiers: Problems, Some Solutions, and Methodological Implications" Thomas Hall considers the role of "frontiers" in macro-comparative research. In Hall's view, frontiers are an inherent characteristic of all world systems, which are constantly expanding, contracting, and interacting with other world systems over time and space. As such, frontiers provide a useful window on the changing dynamics of these relationships. Hall's basic point is that frontiers can provide a useful mechanism for connecting the local and the global, both from the bottom-up and top-down. In other words, the study of frontier social processes can inform world-systemic processes as well as the ways in which world systems analysis can inform local human agency. Hall begins with a discussion of the definition(s) of a "frontier." He first tells us what a frontier is not. It isn't a border or boundary between two countries, nor is it the overlap between two states. The emphasis here is that it is not static or stationary. Rather, they are fundamentally indeterminate or "fuzzy." Hall stresses the active and changing nature of frontiers. His favorite definition is drawn from the metaphor of a biological membrane. Both are selectively permeable in terms of direction, content and quantity.

Hall goes on to discuss in depth the characteristics of frontiers, emphasizing the (world-system) context and scale that determine the nature of these zones of interactions.

Lloyd, Mahutga, and De Leeuw's article on, "Looking Back and Forging Ahead: Thirty Years of Social Network Research on the World-System," is an impressive review of the application of social network analysis (SNA) to world-systems analysis that will undoubtedly find its way onto many graduate student preliminary exam reading lists for its clear and thorough summary of the key concepts, benefits, and drawbacks, to using SNA to empirically study world systems processes. I will certainly add it to ours. LM&D begin by exploring the notion of a "relational-structural" methodology that they argue is particularly useful in studying these global processes. They incorporate into this discussion a comprehensive review of the relevant literature. LM&D close with their own assessment of the future methodological and substantive directions for SNA and world-systems analysis.

So what do these three articles have in common? All of these papers deal with the nature of the local-global relationship. And they all reject the pervasive tendency to understand the "local" as the passive expression of "global" processes. In its place, these authors stress the reciprocal, interactive nature of this dynamic. This is a welcome advance to macro-comparative research in general, and specifically to orthodox world-systems theory.

Salvatore Babones' contribution, "Modeling Error in Quantitative Macro-Comparative Research" will likely be the most controversial article of this issue. Babones highlights a critical and not widely-recognized nor well-appreciated statistical problem with quantitative longitudinal analyses; the misspecification of error in trended data. Babones provides the reader with a clear and comprehensive primer on some of the major problems encountered in quantitative macro-comparative research, focusing specifically on the issue of modeling error in the data that arise from heterogeneity bias and residual autocorrelation. Babones argues that the currently accepted, "hegemonic" methods of dealing with these issues, fixed and random effects panel models, can generate "wildly spurious results." He calls for a return to traditional "difference" models, in which change over time in the dependent variable is regressed on changes in the independent variable. In his critique, Babones boldly calls into question much of the currently published research in quantitative macro-comparative research, a position that is likely to draw a critical response from those who are using these hierarchical models in their work. This is indeed a crucial issue, and it is unlikely that Babones' article will be the final word on this topic. A critical dialogue between proponents and dissenters would be a worthy topic for another special issue of the Journal of World Systems Research.

ETHNOGRAPHERS OF THE WORLD...UNITED? CURRENT DEBATES ON THE ETHNOGRAPHIC STUDY OF "GLOBALIZATION"¹

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ABSTRACT

How does ethnography come to terms with our current "global condition"? Being a method characterized by its in-depth knowledge of a bounded space, how does ethnography cope with a world scale? How does the "global condition" affect the definitions of key ethnographic concepts? In this article, I first reconstruct ethnographic debates regarding the status of "the global," showing how ethnography can contribute to a more nuanced understanding of the binary global/local. Then I review two projects that study global processes from an ethnographic point of view: multi-site ethnography (Marcus 1995) and global ethnography (Burawoy et al. 2000). I compare these two approaches along four dimensions: site, context, research design and reflexivity. I argue that while multi-site ethnography and global ethnography are often used interchangeably, each ultimately presents distinctive answers to key questions for the ethnographic study of global processes.

INTRODUCTION

If we think of ethnography as a method practiced in a delimited geographical space by engaging in face-to-face interactions, it is hard to imagine how such a "micro-oriented" perspective may render significant insights on something as wide as "the world." Imagined in those terms, the encounter between ethnography and "the global" poses nothing but an oxymoron:

After all, in anthropology, we stereotypically picture the lone ethnographer settled in his or her village, itself isolated from the world around. In sociology, we think of the ethnographer as the specialist of face-to-face relations or of situational analysis, but with the context firmly bracketed. (Burawoy 2001:147)

How do ethnographers deal with this problem of scale? How can an "intimate" method such as ethnography observe processes spanning the globe? Despite the oxymoronic nature of the enterprise, a series of scholars have analyzed global processes ethnographically (e.g. Ferguson

¹ I am grateful to Eileen Otis, Javier Auyero and Tim Moran for their critical comments on an earlier version of this paper. Any mistakes and omissions are mine.

1994; Goldman 2001; Hannerz 1996, 2003; Comaroff and Comaroff 2003; Tsing 2005; Englund 2002) engaging cultural analyses of globalization and transnationalism (e.g. Kearney 1995; Appadurai 1996; Mintz 1998; Amselle 2002; Ong and Collier 2005). In this article I concentrate on the theoretical and epistemological debates surrounding ethnographic analyses of global processes by posing two main questions about ethnographic research on “globalization.”² How do ethnographers understand global processes? How does the “global condition” affect traditional definitions ethnographic research? How can we understand fieldwork and its concomitant theoretical and epistemological problems in the light of our “global condition?” To address these questions, in the first part of the paper I analyze how ethnographers have solved the conundrum of using a method traditionally performed in small scale societies to address the global. I pay particular attention to ethnographers’ criticisms of analyses of “the global” grounded in an over-deterministic political economy. I then examine how these ethnographers resolve the global/local binarisms that inform so many studies on global processes. Then, in the second part of the paper, I scrutinize two contrasting perspectives: multi-site ethnography (MSE) and global ethnography (GE). I concentrate on MSE and GE because they represent well-articulated and predominating methodological approaches to the study of globalization. While others have investigated the global from an ethnographic perspective, they do not propose a broader framework suitable for adoption by other researchers, as MSE and GE have done. I closely compare the ways in which MSE and GE approach four key issues in the ethnographic study of globalization: sites, context, research design and reflexivity. Although the analysis of these terms does not exhaust the possible lines of inquiry, they are the major methodological arenas in which ethnographers confront the challenges of incorporating an analysis of global processes. They all point out to emergent problems of ethnographic research regarding fieldwork and the production of theory and texts (e.g. Clifford and Marcus 1986; Van Maanen 1995; Strathern 1995; Amit 2000).

TAPPING ON THE OXYMORON

Can ethnography help achieve a better understanding of global processes? In this section, I discuss how ethnographers³ have conceptualized “the global” by addressing two interrelated problems. First, I will show how ethnographers have developed an alternative perspective on globalization by reviewing their major critiques of analyses of global processes rooted in political economic, organizational and sociological research. Second, I reconstruct the debates within ethnography regarding how we theorize and represent “globalization” in ethnographic terms, focusing on how ethnographers overcome the much criticized global/local binary.

Ethnographic perspectives had criticized conceptions grounded in political science and sociology that see the world in terms of diffusion of institutional models and patterns (e.g. Meyer

² I use the word globalization with quotation marks assuming that it is a twofold phenomenon of both processes in the “real world” and an ideological or cultural construct. Here I understand “globalization” as the increased density of interactions across boundaries and between institutions that produce, reward and regulate those interactions.

³ Under “ethnographers” I conflate a variety of scholars that share a methodological approach dominant in anthropology but with critical contributions of sociology, geography and other social sciences.

et al. 1997) and/or in terms of “second modernity” (Giddens 1990). These ideas have one too many points of connection with “modernization” and its assumptions of universal institutional convergence to pass unnoticed from a cultural perspective (Tsing 2000). Ethnographers of the global point to the shortcomings of institutional frameworks of diffusion to consider the role of power in the global adoption of specific institutional models. Likewise, while notions of “structuration” (Giddens 1990) usefully conceptualize globalization as both enabling and constraining process, they fail to specify who is enabled and who is constrained (Burawoy 2000a). For some, the “privileged lifestyle of high-flying academics” overlooks how global processes are seen “from the underside” (Burawoy 2000b:337). “Global ethnographers” also take issue with teleological interpretations of global changes under recent capitalism. In those terms, most ethnographers of global processes seek an alternative to models that view globalization as dynamics that supposedly emanate from economically advanced centers and impose themselves as an external force on peripheries, with monolithic effects across spaces. Along these lines, global ethnographers find the interpretations of David Harvey (1989) and Frederic Jameson (1991) problematic to the extent that they trace the source of globalization to historical shifts in global capitalism that ultimately have homogeneous effects across different geographies. These grand theorists fail to identify how such processes are locally mediated with diverse effects on everyday life (Burawoy 2000).

Indeed, such views also tend to present an evolutionary picture of capitalism (Tsing 2005). Even at a global scale, capitalism is not a monolithic force; we should always bear in mind the paradoxical feature of capitalism: a social formation in continuous change that nevertheless maintains similar principles (Wallerstein 2004). Thus the assumption of a single world-capitalist system should not preclude questions about its heterogeneous formations and the diversity of projects involving global processes. In other words, we must resist the fallacy of globalism, i.e. the tendency to represent recent transformations in the world as processes given once and for all. Globalisms (“endorsements of the importance of the global”) need to be interrogated as a set of projects “with their distinctive cultural commitments and their powerful but limited presence in the world” (Tsing 2000:330, 353). Put another way, the theories that see recent transformations as the gateway of a “new global era” understand those changes as an all-encompassing uniformity, taking as an assumption what in fact should be the point of departure for inquiry. Along those lines, Anna Tsing has argued that “If globalization can be predicted in advance there is nothing to learn from research except how the details support the plan” (2005:3).

As a method that requires immersion in a local setting and direct contact with informants, ethnography provides the ideal tools to investigate the diversities and heterogeneous manifestations of world-wide capitalism. By “ascending from the local to the global” an ethnographic gaze avoids the perils of assuming “that one can characterize changes of the whole without examining changes of the parts or, to put it the other way round, that the secret of the part can be found in the whole” (Burawoy 2000b:343). By placing in the foreground the “passions and the stakes of global connection” we can avoid the “structures of self-fulfillment” promoted by neoliberal globalization and “immerse ourselves in the drama of uncertainty of global capitalism and transnational liberalism” (Tsing 2005:269). Approaching “globalization” from such an ethnographic perspective circumvents the persistent identification of the global with the universal and the local with the particular, a conflation of level of analysis and geographical scale (Gille and Ó Riain 2002:286). Scholars that propose a research agenda based on GE propose that in “multiscalar research we cannot identify a priori a dominant level of analysis. How do we identify

the limits of a community we are studying when the community is constituted across a variety of spatial scales (local, national, global, transnational, etc.)?” (Gille and Ó Riain 2002:286). As an ethnographic assumption, then, we should not take these projects and descriptions as natural “by assuming that the terms they offer us are true” (Tsing 2000:351), but rather, view them as diverse analytic lenses through which globalization is perceived, legitimated, and even constructed.

What, then, can ethnography offer to research on the current “global condition”? An ethnographic focus and its “concern with concrete, lived experience can sharpen the abstractions of globalization theories into more precise and meaningful conceptual tools” (Burawoy et al. 2000:xiv). Ethnographic studies of global process can help, for instance, to counterbalance the overemphasis on circulation of the “globalization” jargon.⁴ The buzzwords of flow and circulation, so often used in market models (Tsing 2000) and the emphasis in connections and mobility to describe global processes should not obscure the *disconnection* and *irrelevance* created by the same phenomena that inspires that language. An overemphasis on an imagery of connection leads Graeber to claim, “most anthropologists feel instantly uneasy with any formulation on globalization that even appears to cheerfully divide the world into those plugged in, and those not” (Graeber 2002:1223). Accordingly, some ethnographers have avoided a reification of mobility inspecting “the immobilities and experiences of confinement” produced by international practices (Navaro-Yashin 2003).⁵ In the same vein, others have argued that certain subjectivities have “escaped the discursive, productive constraints of globalization only because global forces have lacked the flexibility to capture and mobilize this subjectivity – it is not the heroism of resistance, but the abjection produced by a system that leaves people in those ‘unprofitable’ zones crushed and forgotten” (Friedman 2007:423-424).

Accordingly, the attention to connections, disconnections, juxtapositions, forces and associations does not imply a division of the world into “global” and “local” areas. The dichotomy global/local is deemed as highly problematic in the ethnographic literature on global processes. We can hardly see the local “as the stopping point of global circulations” since “if flow itself always involves making terrain, there can be no territorial distinctions between the ‘global’ transcending of place and the ‘local’ making of places” (Tsing 2000:338). Put another way, the distinction between “global” forces and “local” places, “draws us into globalist fantasies by obscuring the ways that the cultural processes of all ‘place’ making and all ‘force’ making are *both* local and global, that is, both socially and culturally particular and productive of widely spreading interactions” (Tsing 2000:352, original emphasis). Michael Burawoy poses this idea stating that “globalization is not a cause but an effect of processes in hierarchical chains that span the world. In this productivist perspective the global-local antinomy is itself misleading, for if something is global there can be nothing outside that is local” (2001:156-7). Accordingly, George Marcus asserts a similar idea proposing “a research design of juxtapositions in which the global is

⁴ “The distinct contribution of ethnography to debates on globalism lies in its capacity to show the actual limits of the fantasies that the globalist imagination produces and the reality of blockades amid global and transnational flows” (Englund 2002:263).

⁵ Studying Northern Cyprus as a “no-man’s land” Navaro-Yashin makes the point that “Anthropologies of globalization in the model of Appadurai’s work fail to study the ways in which the very processes of transnationalism which supposedly promote mobility and flexibility, also engender the opposite: immobility, entrapment, confinement, incarceration” (Navaro-Yashin 2003:108).

collapsed into and made an integral part of parallel, related local situations rather than something monolithic or external to them” (1995:102).

Summing up, the literature focusing on “the global” from an ethnographic point of view seems to agree in that (1) “globalization” is a phenomena in the “real world” *as much* as it is a project (see also Holtzman 2004); i.e. analyses of global process usually intermingle descriptive and prescriptive elements; (2) we should not devise a sort of “division of labor” in which the global picture developed by macro-structural views is “filled in” by the ethnographic portrait of “the local”; and, following the latter, (3) the distinction global/local is schematic if not entirely misleading (see also Englund and Leach 2000). In short, the task of ethnography could *not* be considered as the description of local instances of global processes; however, ethnographic practice is always *located*. Ethnography is a method that depends on interaction, communication and participation in order to produce insightful contributions to social theory. Thus, how do these ethnographic proposals regarding “global conditions” translate into fieldwork? If processes are global, they can be found everywhere and therefore nowhere. Thus the question: “Where would one locate the global in order to study it?” (Tsing 2005:3). To address this inquiry, I turn now to the debates localizing the possibilities of ethnographic research in a global situation.

ETHNOGRAPHIES OF THE GLOBAL ALONG FOUR DIMENSIONS

How can we recognize “the global” in order to study “it”? Which approaches have been proposed for the ethnographic study of “globalization?” Which are their assumptions and shortcomings? How can we deal with the “lack of fit between the problems raised by a mobile, changing, globalizing world, on the one hand, and the resources provided by a method originally developed for studying supposedly small-scale societies, on the other” (Gupta and Ferguson 1997:3)? Two main strategies have been proposed to answer this question: on one hand, George Marcus’s multi-site ethnography (MSE), a perspective emerging from the deconstruction of “realist ethnographic authority” (Stacey 1999:689). On the other hand, global ethnography (GE), actualizing the legacy of critical theory and invigorating it with “ethnographic flesh.” Although by now an extensive literature focuses on “globalization” from a sociological or anthropological perspective, I will focus here in MSE and GE since they are the most explicit answers given to how to use ethnography to analyze global processes. In the following pages, I develop a critical dialogue between MSE and GE comparing these perspectives along four sets of questions. First, what are the conceptions of “site(s)” in multi-sited and global ethnographies? What kind of field-site and fieldwork is envisioned in each of them? How do MSE and GE challenge traditional definitions of intense fieldwork in a delimited geographical area? Second, how do we determine the connections between the delimited spaces or communities entailed by the ethnographic research and the “external world”? Which kind of global phenomena can be captured through ethnography? Third, what kind of guidelines and orientations should we use for an ethnographic study of global processes? How do MSE and GE propose that ethnographers should collect data? Fourth, how do MSE and GE approach the ethnographer’s “intervention?” How can we incorporate the presence of the ethnographer as a dimension of the analysis? The aim of this discussion is to show that MSE and GE are sometimes used as interchangeable concepts but ultimately present distinctive answers to key questions for the ethnographic study of global processes.

Site(s)

The contestation of the geographically bounded fieldsite as the sole object of the ethnographer's practice is the founding principle of multi-sited ethnography. According to George Marcus, MSE "moves out from the single sites and local situations of conventional ethnographic research designs to examine the circulation of cultural meanings, objects, and identities in diffuse time-space" (1995:96). By doing so, MSE develops

de facto comparative dimensions...as a function of the fractured, discontinuous plane of movement and discovery among sites as one maps an object of study and need to posit logics of relationship, translation, and association among these sites. (Marcus 1995:102)

The guidelines provided by Marcus to practice MSE are defined as "modes of construction" by which the ethnographer follows people (e.g. migrants), things (e.g. the circulation of commodities, money, gifts, etc.), metaphors (discourses and modes of thought), plots, stories and allegories (typically, studying social memory), as well as biographies or conflicts (Marcus 1995:105-110).⁶ The ethnographer reconstructs the site through the links she/he creates doing fieldwork and connecting the sites through the text and the arguments.

Multi-site research is designed around chains, paths, threads, conjunctions, or juxtapositions of locations in which the ethnographer establishes some form of literal, physical presence, with an explicit, posited logic of association or connection among sites that in fact defines the argument of the ethnography. (Marcus 1995:105)

This often quoted paragraph certainly captures one of the basic points of MSE: the field site is understood less in terms of a space and more as connections, a series of "shifting locations" (Clifford 1997). The site is thus not necessarily a concrete space but rather a series of sites connected by a common thread; sites are put together by the ethnographer's argument. In such way, travel and mobility replace the classic ethnographic terms of dwelling and long-term fieldwork (Englund 2002.) As an example of multi-site research, Marcus quotes a project on violence in contemporary Colombia in which ethnography was practiced at two sites: among experts ("violentologists") and in the Constitutional Court (Marcus 1998:21-25). Although the researcher attempts to justify the election of these two sites,⁷ some have argued that in MSE "the problem...is not finding a diversity of leads to follow, but rather finding a way to contain this multiplicity" (Candea 2007:175). From a GE view, the problem with MSE lies in that "while

⁶ Also some single site ethnographies are reanalyzed by Marcus as "strategically situated," on the grounds that they engage in an implicit comparison (the example is Paul Willis's *Learning to Labor* [1977] that looks at boys in school assuming they will become workers).

⁷ "I did not choose [those sites] for their structural similarities, or even for how they define networks of elites, but for the ways in which in its particular social manifestation and embeddedness defines possibilities for opening new public debates and terms of action in the consideration of the myriad forms and the past of violence in Colombia" (Marcus 1998:25).

finding connections is certainly not difficult, deciding which of them are worth pursuing seems somewhat arbitrary...[and] situations where clearly defined relations exists between two sites or across two scales are relatively rare” (Gille and Ó Riain 2002:286-287). For instance, the MSE strategy of “following the people” may be taken literally to study migrations, and an ethnographer can investigate migrants in their points of origin and destination. But if you take the example of “follow the metaphor”, how can you determine which traces of a discourse to follow? If a discourse is produced in multiple points of a network, how can we know which node of the chain is more relevant?

From a GE perspective we cannot take sites literally, as Marcus seems to imply. We rather need to scrutinize how sites are *produced*, which politics of scale mold them and what hierarchies are (re)created in the process. GE thus proposes a research agenda to “replace abstract globalization with a grounded globalization that tries to understand not only the experience of globalization but also how that experience is produced in specific localities and how that productive process is a contested and thus a political accomplishment” (Burawoy 2001:158). In a way, GE takes heed of MSE debunking of the bounded site; yet it takes this insight into another direction. Proponents of a GE propose a perspective “that still locates itself firmly in places but which conceives those places as themselves globalized with multiple external connections, porous and contested boundaries, and social relations that are constructed across multiple spatial scales” (Gille and Ó Riain 2002:291). Therefore, the literal multiplicity of sites could be *one among other strategies* to investigate the causes and consequences of global process. Global ethnographers can also do research in *one site* by determining how it came to be imagined and created as a “local” place or how a place changed historically: “The place-bound site becomes a platform from which a variety of place-making projects can be investigated” (Gille and Ó Riain 2002:291).

Global ethnographies developing a “multi-site” strategy (by focusing on the movement of people and ideas) in part bear similarities with Marcus’s ideas. Ethnographers studying software developers connecting Ireland and Silicon Valley (Ó Riain 2000), nurses connecting Kerala with USA (George 2000); and activists connecting USA and Brazil through feminist discourses (Thayer 2000) deploy methodological strategies similar to Marcus’s “follow the people” or “follow the metaphor.” However, how global ethnographers explain the movement of ideas and people differs from MSE (more on this in the following section). The main contrast between MSE and GE in their approaches to fieldsites is the use of ethnohistory in the latter to expand the “bounded site.” For instance, in her study of the moribund welfare system in Hungary, Haney (2000) moved between state agencies observing and interviewing public officials, experts, and welfare recipients reshaping the practices and “discourses of need.” But Haney broke not only with spatial but also with temporal borders; she perused the archives of a welfare agency to compare the discourses of welfare recipients before and after the fall of the socialist state. Therefore, through the use of ethnohistory she multiplied both the “geographic” and the temporal sites of research. Similarly, the work of Gille (2000) on the conflicts over the installation of a dump site in Hungary moves “backwards” tracing the past history of the locality and moves “upwards” investigating the multiple scales involved in the disputes between different actors (local towns, the global incinerator industry, the Green party, etc).

In short, GE (as MSE) concentrates on connections between sites, yet it does so with an eye to how these connections (re)produce and destabilize hierarchies in the midst of historical change, scrutinizing the politics of scale implicit in the production of spaces. To explain change,

movement and scales, GE necessarily connects their sites to broader forces, as I discuss in the following section.

Context

How can we make an argument about “global processes” using ethnography that, although multi-sited, researches a limited number of subjects, places or connections? In his programmatic review article, Marcus uses the concept of world-system as shorthand to refer to this “wider world”; yet the world-system is for him “not the theoretically constituted holistic frame that gives context to the contemporary study of peoples or local subjects closely observed by ethnographers, but it becomes, in a piecemeal way, integral to and embedded in discontinuous, multi-sited objects of study” (Marcus 1995: 97). A MSE perspective may accept the possibility of a “sort of total world system as long as the terms of any particular macro-construct of that system are not allowed to stand for the context of ethnographic work” (Marcus 1995:99). In other words, the context cannot be reconstructed by resorting to what he calls “grand narratives” or “disembedded macro-perspectives” since ethnographers “are trying to come to terms in shaping their objects of study in the absence of reliable holistic models of macroprocesses for contextualizing referents of research, such as ‘the world system,’ ‘capitalism,’ ‘the state,’ ‘the nation,’ etc.” (Marcus 1995: 103). When ethnographers use these “fictions of the whole” to make sense of the world in which they study, “the ethnographer cannot help but to import into the closely watched life of his knowable community of subjects unexamined assumptions and premises about the way the larger world really is” (Marcus 1998:33). In other words, we should not represent the world of others imposing our theoretical categories.

From a MSE perspective, the context is a problem in itself not adequately solved resorting to other “narratives;” “the broader context is in a sense entirely of the ethnographer’s and his informants’ own making, rather than attributable to more abstract and already morally loaded forces such as capitalism and colonialism” (Marcus 1998:121). In an anthropological twist, the perils of ethnocentrism are now transfigured and conjured as “theory-centrism.” Marcus is here wielding an argument against political economy and historical ethnography, claiming that “its arguments and significance are not produced or given within the frame of ethnographic work itself but by the contextualizing discourses and narratives in which the ethnography comes to be embedded” (1998:13). In contrast, Comaroff and Comaroff have argued that while one too many ethnographers have been worried about “theory” usurping “others” representation of themselves; “the masters of the market, and powerful political pragmatists, fashion new modes of extraction, abstraction, and explanation” (2003:155). From this perspective, rooted in historical ethnography, the elusiveness of the current world does not preclude an attempt to explain it. For instance, the Comaroffs set out the “occult economy” in South Africa as a problem that may,

account for the workings of a metamorphosing capitalism that is both global in its reach and localized in its protean manifestations. Built into that problem is an effort to engage at once with the general and the particular, with variance and similarity, with continuity and rupture. (Comaroff and Comaroff 2003:158)

For Marcus, in contrast, this strategy is a form of holism that “might succumb to relying on ‘canned’ visions of what the world historical system is like” (Marcus 1998:39).⁸ Burawoy shares the critique to the “bounded site” made by MSE, yet on the grounds that the closure of the site is “militantly opposed to history and consideration of the extralocal context” (Burawoy 2000a:8). The logic that justifies the connections between sites or the historical changes of a site is explained by GE as a result of larger determinations or “global forces.” For GE, ethnographies of global processes have to investigate how the forces of global economy –mediated through the still present national state- creates heterogeneous social formations depending on the accommodations, negotiations and resistances of the grounded “local” actors. From the GE perspective, the absence of any role for the nation-state in constituting place and relations between places is MSE’s blind-spot:

If the early anthropologists reflected a period before the rise of the modern nation state, their contemporaries today have sprung straight from village to the world as though the nation had already deceased. (Burawoy 2000a:33; cf. Marcus 1994:427)

In a way, the project of GE is about determining the “broader context” of the global from different perspectives and attending to its variations, resistances, opportunities and accommodations, focusing on the multiform ways in which global capitalisms and nation states interact with “local” actors. In this way, GE avoids viewing “global forces” as inevitable and natural, and instead conceptualizes global forces “as themselves the product of contingent social processes” (Burawoy 2000a:29). Accordingly, global forces can create global connections rendering counterintuitive results. For instance, in her research of migrant nurses traveling from Kerala, India to Central City, USA, Sheba George (2000) explains how economic dislocations create the conditions for migration and at the same time how this process transforms gender hierarchies within Indian families living in USA.

One of the key strategies of GE in elucidating the “broader context” of an ethnographic research is the use of ethno-history, a strategy at odds with the MSE perspective. Marcus takes issue with historical-ethnographic explanations because they “...are not produced or given within the frame of ethnographic work itself but by the contextualizing discourses in which the ethnography comes to be embedded” (Marcus 1998:13). He argues that:

Anthropologists are more actively selecting framing contexts, theoretical associations, and narratives for their ethnography, but they still mostly are not creating them within the heart of the ethnographic process of fieldwork and writing itself. (Marcus 1998:13)

⁸ In a similar vein, the project of MSE also rejects a frame inspired in Jurgen Habermas’s division of “life-world” and “system.” When characterizing MSE, Marcus defines it as a “mobile ethnography” that “destabilizes the distinction, for example, between life-world and system, by which much ethnography has been conceived” (1995:96). Not surprisingly, this framework is explicitly deployed in the articles collected in Burawoy’s *Ethnography Unbound* (1991).

In contrast, from the point of view of GE the main problem of MSE is precisely that it neglects any assessment of the effects of historical legacies. Thus MSE

ignores the dynamic process by which sites are transformed by their external connections...It takes places for granted and leaves no room for accounting for the production and transformation of sites. In short, in multisited ethnography, history remains an afterthought rather than a factor that has implications for what can be seen as a site. (Gille and Ó Riain 2002:287-8)

To remedy the problem of “postmodern fracturing and fragmentation” of studies that become “a pastiche of vignettes” with ethnographers acting as “tourists tripping from resort to resort” Michael Burawoy proposes to “...ground our ethnographies in local histories” and turn ethnographies in ethnohistories (2000a:5). A GE perspective cannot but take history into account, since when reading local contexts ethnographically signs and practices

require to be inserted into the translocal processes of which they are part *ab initio*: processes –commodification, colonization, proletarianization, and the like– composed of a plethora of acts, facts, and utterances whose very description demands that we frame them in the terms of one or other Theory of History. (Comaroff and Comaroff 2003:161)

By tracing historical change, GE follows scholars arguing that the key task is not only to make conceptual connections between places and theory but also to make historical connections between places, which in turn help us to advance in both our understanding of particular people and the refinement of theory (Des Chene 1997). One of the ways in which GE introduces history into ethnographic research is by revisiting sites previously studied or using existing ethnographies as a historical baseline that allows one to observe changes produced in the site/s. This strategy has been formalized as the “extended case method,” originally developed by the Manchester School (Van Velsen 1967; Gluckman 1967) and developed by Burawoy (1998). GE explicitly relies on this strategy, which lead us to consider first how GE and then how MSE conceive the research design.

Research Design

GE seems to join recent trends in ethnographic research emphasizing its explanatory and analytical capacities (e.g. Lofland 1995; Katz 2001, 2002; Snow, Morrill and Anderson 2003). The research design proposed by GE is a dimension of the extended case method. For practitioners of GE, the objective of research is to strengthen and extend theory by accommodating “observed lacunae or anomalies:” “We try to constitute the field as a challenge to some theory we want to improve. What makes the field ‘interesting’ is its violation of some expectation and an expectation is nothing other than some theory waiting to be explicated” (Burawoy 2000a:28). For GE adherents, without the guidance of theory it becomes impossible to

know where to look, what to ask or what fieldnotes to take.⁹ Conversely, the Comaroffs advocate for anthropology “empirically grounded without being empiricist” (2003:164) to address the research of global configurations from an ethnographic perspective.¹⁰ Albeit taking note of the recent “anthropological anxieties” brought by the postmodern critique, these ethnographers maintain the need of relying in theory:

the human world, post-anything and everything, remains the product of discernible social and cultural processes: processes partially indeterminate yet, in some measure, systematically determined; ambiguous and polyvalent, yet never utterly incoherent and meaningless; open to multiple constructions and contest, yet never entirely free of order – or the reality of power and constraint. (Comaroff and Comaroff 1992:xi)¹¹

In a nutshell, portraying fieldwork as a mere “look and see” done without the guidance of theory is unacceptable from a GE perspective. Ethnographers inevitably always perceive, register and interact in the field with the aid of a theory, consciously or unconsciously. Theory and data inform each other, yet theory makes data possible and gives it its significance; the final aim of fieldwork from a GE view is to construct sound explanations of social phenomena developing social theory. The potential pitfall of the emphasis on theory is that it may render monolithic descriptions of the people studied; missing variations of behavior that may defy the theoretical frame (Katz 2004). In fact, one of the critiques of the founding statement of GE₂ is that some of the case studies “lack a substantial ethnographic component, relying instead on either historical data or scholarly interpretation. This leaves the reader with little sense of how the people most affected by the forces of globalization conceptualize, react to, or combat them.” (Hargrove 2001:39; also see Burawoy et al. 2000).

MSE, in turn, has a quite different stance regarding the role of theory in the practice of fieldwork. In Marcus’s perspective, “theory is held in abeyance” since it is in the experience of moving between field-sites that the ethnographer avoids the perils of “overtheorization.” In MSE, the theory emerges when positing a logic of relationship between the sites where the ethnography is conducted (Marcus 1998:19).¹² Theory may constrict the ethnographer’s fieldwork by way of introducing preconceptions:

⁹ Taking to its paroxysm, the idea of doing fieldwork “without theory” equates the ethnographer with a character of a Jorge Luis Borges’ short story, Funes, whose ability to remember everything made him incapable of abstraction (Borges 1967). I owe this telling metaphor to Javier Auyero.

¹⁰ See also the article by Comaroff and Comaroff (2003:168-170) for guidelines on the “methodological operations” to address these problems.

¹¹ In the same vein, in a “Manifesto for Ethnography” (the opening editorial of the journal *Ethnography*) the authors underlie the same preoccupation: “We are not interested in ‘grand theory,’ ‘pure’ scholastic reason, or ‘abstracted’ empiricism. We seek to promote ‘theoretical informed-ness,’ ‘sensitizing concepts,’ ‘analytic points,’ all means of teasing out patterns from the texture of everyday life, from ‘pure’ descriptive ethnography” (Willis and Trondman 2000).

¹² “Anthropologists are more actively selecting framing contexts, theoretical associations, and narrative for their ethnography, but they still mostly are not creating them within the heart of the ethnographic process of fieldwork and writing itself” (Marcus 1998:13).

For example, once we know (or analytically ‘fix’ by naming) that we are writing about violence, migration, the body, memory, and so forth, we have already circumscribed the space and dimensions of our objects of study...The mark of experimental, critical work is its resistance to this too-easy assimilation of the phenomenon of interest by any given analytic, ready-made concepts. (Marcus 1998:187-188)

In MSE, concepts commonly used to set the context of a research could only emerge from the fieldwork and its inscription in a text. Put another way, the fieldwork must inform theory and not the other way around. The ideal of research may sound familiar to sociologists utilizing qualitative methods, since MSE methodology resembles the inductive techniques of “grounded theory” (Glaser and Strauss 1967). The problem of this strategy is that may provide little guidance when the researcher has to make decisions in the field. For instance, Matei Candea, inspired by this spirit of MSE,

followed people, stories, metaphors, and debates through multiple spaces both within ‘the village’ and without [but] in practice [this] led to a constant indeterminacy: how many leads to follow? How much context to seek? How much information is enough information? (Candea 2007:173)

This is exactly the conundrum that MSE leaves for its practitioners, who risk a less than systematic investigation of “the field” for their reluctance to self-consciously adopt a set of theoretical questions at the outset.

Reflexivity

How ethnographies of global processes conceive the effects of the researcher’s intervention in the places studied? How does physical and social immersion in a delimited setting required by the practice of ethnography affect representations of the world/s portrayed in global or multi-sited ethnographies? MSE and GE offer contrasting responses to these questions. The former requires the ethnographer to interrogate relationships of power, in particular the ways in which informants are represented in texts. The latter addresses the issue mainly by reflecting on the structural constraints that exert an influence in the ethnographer’s categories of perception and analysis.

The different sites the researcher observes and participates in MSE demand a continuous change in the spaces of interaction. These changing landscapes traversed by the multi-sited ethnographer demand a perpetual renegotiation of her/his identity vis-à-vis the subjects of the study. The multisited ethnographer moves “between public and private spheres of activity, from official to subaltern contexts,” and in this movement “it is bound to encounter discourses that overlap with his or her own” (Marcus 1995:112). The multi-site ethnography “is thus always conducted with a keen awareness of being within the landscape, and as the landscape changes across sites, the identity of the ethnographer requires renegotiation” (Marcus 1995:112). These changing conditions enable the ethnographer to gain insights about the ways she/he is perceived by others and the management of public presentation, both at the moment of fieldwork and the moment of writing. Reflexivity in MSE terms is then a twofold problem: a matter of analysis

about the unstable identity of the ethnographer in a variety of field sites, and a dimension in the practice of writing (see also Clifford and Marcus 1986; Peirano 1998). Turning the postmodern critique against Marcus, some have argued that the idea of ethnographer facing a “rupture” when moving between settings is problematic, since this will suppose a unitary identity that precedes the entrance to the field (Roberts and Sanders 2005). This indictment is especially pertinent for “halfie” ethnographers (Abu-Lughod 1991) that neither fit in the typical mold of the “white anthropologist” nor necessarily in the role of “native.” Yet beyond these critiques, what I would like to stress here is that reflexivity in terms of MSE turns its gaze towards silences in classical anthropology (namely, the omniscient narrator that bracketed colonialism) by scrutinizing the self of the ethnographer and her/his techniques of representation.¹³ Reflexivity according to MSE thus implies a focus on the ethnographer’s identity and her/his relationships with others, which are inspected in the practice of fieldwork and in the texts produced.

Whereas MSE focuses its attention on the effects of power relations in the text (i.e. the “politics of representation”) and the changes in the ethnographer’s persona (her/his different “strategies of presentation” according to the context), GE considers how the ethnographer’s intervention could be a tool to scrutinize its categories of perception and its effects in the world she/he studies. GE researchers react against trends that view ethnography as akin to comparative literature, “an interpretative exercise, a political stance, which justifies the thinnest of accounts, the most fleeting of engagements, and the most unsystematic of observations” (Burawoy 2000b: 341). In other words, GE is less preoccupied in textual experimentation than in avoiding a picture of “the global” that is a function of the socially privileged position of the researcher. Allow me to quote *in extenso* a passage of Burawoy’s conclusion of the volume *Global Ethnography* in order to illustrate this point:

It is an accident that high-flying academics, hotel circuiting consultants, conference-hopping professionals, and netscaping virtuosos should develop concepts of the network society, should imagine a manichean world of placeless power and powerless places, should expound on time-space compression or aesthetic cognitive maps? ... Their theories of globalization are theories of privileged men, who appear in a privileged air-space above the world they theorize. Their absence from their own accounts aspires to objectivity, but it cannot hide the unspoken, unreflected, stratospheric situatedness of their knowledges. How much of their theorizing is the projection of insulated journeys, unspoken genealogies, self-referential worlds? (Burawoy et al. 2000:340)

Put another way, if one of the basic tenets of ethnography is to avoid ethnocentrism, we should also be wary of falling into “class-centrism.” Burawoy’s critique aims more towards

¹³ In certain passages, Marcus (1998) also seems to take note of the institutional patterns that shape research. See, for instance, his discussion of the different inquiries posed by ethnographers in their “first” (i.e. PhD dissertations) and “second” projects (i.e. once the ethnographer became established).

“cosmopolitan sociologists” than to anthropologists, yet the point should not be disregarded, and perhaps these counterpoints between GE and MSE reflect disciplinary differences.¹⁴

A crucial way to exert a “reflexive vigilance” could be to scrutinize the conditions of production and circulation of the ethnographic studies of global processes. For instance, Englund and Leach remark that

the constant monitoring of scholarly output...presuppose continuous presence in the academia...Unsurprisingly, perspectives which require a minimum of fieldwork, perspectives which demand instant ethnography to illustrate aspects of a metropolitan meta-narrative, hold increasing appeal. (2000:238-9)

Others hold that “reflexivity within ethnography is not only affected by biographical resources...but also, perhaps more importantly, by research resources” (Roberts and Sanders 2005:309). This point is especially pertinent when considering how neoliberalism is reshaping universities (Hannerz 2007), or when discussing North-South relationships within the anthropological field (Lins Ribeiro 2006), and issues of power within the field of global sociology (Burawoy 2008).¹⁵ This broader issue is beyond the scope of this article, yet an important way to exert a reflexive stance is to de-naturalize not only the place of the ethnographer in the “exotic” field-site but also in the more familiar milieu of academic research (see also Bourdieu 1984; Bourdieu and Wacquant 1992).

In the preceding pages, I dissected the tenets of MSE and GE along four dimensions, an analysis which I attempt to summarize in Table 1.

Table 1: GE and MSE Along Four Dimensions

	Site/s	Context	Research Design	Reflexivity
Global Ethnography	Different scales	Capitalism Nation-State History	Extension of theory	Structural position
Multi-site Ethnography	Different locales	“of the ethnographer’s and his informants’ own making”	Theory “held in abeyance”	Self and textual representation

¹⁴ According to Paul Rabinow, “For many years, anthropologist informally discussed fieldwork experiences among themselves. Gossip about an anthropologist’s field experiences was an important component of that person’s reputation. But such matters were not, until recently, written about ‘seriously’. It remains in the corridors and faculty clubs” (Rabinow 1986:253).

¹⁵ Another point not sufficiently discussed is the influence of review boards and ethic committees that oversee ethnographic research, usually using a framework more suitable for the natural sciences. See for instance the special forum in *American Ethnologist* on “IRBs, Bureaucratic Regulation, and Academic Freedom” (2006, Vol. 33, No. 4) and also Cooper (2007); Bosk (2007); Lederman (2007); and Tolich and Fitzgerald (2006).

In retrospect MSE had the virtue of widening the scope of anthropology, bringing it closer to cultural studies and sociology (although the “postmodern” sympathies of MSE fit better with the former than the latter). MSE spelled out what many ethnographers were already doing, providing a rationale to study transnational or global phenomena using participant observation, and loosening the straitjacket of the bounded field-site in anthropology.

MSE, in short, contributed to a conversation to denaturalize the field, fieldwork and its translation into texts. Akhil Gupta and James Ferguson nicely captured the implications of an unstable fieldwork by asserting that “We might emerge from such a move with less of a sense of ‘the field’ (in the ‘among the so-and-so’ sense) and more of a sense of a mode of study that cares about, and pays attention to, the interlocking of multiple social-political sites and locations” (1997:37) In his *Ethnography Through Thick and Thin*, Marcus (1998) somehow softened the agenda of MSE, asserting that multi-site strategies should be thought as “a research imaginary...rather than a set of methods that are very specifically prescriptive for the conduct of fieldwork and writing” (1998:6). Several scholars took heed of the methods and/or the imaginary proposed by Marcus’s manifesto investigating global connections in the sub-areas of legal anthropology (e.g. Anders 2007; Merry 2000), transnational migrations and diasporas (e.g. Hall 2004; Fitzgerald 2006; see reviews in Routon 2006 and Santiago-Irizarry 2008) gender and work (e.g. Ganguly-Scrase 2003), multinational corporations and international management practices (e.g. Hassard, McCann and Morris 2007; Peltonen 2007), consumption relations (e.g. Bettany and Daly 2008), and commodity chains (Freidberg 2001). Still others have aimed to extend the methodological and theoretical insights of MSE (e.g. Bornstein 2007; Line 2007; Fitzgerald 2006).¹⁶ Nevertheless, some consider there is still certain lag between the programmatic gesture of MSE and the ethnographic research of global processes.¹⁷

If MSE brings sociology and anthropology closer, then GE reinvigorates ethnography within sociology. Unlike anthropology, where ethnography has been established as the canonic method, it has been marginalized in the discipline of sociology (see, for instance, the account of Burawoy 2000a:7-15). GE provides useful guidelines to study global processes ethnographically without accepting the postmodern premises of MSE. In other words, GE offers a toolkit for scholars interested in analyzing global process, allowing them to attend to local meanings without losing perspective of the mediating role of the national state or the impinging forces of global capitalism(s). Some researchers have followed these guidelines and produced work in the areas of migration (e.g. Paerregaard 2008), gender and work (Otis 2008; Poster 2002), the dynamics between global process and world’s sub-regions (e.g. Chong 2007; Gille 2004), and the role of financial organisms in fostering “globalization” (e.g. Goldman 2006). With the hype about “globalization” now withering away, GE offers a significant point of departure to problematize

¹⁶ See also Coleman and Von Hellerman (forthcoming) for a review of MSE.

¹⁷ To witness, see the following appraisal in an anthropological forum: “If you were to mention a field, topic, or approach that is sorely lacking in current anthropological dealings with the global world what would that be? Only in the sense that there was this rush of programmatic announcements followed by a smaller number studies of diasporas, media forums, and corporate networks, so the actual ethnography of the global is still largely lacking. That is the reason I am saying we still need to work at the methods, because if we are not counting programmatic statements we still have a very small corpus of ethnographic work. I think the question on how to do work on some of these new topics is still being worked out” (Hirsch et al. 2007:122).

disembodied and abstract accounts of global processes (e.g. Giddens 1990; Beck 2005) and provides a perspective that can be reconciled and put into dialogue with sociological debates.

CONCLUSION

In this article I hope to have shown the contributions that ethnography can make to understanding, explaining and researching the contemporary “global situation.” The first part of the article concentrated on discussions that may bring disciplinary reflexivity into the study of global processes. More specifically, recent global ethnographers caution us to avoid taking “globalization” for granted. Instead global ethnographers are urged to problematize ready-made concepts that describe as much as inscribe/prescribe the ways in which they imagine and investigate “the global” and “the local.” Ethnography can counterbalance certain tendencies in the social sciences that see the global situation in economistic, totalizing, monolithic or evolutionary terms. An ethnographic perspective, always attentive to situations and interactions in concrete settings, can show that the global is not a unitary phenomena but a diversity of projects. Ethnography can help to ground globalization by showing how global “flows” always have to make terrain, i.e. how forces with global reach always interact with actors at the national and “local” scale. This contribution not only enhances our imaginations of the global providing a view “from below.” In fact, one of the points ethnographers of the global have made is that ethnography do not need to be constricted to exemplify the “local impact” of global processes. Rather, ethnographic research has demonstrated that the pair global/local never has to be confused with a geographical scale (New York is not “more global” than a village of the so-called periphery) neither should be equated with the binary universal/particular. For global ethnographers, global and local are scales that are constructed as political projects (i.e. as a function of relations of power) and for MSE those are dimensions that must emerge from the point of view of the sites where the fieldwork is done and not assumed by fiat. Therefore, one of the contributions made by an ethnographic gaze is that it forces us to be more critical about concepts such as “globalization” or the binary global/local, demanding to perceive them as cultural constructs and not simple reflections of “what is going on in the world,” thus avoiding the perils of a naïve constructivism or a positivistic realism.

In the second part of the article, I explored the counterpoints that can be inferred from a close reading of the projects proposing a “multi-site” or a “global” ethnography, focusing on four dimensions. These differences between MSE and GE can be seen as the effect of broader and older disputes. Broader, because they show how MSE and GE are embedded in larger changes affecting the social sciences, the “literary/hermeneutic turn” in the case of MSE (e.g. Marcus and Cushman 1982) and the impact of the “historical turn” in the case of GE. Older, because these contrasts can be interpreted as a re-edition of tensions between “cultural” and “material” orientations within anthropology, which although never purely or overtly stated they still survive in the everyday interactions and rites of the discipline (Winslow 2007).¹⁸

¹⁸ The contrasts between MSE and GE also may be reflecting disciplinary frictions between sociologists and anthropologists. See for example the following telling dialogue:

As it can be inferred, the highlighted contrasts are methodological as much as theoretical and epistemological. Rather than categorize “multi-sited” or “global” ethnographies as mutually exclusive projects, the goal of the second section was modest. Basically I sought to clarify points of incommensurability between these two perspectives to caution scholars against using them interchangeably.¹⁹ Although I do not assert that MS and GE are necessarily incompatible, MSE and GE offer distinctive responses to similar theoretical and methodological questions. Although with parallel inquiries, the two approaches may lead researchers down divergent paths. For instance, a MSE perspective is related to post-modern epistemology and has a parallel stance vis-à-vis participant observation as “grounded theory.” In contrast, a GE perspective is related to a critical or pragmatic realism and analytic ethnography.

We know that both “realist” and “postmodern” ethnographies have shortcomings and insights. The point of this article, however, was not to reach a “middle ground,” taking what is more valuable in each perspective; neither was it to entertain abstract epistemological debates. Rather my aim was to enrich a pragmatist point of view about how we do ethnography. If, as some have argued, research is a process that entails continuous decision-making, then this review can be conceived as a provisional map that may help to inform those choices.²⁰ Both MSE and GE can certainly be sources of inspiration, still my guess is that when the ethnographer has to submit a grant proposal or make sense of the data collected, she/he will probably realize that certain types of data are more readily illuminated with some categories of questions, perspectives and authors over others. In conclusion, this article may be seen as an exercise and a map aimed to contribute towards post-globalist ethnographies.

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Anna Tsing: “When I talk to my students about the kinds of ways that they are copying students from other disciplines, it seems to be one of the distinctive requirements that we think about theory and our empirical material together. We do not separate them out as two layers.”

Emily Martin: “Sociologists do that, right?”

Ann Tsing: “Right.”

Emily Martin: “We do not want to make them the model, though.” (Hirsch et al. 2007:115-116)

¹⁹ Several papers present MSE and GE as synonyms. See, for instance, the articles of Ganguly-Scrase (2003:550), Brown (2003:1793), Peltonen (2007:349), Hassard et al. (2007) and Freidberg (2001).

²⁰ “As every researcher knows, there is more to doing research that is dreamt of in philosophies of science...No matter how carefully one plans in advance, research is designed in the course of its execution. The finished monograph is the result of hundreds of decisions, large and small, made while the research is under way” (Becker 1965:602).

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**PUZZLES IN THE COMPARATIVE STUDY OF FRONTIERS:
PROBLEMS, SOME SOLUTIONS, AND METHODOLOGICAL IMPLICATIONS¹**

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ABSTRACT

This paper examines ways in which world-system analysis can be employed fruitfully to explore frontier social processes. Conversely, it also examines how frontier social processes and events can be very valuable explorations of highly localized processes – geographically and temporally – of world-systems. The study of frontier regions can help to uncover the ways in which many world-systemic contexts shape local human agency. Conversely, the study of these highly localized human practices offers ways to gain insights to how individual actions constantly reconstruct world-systems. Finally, many of the lessons learned here with respect to frontiers, especially in regard to ethnic and national identity formation and transformation can be extended to other social concerns.

INTRODUCTION

At first, frontiers may seem like an odd topic to explore comparative methods for world-systems analysis. Some might argue that with globalization there are few if any frontiers left. Some might even go so far as to argue frontiers no longer have an impact on the contemporary world. Both arguments are wrong, but not transparently so.

The initial issue is what are we discussing? What is a frontier? This is a very frustrating question as there are almost as many definitions of frontiers as there are people who have studied them! I begin with a working definition. A frontier is a zone, where two different social systems – nonstate societies, state societies, and even world-systems come into more-or-less sustained contact (Hall 2005a). Of course, this definition is vague. That is intentional. It is a starting definition to which refinements might, and later will be added. A frontier is *not* a border our boundary between two states – though that is how the term is used in much of Europe. The term,

¹ I would like to thank the many panelists and discussants who commented on various papers I have drawn from in writing this paper. I would also like to thank Richard W. Slatta for carefully reading an early draft of this paper. The comments of an anonymous reviewer helped me to clarify the logic of my presentation. And finally I thank Jeffery Kentor both for suggesting this paper, and for his patience while I worked on it. As always, remaining errors and problems are mine. The following items all shaped this paper in one way or another: Hall 1989a, 1989b; 1998a, b; 2000a, 2000b, 2000c; 2001a, 2001b; 2002; 2004a, 2004b; 2005a, 2005b, 2005c; 2006a, 2006b.

“borderlands,” which refers to a zone on either side of a border or boundary, often is a frontier. Some might argue that transnationalism constitutes a kind of frontier. While that suggestion is not without some merit, the term itself means spanning state boundaries, not the zone of overlap between two states.

The study of frontiers, reviewed briefly later, has often been mired in two problems. The first problem was rooted in garden variety ethnocentrism that conflated the westward moving frontier of the United States with new territories and peoples as it expanded. Ironically, this approach paid little or no attention to frontiers of Spanish expansion. The second problem was a much more serious problem conceptually, theoretically, and historically. All frontiers at first appear to be similar – why the label frontier is applied. However, on much closer inspection each frontier had many specific and peculiar qualities to the point that each appears unique.

This, of course, is a classic methodological problem: how to abstract to underlying factors and processes from the messy and fuzzy qualities of social experience and history. Several decades ago Arthur Stinchcombe (1978) noted that a key problem in historical sociology was to figure out what constituted a case or instance of a social factor or process that a researcher wanted to study. Indeed, he offered a caution in the days of rampant quantification, that counting was the last part of the study, not the first.

World-systems analysis, especially comparative world-systems analysis, points to solutions to these problems. This might seem obvious since nearly every frontier (broad generalizations about frontiers are notoriously hard to sustain) is to some extent shaped by processes of interactions among societies – the quintessential subject of world-systems analysis. Indeed, what was missing was a larger framework within which to place both comparative studies and individual case histories. Since all world-systems expand and incorporate new territories and new peoples, they create, transform, and destroy frontiers. Thus, it is a valuable form of analysis to study frontiers. Furthermore, world-system analysis, when done well studies such interactions from the bottom up as well as from the top down. But the world-systems analysis must be used dialectically. Extant world-systems analyses can be a source of theoretical, empirical, and methodological techniques. Conversely, frontier studies can highlight nuances and particularities within general patterns. The use of world-systems analysis also affords opportunities to connect the local and the global in new ways and focus on human agency in world-systemic process.

So why is this suggestion relatively novel? On the one hand, the conflation of frontier in the abstract with westward expansion of the United States from its eastern coast is rooted in the popular imagination, which in the United States (and in many other places) derives from Frederick Jackson Turner’s “frontier thesis” (Slatta 1997, 2001, 2003, 2007; Taylor 1972). Jackson argued that the frontier experience in the United States shaped the character of U.S. culture.

Since the late 1980s there has been a near cottage industry among historians of the American West arguing that Turner was wrong, and indeed got it backwards (e.g. Limerick 1987; Limerick et al. 1991). Some of the “new western historians” went so far as to argue for abandoning the term ‘frontier’ altogether. In part, this is because so many accounts were marked by a triumphalism of the westward march of white males and nearly complete lack of attention to Indigenous peoples, women, and in many cases the environment. Within a few years, however, the F-word was rehabilitated by many of those same historians (Klein 1996; Weber 1986).

Gradually these historians came to see frontiers in a broader context, of which the U.S. frontier was not the archetype (Slatta 2003, 2007; Weber 1986). These new approaches revived a

weakened tradition of comparative frontiers (Gerhard 1959; Hofstadter and Lipset 1968; Lamar and Thompson 1981; Lattimore 1951, 1962c). While insightful many of the comparative studies wreaked of the garden variety “compare and contrast” approach, which lends itself to perpetuating the “this example is unique” approach the study of frontiers.

This is where and how world-systems analysis can be very useful. In what follows I will not follow a common format of: here is a list of problems followed by another list of how world-systems analysis solves those problems. Despite significant work on frontiers and incorporation of new peoples and/or territories into world-systems (e.g. Chase-Dunn and Hall 1997; Hall 2002, 2006a, 2006b), there are too few studies to make such as list. Rather, I seek to contribute to and promote a dialogic discussion of how the study of frontiers and the study of world-systems can usefully inform each other. My goal is not to provide a methodological manual, but to describe some enticing issues and problems upon which such a dialog might be based.

METHODS OF STUDYING FRONTIERS COMPARATIVELY: AN OVERVIEW

Some of the points I will raise here are rather pedestrian and familiar, others are far less so. I begin by revisiting what constitutes a frontier.

Frontiers are zones or regions that have moved and changed through space with time. Richard W. Slatta (1997, 1998) put forth one of the best metaphors for frontiers, that they are membranes. From basic biology we know that membranes have thickness. They only look like lines when viewed from a distance. They are permeable, but that permeability varies both with respect to direction and with respect to what kinds of things may pass through them, and at what rates they pass through them. Membranes have flexibility, they move and stretch in response to various pressures. A similar point was made by Edward Luttwak (1976) in *The Grand Strategy of the Roman Empire*, where he focuses on walls, such as Hadrian’s Wall which supposedly demarcated the northern end of Roman occupation of England. He noted that Hadrian’s Wall was far from a complete barrier. In practice it served as a regulator and concentrator of people and goods that passed through it. Other writers (e.g. Lattimore 1951, 1962a, 1962b, 1962c; Barfield 1989) have made similar points about the Great Wall(s) of China (the so-called Great Wall is actually a series of Walls constructed over centuries). The key point here is that walls, such as the Great Wall of China and Hadrian’s Wall were really filters, not barriers. As often as not they served as much to keep “civilization” in as to keep “barbarians” out! And, they notoriously failed at both!

There is a large collection of literature that studies frontiers comparatively. This literature is rooted in many disciplines.² One theme that appears repeatedly in comparative studies of

² The literature on comparative frontiers is actually quite large. I list a few major items here. An excellent resource is Richard W. Slatta’s *Comparing Frontiers: A Working Bibliography*, on line at: <http://social.chass.ncsu.edu/slatta/>. Many others are also cited in Hall 2001b. Classic sources include: Gerhard 1959; Hofstadter and Lipset 1968; Lamar and Thompson 1981; Lattimore 1940, 1962a, 1962b, 1962c; McNeill 1964; Taylor 1972. Many archaeologists have studied frontiers, some of them comparatively: Godsen 2004; Parker and Rodseth 2005; Smith and Rubinson 2003; Weber and Rausch 1994. There are a number of interesting accounts: Bartlett and McKay 1989;

frontiers is how frontiers that initially seem similar, turn out on closer inspection to be radically different. This “puzzle,” why all frontiers seem similar at first glance but then seem unique, demands an account of what social processes could be driving this. This was a common conclusion among a number of scholars, some of whom had reached that point independently.

One “explanation” for this situation is to posit a small number of factors or variables, each divided into a small number of categories. This will generate a large number of types of frontiers. This illustration draws from various studies of frontiers and from world-systems analysis:

- the type of frontier (4 types: buffer, barrier, internal, or external);
- the type of ecological differences (4 types: steppe, sown, hill, or valley);
- the types of nonstate groups (3 types, such as those conventionally labeled bands, tribes, or chiefdoms);
- the types of groups that come into interaction (4 types: non-state, tributary states, tributary empires, capitalist states [Chase-Dunn and Hall 1997]); and
- the type of boundaries involved (4 types: local economic, political or military, long-distance economic, and cultural [Chase-Dunn and Hall 1997]).

These few factors, divided into a few basic categories will generate 768 different types of frontiers (Hall 2001a, 2005a). The list could contain fewer or more factors. They could be divided differently. This would yield a different total of types. For purposes of this argument, it is not necessary to have an exhaustive list of factors, nor an elaborate rationale for the subcategories. Rather, the point is just a few ‘variables’ with just a few ‘categories’ can generate a large number of types of frontiers. Indeed, the number of categories itself is not the issue, only that it is sufficiently large that techniques (such as Ragin’s [1987] work on Boolean logic) would require an inordinate number of cases to sort out all the variables. Even if this could be done, at this stage it would be premature. Without a sense of what all the factors or variables are it is not possible to construct a list because we have almost no idea of the size of the ‘universe’ of frontiers. Here we may draw some lessons from basic sampling procedures.

Frontiers and Sampling

Basic methods and sampling theory show that biased samples, that is samples that over- or under-represent some aspect of the distribution of the phenomenon one is studying, lead to erroneous conclusions. Recall three old chestnuts of failures of sampling. First is the prediction that Dewey won the election when Truman did, because pollsters did *not* take into account that Republicans more commonly owned phones than did Democrats. Second, if a random sample is drawn on the basis of the first letter of surnames, the Latino/a population will be over- or under-sampled because a high proportion of Latino/a names begin with the letter “S.” Third, there is old story of the spurious correlation of storks and babies. The explanation is of course that in rural areas in Europe storks nest in rural areas (see Figure 1), and rural areas typically have higher birth rates than urban areas.

Figure 1: Stork Nesting on Chimney in Rural Central Poland; June 4, 2004

Chappell 1993; Power and Standen 1999. A few accounts draw heavily on world-systems analysis: Batten 2003; Dunaway 1996.



(picture by Thomas D. Hall)

These sorts of problems, combined with a poorly delineated universe, suggest that frontiers need to be studied globally and across time. It is well known that states have changed over time, whether one follows an expanded world-systems view (Chase-Dunn and Hall 1997), or a Frankian world system view (Frank and Gills 1993), or more conventional explanations (Sanderson 1999; Sanderson and Alderson 2005). Thus, to study frontiers sampled from only one type, or more likely, a small range of types of states, could easily miss important variations and variables. More is at stake here than methodological issues. Virtually all studies of frontiers recognize that frontiers are created, shaped, reshaped, and occasionally closed in the interactions of local, regional, state-wide, and global processes (Hall 2000b). To omit one or more types of states could easily hide or mask important sources of variation. Thus macro, micro, and comparative analyses are essential. Therefore the sample should be drawn from the entire world, and *at least* since the first state appeared, in Ur, some 5,000 years ago. This, of course, omits non-state – state frontiers (e.g. Hall 1989a) and non-state vs. non-state frontiers (e.g. Chase-Dunn and Mann 1998; Burch 2005). Omitting these latter types could prove to be misleading. Thus, sampling over only one of the three types of societies: state vs. state; state vs. non-state; or non-state vs. non-state should be done cautiously. Any conclusions should be taken as provisional in light of such limitations.

Even within such limitations, I argue that any ‘sampling’ of frontiers should pursue a variance-maximizing strategy. Simply put, comparing as widely different types of frontiers as can readily be identified. Again, with eye toward noting any types of frontiers inadvertently omitted. The same argument can be applied to studying peripheral and semiperipheral areas within any world-system. Studies of the modern world-system have been plagued the paucity of reliable data over long periods of time (e.g., centuries).

A common critique of variance maximizing strategies – most common when they are not fully explained – is that one is comparing apples and oranges. But to be a bit waggish about it, apples and oranges are fruit, edible, spherical in shape to name a few similarities. The key point here is legitimacy of a comparison is *not* found in the objects or institutions being compared, but the theoretical point of the comparison. But that, in turn, implies that the point(s) of the

comparison are clear. The other source of this objection is an implicit or sometimes explicit rejection of the theoretical agenda for the comparison.

So far I have mentioned “types” of frontiers several times. Earlier I argued that “it is not necessary to have an exhaustive list of factors, nor and elaborate rationale for the subcategories.” Here I want to underscore a commonly misunderstood aspect of defining categories, types, subcategories, and often in identifying variables. Basically typologies are heuristic tools for sorting large numbers of instances into groups that, in general, are more similar to each other and that differ in some important way from those in other categories according to one or more identifiable criteria. So again, the categories are theoretically driven, intellectually constructed, and have no agency of their own. A different theoretical agenda suggests rethinking, and often reorganizing the typology. In the opening example if different variables are identified, or if different numbers of subcategories are differentiated, the total number of types of frontiers will change.

Sometimes, as initially discovered by Snyder and Kick (1979), categories such as core, periphery, and semiperiphery have an underlying empirical reality. I argue that there is an analogous situation with respect to frontiers. At this point in time, however, the universe of frontiers is so poorly known that this can not be demonstrated empirically. Still, a reasonable working hypothesis is that there may be an underlying empirical reality that can be useful in guiding and structuring comparisons. As the relevant criteria and measures or indicators of them are developed it would be at least in principle possible to construct elaborate Boolean comparative strategies (Ragin 1987), something along the lines of what John Foran (2005) did in his comparison of Third World revolutions. Another major factor, alluded to in the brief discussion of types of states, is the need to keep track of contexts and scales.

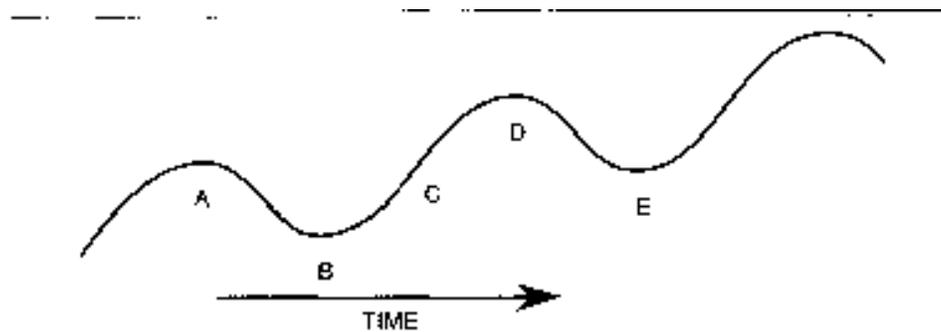
Contexts

By contexts I am thinking specifically of world-system contexts. These contexts might include the following (e.g., Chase-Dunn and Babones 2006; Chase-Dunn and Hall 1997; Hall 2000c, 2002):

- condition *of* the world-system: expanding or contracting
- position *in* the world-system: core vs. semiperiphery vs. periphery
- dynamic: rising or falling or stable
- cyclical position:
 - a- or b- phase in Kondratieff cycles [about 50 years]
 - rising or falling in hegemonic cycles [about 100 years]
 - rising or falling “long waves” [about 200 years]
 - rising or falling Ibn Kaldhun cycles [about 300 years](Turchin and Hall 2003; Hall & Turchin 2007);
 - dark age cycles [about 600 years] (Chew 2001, 2007)

The evidence for some of these cycles is quite solid, for others it is quite weak. Few if any of them can reasonably be extended far back in time, though Frank (1993) has argued for K-waves from the Bronze Age forward. The point with regards to comparative strategies is, that if one is unaware of cyclical processes, one can draw quite seriously flawed conclusions (see Figure 2).

Figure 2: Secular Trend with Cycles: Possible Comparisons



(From Hall 1989:233, based on Skinner 1985:289)

Figure 2 presents a variety of comparison points in a cyclical process which is itself part of a secular trend. Only the comparisons B – E or A – D will reveal the underlying secular trend. Comparisons A – B or D – E would suggest a falling secular trend. If the secular trend were removed, these would be B-phases of a K-wave (Goldstein 1988; Grimes 2000). Comparisons B – C, C – D, or B – C – D would overstate the secular trend. If the secular trend were removed these would be an A-phase of a K-wave. Finally comparisons A – C, C – E, or A – C – E would indicate a static condition in which no change occurred.

Furthermore, since we do know a great deal about the dynamics of several of the world-system cyclical processes and their complex interactions, failure to attend to these differences could lead to erroneous conclusions. Of course the fuzzier or messier the data *are*, the more difficult it is to take account of such comparisons. Nevertheless it would seem to be a reasonable working hypothesis that state – state, and even state – non-state frontier interactions would follow somewhat different patterns during an A-phase than in a B-phase, or especially between phases of expansion phases versus phases of contraction. For instance, Thomas Barfield (1989) has demonstrated that, contrary to popular belief, confederations of steppe pastoralists only grew and became strong when China was united and strong. The evidence and argument is complex, but a key point is that raiding and/or trading pastoralists cannot extract wealth from a failing empire.

Cyclical processes are not the only contexts that are relevant. If we conceive of several different types of world-systems, marked by different modes of accumulation, say kin-based, tributary, and capitalist (Chase-Dunn and Hall 1997), we might again expect different sets of frontier dynamics. Failure to consider these possibilities has often led students of the modern world-system to attribute various processes to capitalism, that in fact are actually part of all state systems, such as varieties of ethnic interactions (discussed further below [also see Hall 1998a, 2000b; Chase-Dunn and Hall 1997]). Then there are scale considerations.

Scales

Scale effects can be both spatial and temporal. The latter are somewhat easier to discuss. If a comparison is made over relatively short periods of time, some sorts of cyclical effects and mode of accumulation effects might be considered static or nearly so. But when comparisons bridge larger time segments these changes cannot be ignored. This, of course, is an issue first raised by

Braudel (1979, 1980) and discussed by John Hall (1980). The consideration of the various world-system cycles further suggests there are relevant time scales corresponding to the length of each of the cycles.

There can be analogous spatial scale effects. John Bodley (2003) discusses these in some considerable detail. For instance, once living groups expand beyond a certain population size, on an order of magnitude of 1,000, it was no longer possible to base all interactions on personal interactions. Bodley argues that this is a factor in the emergence of hierarchy and inequality. Robert Carneiro (1970, 1988, 2000) argued that circumscription was a major process in the emergence of hierarchy and the origins of the state. These two are not so much opposed, as complementary. The larger the area circumscribed by various barriers the longer and more complex will be the emergence of states and hierarchy.

David Anderson (1994, 1996; see too Hall 2001a) describes in detail how chiefdoms cycle from one to three levels of hierarchy. His argument does not differ radically from Bodley's. Anderson does add an issue of chiefly succession mechanisms as a factor in a collapse from three levels to one, and gradual demographic growth as a factor in movement from one to three levels of hierarchy. Analogous processes can occur among states, empires, and world-systems. It appears that the larger the spatial extent, the longer the temporal cycles, but such a correspondence is far from perfect.

The point here is not to argue the specifics of each of these approaches. Rather, the point is to underscore that issues of scale also play a role in comparative strategies. In general, the shorter the time scale, and the more restricted the spatial scale, the less important changes at the higher levels will be. However, this does *not* mean they are irrelevant. Rather, for restricted comparisons they may be treated more or less as constants. Still, the value of that constant may be critical in and of itself. This point will be illustrated in the next section where contributions from world-systems analysis for comparative study of frontiers are discussed further.

CONTRIBUTIONS OF WORLD-SYSTEMS ANALYSIS TO COMPARATIVE STUDY OF FRONTIERS

I begin with a quick sketch of the utility of world-systems analysis for the comparative study of frontiers, highlighting those that are most germane. Chase-Dunn and Hall (1997) show that all world-systems pulsate, that is, expand and contract or expand quickly then slowly. Pulsations are one of many cycles that characterize all types of world-systems. Karl Butzer (1997) pointed out in another context, that the presence of cycles, no matter how crude, is evidence that there is a system.

Chase-Dunn and Hall (1997:275) defined a world-system in the following general terms: "intersocietal networks in which the interactions (trade, warfare, intermarriage, information, etc.) are important for the reproduction of the internal structures of the composite units and importantly affect changes that occur in these local structures." This definition is necessarily abstract to fit all sorts of systems. Note well that this definition leaves as an empirical issue how many "levels" (core, semiperiphery, periphery) a system has, and whether those levels are merely different (core-periphery differentiation) or have some form of hierarchical relationship (core-periphery hierarchy). They explicitly argue that the transformation of core-periphery

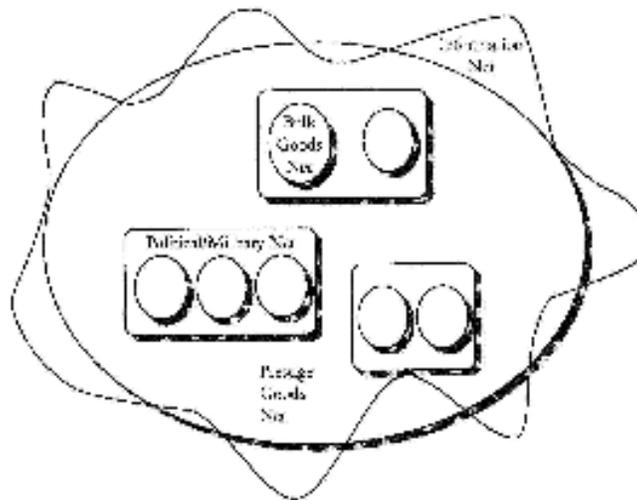
differentiation into core-periphery hierarchy remains a thorny empirical and theoretical problem. Of course, none of these issues applies to the modern world-system.

Bounding world-systems is problematic. Chase-Dunn and Hall (1997) argue that world-systems typically have four sets of boundaries [see Figure 3], marked by sharp falloffs in density of exchange of bulk goods, political and military interactions, luxury or prestige goods exchanges, and information exchanges:

- a bulk goods network (BGN) a system of exchange of low value to weight goods;
- a political-military network (PMN) of regularized military or political interactions;
- a prestige or luxury goods network (PGN) of more or less regular exchange of high value to weight goods; and
- an information or cultural network (IN) of regularized exchanges of information.

The sharp lines in Figure 3 give a false sense of precision. Rather, one should imagine a contour map of the density of exchanges in these networks. The “boundary” of any one network would be a region where there is a very steep fall off in exchange densities. In this sense, they are frontiers internal to a world-system. Exchange must be systemic – even if inchoate – to form a boundary zone. Note, too, that whether a specific good is a bulk or luxury good is only partially a property of the good itself. Whether a good is bulk or luxury is also a consequence of transportation costs, and productive use of the good in both the sending and receiving economies.

Figure 3: World-System Networks



(from Chase-Dunn and Hall 1997:54)

Incorporation into World-Systems

As noted, one dynamic property of world-systems is that they all pulsate, with a net tendency to increase in size and incorporate new territories and/or peoples (this discussion draws heavily on

If zones of incorporation are, or become frontiers, then clearly a large number of processes can create many different types of frontiers. Furthermore the types, densities, and social processes in those frontier zones have changed with the evolution of world-systems.

Recent work on synchrony among population, city size, and empire size (e.g. Chase-Dunn and Hall 1997; Chase-Dunn, Manning and Hall 2000; Chase-Dunn and Manning 2002) shows that even low levels of incorporation can cause various cyclical processes to become synchronized. Similarly, related work based in faunal population ecology models has begun to explain cyclical processes and synchrony among cyclical processes among ancient world-systems (Turchin and Hall 2003; Hall and Turchin 2007; Chase-Dunn, Hall, and Turchin 2007). A major insight from these studies is that it takes relatively low levels of interaction, such as long distance luxury trade, to synchronize various social processes. Such processes may account for what may seem like “action at a distance” among geographically widely separated frontiers. It also suggests one of the world-system mechanisms by which system-level events shape, or serve as a context for, frontier processes.

Finally, these zones of incorporation, or frontiers, are arenas where processes of identity formation, reformation, transformation, and annihilation are both more contested and more visible at several scales simultaneously than is typically the case in core regions. This approach in essence argues that Hill’s (1996a, 1996b) critique of world-system analysis with respect to ethnogenesis and ethnocide, is actually an instance of how world-system processes work out in space and time.

It is important to note here that none of these relations can be readily discerned without using a world-system approach. Comparative frontier strategies, which typically compare only a small number of frontiers (less than five, often only two), are insufficient to uncover such processes.

I have emphasized the weaker pole of the continuum of incorporation where boundaries are especially fuzzy. An important point, both methodologically and theoretically, is that this “fuzziness” is not a result of poor measurement or indicators. Rather, it is a reflection of an underlying reality that is fundamentally indeterminate. This suggests that as more knowledge and data are collected fuzzy set analysis (Ragin 2000) may be a useful tool for studying these processes.³

My studies of European intrusions into what is now southwestern United States show that even very mild forms of incorporation can have drastic effects on incorporated regions and peoples (see especially Hall 1989a for detailed examples; Hollis 2005 shows similar processes for Southeastern U.S.). Following Sing Chew’s work (2001, 2007) on ecological degradation, the ecological footprint of an urban area is quite large, extending into a large hinterland, or in the terms used here into regions that are only mildly incorporated. In both these sets of examples the degree of incorporation is only along the prestige goods and/or information networks, and often at

³ Ironically students of globalization who claim that contemporary state boundaries are becoming less important, and that transnational process are becoming more important, are arguing and documenting how formerly relatively sharp boundaries are becoming fuzzy, or vague. For instance, Sklair (2002) and Robinson (2004) provide plausible evidence for the formation of a transnational capitalist class. The formation of such a transnational class is rendering borders less important and vaguer. This is a process that seems to be the inverse of world-system expansion and incorporation. It merits further theorizing, but is beyond the intent of this paper.

a low level. Still the effects on local areas can be profound. An effect R. Brian Ferguson and Neil Whitehead (1992a, 1992b), Jonathan D. Hill (1996a), and Ken Coates (2004) have also noted (though these writers do not use the term incorporation nor do they use world-systems analysis).

These world-system processes are a major source of the puzzle of frontiers -- that they all seem similar, but upon closer examination, each frontier seems unique (Hall 2001b, but see also Lamont and Molnár 2002; Lightfoot and Martinez 1995; Rice 1998; Rice and Rice 2005). Frontiers can also give rise to buffer zones that are emptied of residents, and thus leave room for local ecologies to recover from over use (Martin and Szuter 1999; Rice and Rice 2005). Furthermore, processes that first occur in newly incorporated regions may rebound back to more central areas to produce further changes there. Indeed, this seems to be a part of why semiperipheral marcher states are often instigators of significant system change (see Chase-Dunn and Hall 1997; Hall, Chase-Dunn and Niemeyer forthcoming; Lattimore 1980).

As noted, frontiers are zones of ethnic change: formation, transformation, amalgamation, fractionation, or destruction. Destruction of an ethnic group does not necessarily require death of members of the group, it can also be due to destruction of the identity (ethnicide, or for some, assimilation) or destruction of a culture (culturicide). In such processes all levels of context interact. This can be very difficult to disentangle.

In *Captives & Cousins*, James Brooks (2002) chastises me for being far too structural. He is voicing a common criticism of any structural analysis: actors are left out, it is too abstract. It is a criticism with *some* merit, but only *SOME*. It calls to mind a comment by Robert Pirsig (1974:26) about how two of his motorcycling companions could not be bothered with the technology of motorcycle maintenance: "The Buddha, the Godhead, resides quite as comfortably in the circuits of a digital computer or the gears of a cycle transmission as he does at the top of a mountain or in the petals of a flower."

Analogously, what happens as local history is still part of a world-system, and plays an important role in its overall dynamics and evolution, and is a vital part of the system's history. Furthermore frontiers, with their great messiness and myriad anomalies, are great places to "seize the Buddha," to see how local processes connect to global and long-term processes.

Yet, of the things Brooks does very well – and he does many things well – is to show the complexity of ethnic and racial interactions along the Southwestern frontiers, note the S. But he then leaps to the erroneous conclusion that race and ethnicity are not key structural factors. This is similar to Janet LeCompte's (1985) claim that the revolts in northern New Mexico in 1836 in Chimayo, then again in 1847, wherein new American governor Charles Bent had his spine relieved of the burden of supporting his head, had nothing to do with race or class. Andrés Reséndez (2005) gently notes that the major players in both rebellions were Pueblo Indians. Nope, race was not an important factor!

Both Brooks and LeCompte missed the Buddha. Maybe they only took intro to Sociology wherein all too often race, class, and gender are painted with far too broad of a brush. I do not think that this is the case. Rather, to go back to Pirsig, they did *not* see the Buddha because they were *not* looking for him. In less metaphorical terms, structural processes, world-systemic, ethnic, and so on operate in interaction with other processes, especially local processes and individual agency in complex ways. This is well illustrated in Ned Blackhawk's (2006) *Violence over the Land* which carefully details such interactions and their multifaceted consequences for and resistance to larger processes that disrupted Paiute social life from the eighteenth through the twentieth centuries.

What a world-system approach contributes to all of these studies are explanations of why these supposed individually motivated processes (a.k.a. agency) occur where they did (on frontiers) and when they did (when the cyclical condition of state strength was insufficient to regulate such interactions). Put another way, world-system analysis explains how the structural conditions originate that allow individual agency to have such force locally.

This underscores another methodological (and theoretical) contribution of world-systems analysis to the study of frontiers. Frontiers, peripheries, and other “obscure” places offer many opportunities to observe and understand the interactions among local, regional, state-level, systemic, and since the late twentieth century global processes. Indeed, occasionally they are the only place where these interactions can be studied because core, and even semiperipheral process are so strong that these more subtle interactions are all but invisible (Hall 1989b). In short, frontiers are great places to seize the Buddha: to draw the connection between global, *longue duree* and the most micro of interactions. This is where empirical studies of frontiers can contribute to refined world-system theorizing. Conversely, this is where world-systemic processes can account for contexts which promote or inhibit levels of individual agency.

I illustrate some of these interactions in a brief discussion of nationalism and identity on frontiers.

IDENTITIES, ETHNICITIES, RACES, AND NATIONALISMS ON FRONTIERS

Frederik Barth (1969) in *Ethnic Groups and Boundaries* claimed: “The critical focus of investigation from this point of view becomes the ethnic *boundary* that defines the group, not the cultural stuff that it encloses.” Barth’s key argument is that such boundaries often coincide with ecological boundaries, where, given a fixed technology, the mode of making a living must change at the boundary. This is a key element in such differences as between the steppe and sown, between hills and valleys, and so on. It also stands on its head an all too common approach that says that ethnic differences create boundaries and frontiers.

Frontiers are where identities crash, conflict, coalesce, and become conflated. Frontiers are ideal places to study how identities are born, shaped, reshaped, and destroyed. Yet they are also full of contradictions, dissembling, confusions, and murky processes. Hence they are fascinating and frustrating areas to study. But I think social scientists have been wont recently to exaggerate the importance of identities. We too often give them causal primacy, or in basic methods terms, view them *only* as independent variables, even while we are expounding on how they are socially constructed, which means we are claiming they are dependent variables.

Barth’s claim, and my claim which is a bit more forceful, may seem counter intuitive. To a large extent they are. There seems to have been a vast increase in recent decades of ethnic conflict and violence as documented by Gurr (1993, 2000) and Harff & Gurr (2004). Yet Wilma Dunaway (2003) argues, that there has not so much been an increase in ethnic conflict and violence as an increase in its costs to global capitalism. Clearly, identity is vitally important.

My argument here is that identities, as manifest in ethnicity, race, nationalism, and so on are more often consequences, results, or outcomes of social processes than they are causes. To be sure, once they are in place they become part of complex feedback or recursive processes, which when narrowly viewed appear to play causal roles. But in highly recursive systems the concept of cause between variables itself becomes very muddy at best. This is part of Barth’s point, too as I

read him and his examples: it is the boundary that maintains the ethnicity, not the reverse. In short, identities are what *need* to be explained. Before proceeding with this line of argument and connecting it to the study of frontiers, I want to return to a critical comment on Karl Marx (Hall & Fenelon 2003, 2004).

In Charles Kenner's (1969) *History of New Mexican - Plains Indians Relations*, James Brooks's *Captives and Cousins* (2002), and Ned Blackhawk's *Violence over the Land* (2006) present accounts about how during raids nomadic Indians routinely killed all males at or beyond puberty, but took women, and minor children of either sex, captive or sold them off to Spaniards for slaves, or traded them to redeem their own people who had been captured by Spaniards. These accounts describe how such children were often absorbed into the societies of their captors and grew up to become full-fledged members of the group, at least culturally. Here a parenthetical note that the peoples the Spaniards were accustomed to call *los indios bárbaros*, did this much more often than Spaniards who typically consigned captives to a degraded ethnic category of *genízaro* (Hall 2004a; Weber 2005). The surprising lesson is that these captured children grew up to be quite healthy adults, at least within what was considered healthy in their new home societies. In short, their identities changed.

So if identity is "so easily" changed, especially if we consider myriad instances where individuals or groups – here Barth's accounts come to mind – voluntarily shift identities, how can it be more important than class, which in practice is often more difficult to change? Part of the answer is that identity communities are forms of social organization not based capitalist relations. This is *not* a "primordialist" or "essentialist" claim. Rather, it is a recognition that the fundamental social link in identity groups is one of kinship and community, irrespective of how they make their living. This is where Marx errs. He failed to recognize that ties of common work experiences – relations of production – are often *not* sufficiently powerful to overcome completely ties of kinship and face-to-face community. This is why both nations and movements always adopt metaphors of kinship to build solidarity; or to invert Benedict Anderson (1991), that is why the "imagined community" must be imagined. It must be socially constructed because it is not there spontaneously (Geary 2002). This is not to gainsay that such a transformation might happen, but rather to note that such transformations are rare. But here Marx deserves some credit on this issue. Although humans construct their own histories, and their own identities, they cannot do so any old way. There are constraints. Figuring out what they are, how they work, and just how constraining they are remains to be done, though many of the new western historians have begun to do so (e.g., Blackhawk 2006; Brooks 2002, Limerick et al. 1991; Reséndez 2005; Robbins 1994; Slatta 1997; Weber 2005). Furthermore, world-systems analysis offers ways to delineate where and when such processes are more, or less, likely. Consideration of the phases of various world-systemic cycles, degree of incorporation into a world-system, and world-system position – core, periphery, semiperiphery – and relative change, rising or falling in world-system position are all important contextual conditions to analyzing the timing and location of many different kinds of ethnic change.

Indeed, identity groups, such as ethnic groups, are typically constructed historically. Where the historical depth is sufficient (what is sufficient is itself problematic), it can often appear "natural," "primordial," "essential," or "racial." In those times and places where humans did not move far from their home territories there is in fact a real association of phenotypical features, culture, and identity. This association, however, is not "natural," "normal," or "primordial," but an artifact of relative geographical stability, which itself is historically rooted

(for further details see Hall 2004b). If 19th century factory organization had remained stable for many decades or for centuries, class might have become a major identity group. However, identity groups in fact change and transmute through time, fragmenting, amalgamating, and exchanging members. Where these processes are slow, they *may appear* to be racial or biological because their social and historical construction is not readily perceptible to a casual observer. Where they are faster the social construction is more apparent. Hence, the increased concern with the social construction of identity in recent decades.

It is precisely because such processes are more contested and often change more rapidly in frontier regions that frontiers are good places to study the processes of such changes. In fact in frontier zones it is ethnic or identity *persistence*, rather than change that is problematic. This is precisely why studies like Brooks's (2002) *Captives and Cousins* can be insightful. However, Andrés Reséndez's (2005) *Changing National Identities at the Frontier* or Ned Blackhawk's (2006) *Violence over the Land* are also useful. These two accounts focus precisely on the interplay of local, regional, state-level, and world-systemic forces in shaping these ethnic processes. In the longer term perspective used here, one could add part of the shaping is because these processes were occurring on the peripheries of nascent capitalist states. Quite often, but not always, tributary states had far less concern with identity and culture, and more concern with minimizing resistance and a facilitating trade and collection of taxes (or tribute).

In short, although the kind of world-system was not changing, or not changing rapidly during the periods of these studies, its position – nascent capitalist states – was an important background 'constant' to other processes of change operating at a much faster time scale.

So what kind of methodological lessons might we draw from these accounts?

CONCLUSION: STRATEGIZING COMPARISONS AMONG FRONTIERS

Clearly, variations can be spatial, temporal, physiographic, or organizational, different kinds of native peoples, and different sorts of settlers. So these are all factors that must be considered in comparisons. Also important are type, phase, type of boundary, and state of the world-system(s) that are shaping frontiers and that frontiers are influencing. It would also seem reasonable to consider whether the frontier was on the edge of a world-system, whether it be at the bulk goods, political-military, luxury goods, on informational edge or along some internal boundary. Internal boundaries could be between states or groups in similar positions within the world-system (e.g., core, periphery, or semiperiphery) or they could be between these different zones. A reasonable working hypothesis would be that these two broad categories of frontiers would exhibit different dynamics. Blackhawk's (2006) study focuses more on the edge of the system, whereas Reséndez's (2005) study is concerned with the processes of identity change within the system (as what is now southwestern United States was wrested from Mexican control).

A subsidiary hypothesis might be that frontiers between different positions in a world-system might also differ. Here it is useful to recall Chase-Dunn and Hall's (1997) claim that how many layers, and how differentiated they are in a world-system is an empirical as well as a theoretical problem.

So how should one make comparisons? One strategy would be to choose frontiers that vary only along a few dimensions. This was largely the strategy in *Contested Ground* (Guy and Sheridan 1998a, 1998b) which compared the extreme northern and southern edges of the Spanish

Empire in the Americas. World-system position and dynamics, if not constant, changed in similar ways for most of the empire. This facilitated closer focus on types of indigenous organization, ways of making a living, and various geographical features.

Often, however, one does not have the luxury of having appropriate frontiers to compare. Rather, one must examine the cases at hand to figure out what is different and what is the same, then investigate whether some of those differences are causally, or recursively, connected to other changes. What differentiates this approach from ‘garden variety compare and contrast’ approaches, is the harnessing of its theoretical concerns with an eye to discovering new variables in frontier processes.

Another strategy, discussed extensively by Richard W. Slatta (1997), is to examine one institution, the cattle ranch, in many different frontier settings. While for the most part nineteenth-century cattle ranches employed similar technologies for cattle breeding, raising, and controlling, they were often located in different kinds of markets, and connected in the world-economy in different ways. A major factor in the transformation of cattle ranches was the development of reliable refrigeration technology in the late nineteenth century which allowed more extensive and tighter connections between the ranches and their potential markets. These had massive effects on the ranching industry (Slatta 1990).

In *Class & Community* Richard Hogan (1990) follows a similar strategy, arguing that the miners and farmers in nineteenth century Colorado developed divergent self-governing styles out the interaction of the specific relations of production with larger society. Given that both mining and farming are primary extractive industries, this is quite interesting. It suggests that “extractive industry” is too inclusive of a category, which has many important variations. As in the *Contested Ground* (Guy and Sheridan 1998a) project this comparison holds relatively constant position in the world-system, and world-system time. This allows foregrounding the critical role of the relations of production and the roles of individual actors.

Many case studies of frontiers can be [re]interpreted as ‘incorporated comparisons’ (McMichael 1990) which are often a series of comparisons within an historical trajectory of a case. In *Colony and Empire* William Robbins (1994) examines the American West and argues that far from being an open or free frontier, it was highly constrained by the demands of assorted capitalist enterprises. This, of course, makes sense within a world-systems framework. But it also sheds a different light on the common claim that in Canada the law arrived before the settlers, whereas in the United States the settlers preceded the law, and hence the western U.S. was far more violent than western Canada. Rather, frontier violence served the interests of capital. Blackhawk (2006) shows that violence served to ‘ethnically cleanse’ the U.S. west of indigenous peoples. This, in turn can be seen as yet another instance of ‘war in the tribal zone’ (Ferguson and Whitehead 1992a, 1992b; Whitehead 2004a, 2004b). Here it interesting that the ‘war in the tribal zone’ effect occurs in both tributary and capitalist world-systems. Thus, as with ecological degradations discussed by Chew (2001), a key component of change is the state, irrespective of the mode of accumulation.

In analogous ways comparisons of modern and ancient frontiers show that genocide, ethnocide, culturicide, ethnogenesis, amalgamation, and fractionation are common processes on many different frontiers. What remains to be studied systematically is how various local, regional, state-level, and world-system conditions and dynamics shape these processes. Again, states seem to be as important as mode of accumulation and local relations of production in shaping ethnic change.

This entire discussion can be useful in the study of other kinds of social structures and processes. That is, these world-system considerations in strategically making comparisons potentially have very broad applications. However, they need to be adapted to specific theoretical and empirical concerns. In particular, it is in these kinds of studies that one of the common critiques of world-systems analysis can be answered: that it ignores local actors and/or non-economic processes.

Finally, the study of frontiers illustrates how much can be learned by the study of peripheral regions and peoples and their roles in system change (Chase-Dunn and Hall 1997, Ch. 5; Hall 1989b; Lattimore 1980; Hall and Fenelon 2009). Indeed, some of these processes may be visible only in peripheral and/or frontier areas. This then becomes a method to explore how it is that actions and changes in peripheral areas (and semiperipheral areas) play important roles in world-system evolution. A key point here is that many if not most of these questions can only be asked from a world-system perspective, even if they must be answered in large part locally.

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**LOOKING BACK AND FORGING AHEAD: THIRTY YEARS
OF SOCIAL NETWORK RESEARCH ON THE WORLD-SYSTEM¹**

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ABSTRACT

We review three decades of research linking social network methods with world systems theory. We identify four themes nested within two versions of a general social network methodology—the identification of network Roles and Position. The themes vary by the type of data and the definition of equivalence used to identify roles and positions. Second, we provide a demonstration of the general methodological approach taken in the literature, applying a recent methodological innovation to a newly compiled large global trade dataset. The

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results identify the expected core / periphery interaction pattern, suggesting that it is a fundamental feature of cross-national trade data, regardless of how the data are analyzed. We conclude by suggesting both methodological and substantive directions for future social network research on the world-system.

INTRODUCTION

At the center of the world-systems perspective is the intuition that structure (is all that) matters. Wallerstein stresses that focusing on social structure leads to a radical transformation in insight:

Once we assume that the unit of analysis is such a world system and not the state or the nation or the people, then much changes in the outcome of the analysis. Most specifically we shift from a concern with the attributive characteristics of states to concern with the relational characteristics of states. (Wallerstein 1989:xi)

It is therefore unsurprising that a thriving research tradition exists that brings together a relational-structural orientation—the world-systems perspective—with a relational-structural methodology—social network analysis (SNA).

This article's contribution to this special issue on research methods for world-systems research in the *Journal of World-Systems Research* is three-fold. First, we review the literature on the world-systems perspective that uses social network analysis. There are four main substantive themes addressed in this literature. These include studies which (1) assess the extent to which cross-national relational data exhibit a core / periphery structure; (2) delineate boundaries between core, periphery and semi-periphery; (3) adjudicate between the core / periphery distinction as categorical or continuous; and (4) assess the hypothesis that some form of "unequal exchange" occurs across zones of the world-system. Several studies also combine one or more of these categories with an effort to assess levels of mobility and economic growth over time. These four categories are nested within two general methodological approaches to the identification of world-system structure, which differ on what type of relational data is used. The first approach combines economic (total trade) with non-economic (treaty co-membership, military interventions, diplomatic exchanges) data (Snyder and Kick 1979). The second approach distinguishes between different types of commodity trade as the basis for analysis (Breiger 1981; Nemeth and Smith 1985).

Second, we create a detailed analysis of world trade data in order to provide a more extensive explanation for world systems scholars unfamiliar with social network methods. This section demonstrates an up to date variant of the classic Role and Position approach taken in the literature. As such, this section provides practical suggestions on decision making through each step of the analysis. This includes explaining the difference between various equivalence criteria and how to choose between them, how to conduct both categorical and continuous analyses, and how to simultaneously analyze multiple relations. Finally, this section assesses the extent to which some classic findings are "robust" to multiple methodologies by comparing our results from the Role and Position analysis to those from a recent methodological innovation, the Exponential Distance Model (EDM).

Third, we conclude by discussing recent methodological innovations in social network analysis with an emphasis on statistical social network methods, and by suggesting potential fruitful avenues for future research. Our goal for this final section is to stimulate interest in applying social network methods to world systems research and to suggest new substantive areas for research in this area that could add to our understanding of world-systems analysis.

WORLD-SYSTEMS THEORY AND NETWORK ANALYSIS: A REVIEW

If there is anything fundamental about world-systems analysis it is the structural intuition that countries occupy stratified positions in the world-system—core, periphery and semi-periphery. While there is some debate as to whether these are qualitatively distinct positions, or rather ideal-typical categories reflecting an underlying hierarchical continuum (e.g. Arrighi and Drangel 1988; c.f. Chase-Dunn and Rubinson 1977), they are overshadowed by the widespread acknowledgement that the world-system consists of a hierarchically organized structure of states, in which core countries are the most advanced, peripheral countries the least advanced, and semi-peripheral countries somewhere between the other two categories.

Further, the structure of the world-system is seen as the major source of variation in the distribution of the returns to capitalist enterprise. While many mechanisms have been proposed, the central idea is that the boundary lines between core, peripheral and semi-peripheral zones of the world system demarcate distinct roles in the international division of labor and that the world economy systematically distributes wealth from peripheral and semi-peripheral countries to core countries (Wallerstein 1974). The concrete forms of economic activity that constitute core and peripheral activities have changed over the course of world-systemic development. Nevertheless, an analytical distinction between a manufactured goods-producing core and a raw material / primary goods-producing periphery has provided a useful description of the territorial division of labor in the modern world-system through roughly 1980 when the Global South experienced a dramatic rise in manufacturing activity (Chase-Dunn and Rubinson 1977; Dicken 2003).

One mechanism proposed as an explanation for inequality in the world-system is that the core's dominant position in the world-system generates the highest "concentration of innovations in new lead industries" (Chase-Dunn and Grimes 1995: 397). Thus, semi-peripheral and peripheral countries are simply excluded from the most profitable activities in the world economy (e.g. O'Hearn 2001). Another major proposed mechanism is that of "unequal exchange" (Frank 1969; Galtung 1971; Emmanuel 1972). While several variants of unequal exchange exist, they all share the view that the world-system involves an asymmetrical flow of surplus value between core and non-core countries which results in the unequal distribution of wealth.²

Social network analysis (SNA) is a structural approach to studying the relations between entities in a social system. It has its own unique concept of a core / periphery structure that parallels some of the structural lines of thought in the world-systems perspective. In SNA, a core /

² One example that fits the older territorial division of labor was the tendency for primary good prices to fall vis-à-vis the price of manufactured goods. This resulted in a greater share of surplus value accruing to the manufacturing exporter vis-à-vis the primary good exporter (Prebisch 1950; Singer 1950).

periphery structure is one that contains a dense and cohesive³ subgroup of core actors who exchange ties with one another and with a group of peripheral actors who tend to have ties with the core but not with each other, illustrating dependency (e.g. Boyd et. al 2006a; 2006b; Borgatti and Everett 1999). Thus, the core / periphery concept in social network analysis captures the extent to which a given graph has a latent core / periphery interaction pattern among actors in the network, and has been implemented across a wide array of substantive contexts, including epidemiology (Jolly et al. 2001; Christley et al. 2005), small groups (Cummings and Cross 2003), interpersonal networks (Bourgeois and Friedkin 2001), linguistics (Dodsworth 2005), groups in isolated or extreme environments (Johnson, Boster, and Palinkas 2003), networks of creative artists (Uzzi and Spiro 2005), PhD exchange networks (Burris 2004), and knowledge communities of firms (Giuliani and Bell 2005).

Table 1: Ideal-Typical Core / Periphery Structure

	Core	Semi-Periphery	Periphery
Core	100	70	30
Semi-Periphery	50	20	10
Periphery	10	0	0

Table 1 represents an ideal-typical image matrix that reflects a social network conception of a core / periphery structure. An image matrix is a simplified representation of a set of relational data in which the rows / columns represent subgroups of actors who are similar to each other by some criterion, and the cells represent either the presence / absence of a tie (in the case of dichotomous data) or the strength of a tie (in the case of valued data) between the two groups. The rows of an image matrix represent ties sent from the row group to the column group. In the context of international trade, for example, the rows give the pattern of “exports,” while the columns represent the pattern of “imports.” Keeping with the example of trade, Table 1 represents a hypothetical situation in which actors are assigned to groups based on the similarity in their patterns of interaction with other actors in the network, and illustrates a latent core / periphery structure in four respects. First, the densest interaction occurs between actors in the core group, as indicated by a high value in the core-core cell of Table 1. Second, the core group has the largest “reach” throughout the network as indicated by the core-group’s large flows to and from the other two groups. Third, the periphery is isolated from itself as indicated by the zero value in the periphery-periphery cell in Table 1. Finally, the periphery is “dependent” on the core for both sending and receiving ties, as indicated by the fact that the periphery has only non-zero cells in both the core-periphery and periphery-core cells in Table 1. Evidence of some form of “unequal exchange” is also implied by the trade surplus of the core (e.g., exports are larger than imports) in

³ The density of a set of relations refers to the ratio of ties that exist within a group to all possible ties within a group, while cohesion refers to the extent to which interaction is much more intensive within a group than between one group and another (Wasserman and Faust 1999: 101-103, 267-270).

their relations with both the semi-periphery and periphery, as indicated by the fact that the cells above the diagonal are larger than those below the diagonal.

What becomes evident at this point is that the core / periphery concept in world-systems analysis and that in social network analysis are distinct entities: where one is an approach to understanding how (among other things) various positions in the world-system cause divergent levels of accumulation over time, the other is a category of social structure defined by a specific type of interaction pattern. Despite these distinctions, a long-standing research tradition provides empirical confirmation of a number of critical world-system ideas by utilizing SNA techniques to operationalize various structural intuitions of world-system theory.

The logic behind the empirical operationalization of world-system structure with SNA methods can be summarized as follows: if there is, “in reality,” an interconnected international division of labor in which “core” countries occupy dominant positions in the world economy, and peripheral and semi-peripheral countries occupy comparatively subordinate positions, then core actors in the international division of labor should be expected to have similar trade patterns vis-à-vis each other, but dissimilar trade patterns vis-à-vis peripheral or middling position countries (Arrighi and Drangel 1986; Hopkins and Wallerstein 1977, 1986). In short, patterns of trade between countries constitute a relational structure in which some positions—core positions—encourage relatively autonomous activity while others—peripheral positions—encourage constrained or dependent activity. Indeed, this structural intuition even predates the world-systems perspective, and can be traced to early scholars such as Albert Hirschman ([1945] 1980) and Johan Galtung (1971). Given this understanding of the way in which cross-national relationships generate a social structure in which power, prestige and disadvantage vary by position in that structure, we will now trace the lineage of world-systems research using SNA techniques.

Early SNA studies focused on a method designed to identify the Roles and Positions of entities in a set of relational data (Wasserman and Faust 1994). At a conceptual level, the method starts with a relation (or set of relations) and (1) estimates the degree of similarity with an “equivalence criterion,” (2) uses these estimates as the basis for assigning actors to relatively equivalent structural positions (either categorically, continuously, or both), and sometimes (3) determines the role of each equivalent group by analyzing the relations within and between equivalent groups (or “blocks” in the block model literature). This general approach has been treated extensively—both with reference to the world economy and with other substantive areas—in each of the “big three” generalist sociology journals: *American Sociological Review* (Mullins et al. 1977; Van Rossem 1996); *American Journal of Sociology* (Boorman and White 1976; White et al. 1976; Snyder and Kick 1979; Alderson and Beckfield 2004) and *Social Forces* (Anheier and Gerhards 1991; Smith and White 1992; Mahutga 2006).

One of the major methodological advancements in the SNA literature is the evolution of the equivalence criterion. Early SNA studies used structural equivalence as the criterion, which required two actors to have identical relationships to identical others, e.g., correlations of 1 on the row and columns of a socio-matrix. Later studies relaxed this requirement, allowing two actors to have identical relations with *equivalent* others. For example, despite a high degree of similarity on the patterns of exchange with other countries, the US and the UK would not be considered structurally equivalent because the US has ties to peripheral countries in Latin America, while the UK has ties to peripheral countries in Anglophone Africa. They share the same type of ties, for example, they may both exchange machinery for cocoa with peripheral countries, but they do not

meet the criteria of having the same ties to the same set of countries. This relaxation is called regular equivalence, and quantifies the extent to which two actors have similar relationships with *equivalent* others, rather than *identical* others. It is less restrictive and therefore a more general type of equivalence that (arguably) better captures the notion that core actors are equivalent by virtue of their similar relations to equivalent others (peripheral actors) (see Faust 1988; Borgatti and Everett 1992; Wasserman and Faust 1994). We will elaborate further on the particulars to this method following our overview of the world-systems literature using SNA techniques.

Table 2: Major Articles Reviewed

Authors	Year	Economic and Non-Economic data?	C/P	Zonal Boundaries	Discrete or Continuous?	Unequal Exchange?	Other
Snyder and Kick	1979	Yes	X	X	---	---	Growth
Breiger	1981	No	X	---	---	---	Intra-core competition
Nemeth and Smith	1985	No	X	X	---	X	Growth
Smith and White	1992	No	X	X	X	X	Mobility
Van Rossem	1996	Yes	X	X	X	---	Growth
Kick and Davis	2001	Yes	X	X	---	---	Growth
Mahutga	2006	No	X	X	X	X	Mobility

Table 2 delineates four main categories of inquiry with which researchers use social network analysis to evaluate world-systems hypotheses within the general methodological framework of Roles and Positions. These include studies that (1) assess the extent to which cross-national relational data exhibit a core / periphery structure; (2) delineate boundaries between core, periphery and semi-periphery; (3) adjudicate between the core / periphery distinction as categorical or continuous; and (4) assess the hypothesis that some form of “unequal exchange” occurs across zones of the world-system. Several studies also combine one or more of these categories with an effort to assess levels of mobility and economic growth over time.

These four categories are nested within two approaches to the identification of world-system structure that differ with respect to the kind of relational data used. The first approach combines economic (total trade) with non-economic (treaty co-membership, military interventions, diplomatic exchanges) data (Snyder and Kick 1979). The second approach distinguishes between different types of commodity trade as the basis for analysis (Breiger 1981;

Nemeth and Smith 1985). The remainder of this section will focus on the empirical world-systems literature that uses social network analysis and follows one of these two data collection approaches. The literature is organized chronologically within each of these two approaches in order to illustrate the evolution of the field.

We begin with the classic network study of the world-system by Snyder and Kick (1979). The article was largely confined to the first and second categories above—delineating the existence of a core / periphery world-system and the boundaries between the core, periphery and semi-periphery. Snyder and Kick were also the first to assess whether or not occupying a high position in the core / periphery hierarchy actually did predict higher growth, as opposed to simply showing that countries that manifested some form of “dependency” had slower than average growth. Using data on four types of global relationships (trade, military interventions, diplomatic exchanges and treaty memberships) collected between 1960 and 1967, they applied a structural equivalence algorithm—CONCOR (Breiger et al. 1975; White et al., 1976)—as their equivalence criterion. Of these four types of relationships, they found the hypothesized core / periphery interaction pattern to be evident primarily in the trade relationships. The authors made two other contributions. First, they argued for greater nuance in the periphery and semi-periphery categories, depicting three partitions within the semi-periphery and six smaller partitions in the periphery. Second, using OLS regressions, they found a difference in growth rates between categories, noting that the core group grew consistently faster than lower tier groups in the years studied (1955 to 1970).⁴ In a follow-up study, Kick and Davis (2001) used a structural equivalence analysis, which confirmed that the core was comprised of Western industrial countries, and that they dominated the world system in economic, transportation, communications, sociocultural, political and military networks. They concluded that the strength of international economic ties impacted domestic (national) economies and trajectories of overall economic growth and well-being (Kick and Davis 2001:1570-1573).

Van Rossem (1996) also combined economic and non-economic relationships, including imports, exports, arms trade, diplomatic exchange and presence of foreign troops as the basis for his analysis. He used a novel methodology—a role equivalency measure based on the triad census—to “test the world-system paradigm as a general theory of development,” (Van Rossem 1996: 508), and to address the question of whether or not the core / periphery hierarchy is best conceptualized as categorical or continuous. Van Rossem’s findings with respect to the first two categories in Table 2—assessing whether or not the network conforms to a core / periphery structure and assigning countries to groups—was largely consistent with previous studies, except he placed China, Brazil, Saudi Arabia and the Soviet Union in the core (using 1983 trade data). Unlike previous work, however, Van Rossem found that world-system position had no direct effect on economic growth, challenging expectations of world-systems analysis. Finally, Van Rossem’s secondary analysis suggests that “coreness” in the world-system is much more continuous than categorical, and that there are “large differences in power among core countries”

⁴ Snyder and Kick’s (1979) assignment of countries to positions in the world-system and various amendments by Bollen (1983) and Bollen and Appold (1993) has been the basis for a large number of empirical studies in which world-system position is an independent variable on which dependent variables such as income inequality (Alderson and Nielsen 1999), world-city position (Alderson and Beckfield 2004), IGO and INGO memberships (Beckfield 2003), urbanization (London 1987), democracy (Wejnert 2005), and others are regressed.

(Van Rossem 1996:518), an argument that is consistent with the world-systems literature on hegemonic cycles (e.g. Arrighi 1994; Chase-Dunn 1998).

In summary, the work of Snyder and Kick (1979) and those in its lineage are similar with respect to (1) incorporating both economic and non-economic data; (2) a particular focus on categories 1 and 2 from Table 2, by testing for a core / periphery structure and delineating the boundaries between zones of the world-system; and (3) using the results of network analysis as independent variables to explain subsequent growth. The major difference rests primarily with the work of Van Rossem (1996) who (1) uses a less stringent equivalence criterion—"role equivalence;" (2) estimates growth regressions that largely contradict the hypothesis that world-system position should have a significantly positive direct effect on economic growth; and (3) asks the additional question of whether or not the core / periphery structure he identifies is categorical or continuous.

The second broad approach begins with Breiger (1981) and is further developed by Nemeth and Smith (1985), Smith and White (1992) and Mahutga (2006). These authors also address categories 1 and 2 from Table 2, but they differ from the Snyder and Kick lineage in two ways. First, they base their analysis solely on economic data in the form of commodities that are roughly classified according to their levels of industrial "sophistication." Second, they add a third and fourth category that focuses on the continuous vs. categorical nature of the world-system and attempt to operationalize the notion of unequal exchange. In what follows, we review the works of Breiger (1981), Nemeth and Smith (1985), Smith and White (1992) and Mahutga (2006), the most recent work in the lineage. Following the review we will summarize the similarities / differences between this lineage and that of Snyder and Kick (1979), as well as the evolution of the field as a whole.

Chronologically, the first piece is that of Breiger (1981). Although Breiger restricted his focus to relatively wealthy developed countries of the Organization for Economic Cooperation and Development (OECD), his goal was to connect a more general social structural approach to international trade and the typical approach still prevalent in economics. The latter approach views trade between two countries as the linear function of individual attributes, such as GDP and population, and geographical distances from potential partners (e.g. Feenstra et al. 2001). Breiger used an identical analytic strategy to that of Snyder and Kick (1979), e.g., a structural equivalence criterion with the CONCOR program to analyze four types of trade relationships (Agricultural Products, Raw Materials, Manufactured Goods and Energy Resources). He found that even the OECD countries in his investigation engaged in a core / periphery interaction pattern among themselves across all four types of commodities. He also found "multiple competing centers" after adjusting for each country's overall volume of trade (Breiger 1981:375).

Following Breiger (1981), Nemeth and Smith's (1985) attempted to differentiate core, semi-periphery and periphery relations based on patterns of trade in commodities that embody different forms of industrial sophistication or capital intensity at a more specific level of aggregation than Breiger. Using CONCOR, they located a "strong" and "weak" semi-periphery, creating four distinct categories of trade relations. They also operationalized the notion of unequal exchange, and thereby discovered variation in interaction patterns across different commodity types and zones of the world-system (Nemeth and Smith 1985:544).⁵ Using regression analyses,

⁴ Another major contribution made by Nemeth and Smith (1985), and subsequently Smith and Nemeth (1988), was an attempt to categorize commodities based on the pattern of exchange in the

they demonstrated that the core had significantly higher wealth and lower child mortality than the weak semi-periphery and the periphery, and higher energy consumption, wealth generation and energy consumption than all lower blocks. They did not find significant differences between the core and lower blocks with respect to economic growth in percentage terms and in levels of within country inequality, however.

Smith and White (1992) built on these findings by 1) introducing a more general measure of equivalence—regular equivalence; 2) testing the theory of unequal exchange; and 3) conducting the first analysis of world-system mobility. The regular equivalence algorithm produced a matrix that assigned a level of equivalence between each actor that ranged from 0 to 1. This matrix served as the basis for subsequent analyses in which they were able to create both a continuous scaling—with a correspondence analysis of the equivalencies—as well as a 5 position block model by analyzing the equivalence matrix with a hierarchical clustering routine (Borgatti 1994). They identified five positions—core, strong semi-periphery, weak semi-periphery, strong periphery and a weak periphery—that corresponded to an increasing level of dissimilar trade patterns vis-à-vis the core. They were also the first to find empirical support for the notion that the structure of world trade was more fundamentally continuous rather than categorical.

In addition to the methodological contribution of regular equivalence, Smith and White provided stronger evidence of unequal exchange by showing that countries in higher zones of the world-system produced and exchanged sophisticated capital intensive manufactured goods for raw materials and labor intensive goods produced in lower zones (1992:880-882). Finally, Smith and White were the first to empirically examine the issue of mobility in the world-system. Using world trade data from 1965 and 1980, they found evidence of “much more upward than downward mobility” (Smith and White 1992:880). The reasons for the mobility were speculated upon but the question was left for future research.

Mahutga (2006) provided the most recent contribution in the lineage of Breiger (1981) and Nemeth and Smith (1985). He used an analytic strategy similar to that of Smith and White (1992) to evaluate how hypothesized structural changes associated with globalization and the new international division of labor (NIDL) affected inequality in the structure of the world economy in the period spanning four and half decades (1965 to 2000). He advanced this line of methodological by quantifying the fit of a core / periphery model to the data, levels of asymmetric commodity flow, and mobility—as well as temporal variation in these types of changes. His findings challenged some claims in the globalization literature of decreasing inequality by demonstrating that the core / periphery interaction pattern remained intact through 2000, that commodity exchanges across zones of the world-system remain unequal, and that the globalization era (1980-2000—or more recent wave) was associated with less structural change than the prior period (1965-1980) despite evidence of significant upward mobility of a small number of countries.

Our review identifies two discernable lineages with respect to network studies of the world-system: those beginning with Snyder and Kick (1979), and those beginning with Breiger (1981) and Nemeth and Smith (1985). The major differences between the two types are 1) the types of data used for their analyses—the former using both economic and non-economic

world economy. In short, a factor analysis revealed 5 “bundles” of goods that had similar flow patterns and were interpretable along a hierarchical processing continuum from food products to heavy manufacturers / high technology.

relations while that latter use multiple commodity types, and 2) how they address the mechanism of “unequal exchange” and mobility. The major similarities between the two types are 1) they follow the general SNA methodological approach of Roles and Positions; 2) they address categories 1, 2 and 3 in Table 2—testing for the presence / absence of a core / periphery structure, delineating boundaries between zones of the world-system, and trying to assess whether or not the structure of the world-system is best conceptualized as categorical or continuous; and 3) a general evolution away from structural equivalence toward less restrictive and more general definitions of equivalence such as regular and role equivalence (Kick and Davis 2001).

There are also a number of studies that utilize network analytic techniques to answer questions of interest to those studying world-systems issues, but do not necessarily follow the lineage we discuss above. Among this research are those that study the structure of the world-city system (Alderson and Beckfield 2004; Smith and Timberlake 2001; Taylor 2004)⁶, the formation of other structural properties in international trade such as trading blocks (Su and Clawson 1994; Blanton 1999b), regionalization (Kim and Shin 2002) and alternative conceptualizations of the structure of the world economy (Blanton 1999a; Kim and Shin 2002). While this is not an exhaustive list, it is suggestive of the many possibilities for using social network methodology to pursue questions that are important to world-systems research.

ANALYZING RELATIONAL DATA: A DEMONSTRATION OF CLASSIC AND RECENT APPROACHES

In the previous section, we outlined the intellectual lineage of social network analyses of the world-system that began with the classic work of Snyder and Kick (1979). A major motivation of this article, however, is to give the reader a basic sense of the analyses that are involved in this tradition in order to encourage future research. Space limitations preclude an exhaustive treatment of possible methodological applications to the study of the world-system, so we focus instead on the general approach taken in the classic lineage of the literature—the analysis of Roles and Positions, as well as introducing a recent variant—the Exponential Distance Model (EDM) that bears a relationship to some older techniques that have not made their way into world-systems research.

We begin with a brief introduction to *relational* data. Understanding relational data becomes easier by way of comparison to the type of data that is more commonly used in the social sciences—*attributinal* data. Relational data are, as the name would suggest, data that measure the presence, absence or strength of a tie on some relationship—be it trade, investment, military interventions, etc.—between at least two actors. Attribute data, on the other hand, is data that is collected at the level of one individual that captures their relative level of an attribute—GDP per capita, foreign investment stock, economic growth, income inequality, etc. Thus, where relational data captures a relationship between at least two actors, attribute data captures the characteristics of individuals.

⁶ Peter Taylor is associated with a fairly large community of scholars interested in questions about the world-city system. For an overview, see <http://www.lboro.ac.uk/gawc/>.

Because social network data is relational, social network analysis is almost uniformly interested in describing and / or predicting the latent structure of a set of relationships. Analysis of attribute data, on the other hand, is almost uniformly interested in describing and / or predicting the pattern of association between levels of attributional covariates. The theoretical underpinnings of most attributional approaches in the social science, however, tend to assume that attributional covariates reflect the outcome of an underlying structural dynamic—i.e. a high value in foreign capital penetration is an indicator of occupying a “dependent” position vis-à-vis some other country in the world-system. Indeed, much has been written as to the strengths of relational data vis-à-vis attribute data, but no where more eloquently than in White and Breiger (1975:68), where they state that, attribute data

measure select consequences of structural pattern (of the actual ties among individuals or organizations); they are useful indicators of questions to be asked by analyzing social structure directly, but they are neither descriptions nor analyses of the structure itself. (also in Breiger 1981:357)

This is not to say that relational data are intrinsically *better* than attribute data, only to note that if faced with a relational theory, i.e. that a country’s position in the world system is defined by their relationships to others, relational data and network analysis provide a direct research strategy with which to uncover the structure of relationships between actors (Chase-Dunn and Grimes 1995; Hopkins 1978; Wallerstein 1974).

As described above, the network analyses utilized in studying the world-system share a common analytical framework that is driven by a desire to understand the structure of the world-system, which makes it possible to understand how this structure does or does not impact many outcomes for individual nation-states within the structure. The general approach taken in the current social network analysis-world-systems theory literature is called *Network Positions and Roles* in the standard reference book for social network analysis (Wasserman and Faust 1994: 347-393; 461-502; also see Doreian et al. 2005). According to this text,

There are two key aspects to the positional and role analysis of social networks: identifying social positions as collections of actors who are similar in their ties with others, and modeling social roles as systems of ties between actors or between positions. (Wasserman and Faust 1994:351)

Practically, the research process involves 1) measuring the similarity between actors with a formal definition of equivalence (i.e. structural, regular, etc.), 2) grouping similar actors into mutually exclusive positions in such a way that inter-group similarity is minimized, and 3) understanding (modeling) the ways in which the various positions interact with each other to understand their various roles in the network.

We illustrate this procedure by providing a detailed position and role analysis of a new data set of world trade data. At each step in the process we will discuss choices that must be made before moving onto the next step, as well as compare and contrast the research strategy adopted by the papers reviewed earlier. We begin with a discussion of the data used for this analysis.

Trade Data

The primary dataset used for this study comes from the World Trade Analyzer (WTA) (Statistics Canada 2008). The data represent total trade between country pairs in 1980 (N = 164) and 2001 (N = 181), respectively. Countries included in the analyses are reported in Appendix A. Countries report their exports and imports to the United Nations using various commodity classification schemes and with varying levels of detail. Statistics Canada then organizes the data. They begin with reported trade as the base data, estimate missing values through mirror statistics, and, wherever possible, distribute highly aggregated regions or commodity categories to more detailed countries or categories. The end product is a non-symmetrical, square matrix for each year. While our analysis focuses on total trade, data are also available according to the classification scheme SITC rev. 2. Current versions of WTA contain data from 1985 to 2003. The version released in 2001 included data from 1980 to 1999; earlier versions also included data beginning with 1980.⁷

Role and Position Analysis

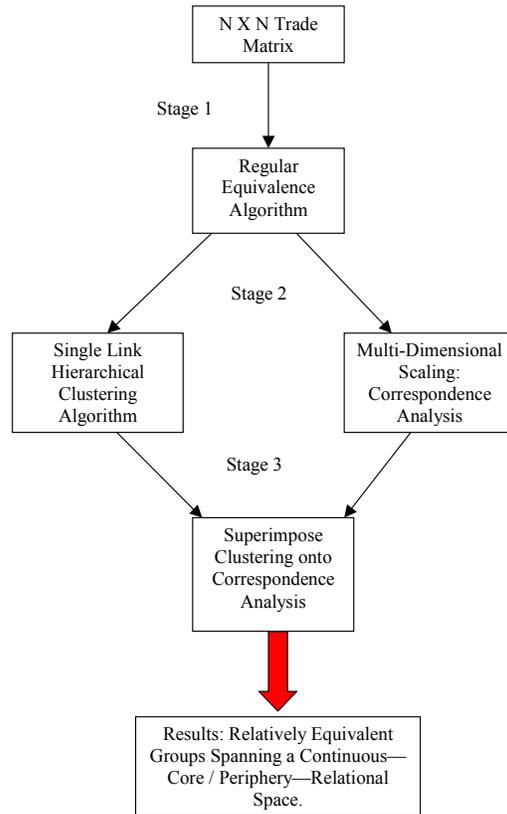
Figure 1 depicts the three stages in the role and position analysis we carry out here. In the first stage, a researcher chooses a measure of equivalence with which to gauge the level of similarity between each country based on their patterns of trade with others. The measure we use is regular equivalence, which is available in UCINET (see Borgatti, Everett, and Freeman 2002).⁸ We noted in the review above that structural equivalence, and particularly the CONCOR program, was the measure of similarity used in Snyder and Kick (1979), Breiger (1981), Nemeth and Smith (1985) and Kick and Davis (2001). CONCOR was a top of the line technology in 1979, but has since been shown to be a less optimal strategy than some other approaches because of the fact that it is an iterative program that by design produces an equal number of groups. CONCOR begins by splitting a data matrix into two groups, which are each then split into two more groups, and so on, until no further partitions are made. The resulting large and even number of groups is somewhat artificial (Wasserman and Faust 1994:375-381). A better approach to determine structural (rather than regular) equivalence might be to simply correlate the rows and columns of the data

⁷ The UN does not report data for Taiwan for political reasons (e.g. China's "One Nation" policy), and often the data for China includes Taiwan's information. The UN Comtrade database incorporates Taiwan as a trading partner by adding it to "Other Asia, not elsewhere specified" (code 490) which could in principle contain trade other than from Taiwan but is generally considered Taiwan. In general, using reporter data to represent Taiwanese trade reasonably matches the data actually reported by Taiwan (although never a perfect match). WTA is presumed to use the Comtrade data directly and to not supplement it with OECD data. Hong Kong re-exports are another problematic reporting area. These issues reflect a need for the ITRB to compile a sifted, documented and transparent set of estimates. Taiwan's reported data is therefore not included in Comtrade; data for Taiwan in the WTA is taken from other countries' reported trade with Taiwan (e.g., "mirror flows"). We thank Ronald Jansen, UN Statistics division, and Scudder Smith, WTA, for their help in answering the questions related to preparing the data for analysis.

⁸ The original trade matrices were transformed with the base 10 logarithm to reduce skew.

matrix, and then proceed with the rest of the steps outlined in figure 1 (Breiger 1981; Wasserman and Faust 1994:368-9).

Figure 1: Analytic Strategy for Role and Position Analyses



We begin this process by calculating the degree of regular equivalence for each pair of countries with the following algorithm (White and Reitz 1985). The regular equivalence (M_{ij}^{t+1}) between countries i and j at iteration $t+1$ is:

$$(1) \quad M_{ij}^{t+1} = \frac{\sum_{k=1}^g \max_{m=1}^g \sum_{r=1}^R M_{km}^t (M_{kjr}^t + M_{jir}^t)}{\sum_{k=1}^g \max_m^* \sum_{r=1}^R (M_{kjr}^t + M_{jir}^t)}$$

where the denominator is the maximum possible value of the matches between the profiles of ik and jm that would occur if all of the ties between i and its alters (k) were perfectly matched to the ties between j and its alters (m), and all of k and m were regularly equivalent. The numerator

determines the *best matching* of the ties between j and m for i 's ties to k weighted by the regular equivalence of k and m from the previous iteration (Wasserman and Faust, 1994). Thus, the algorithm determines the best possible matching of ties between i and j weighted by the equivalence of their alters, and divides that value by the maximum possible value of the numerator. It is important to remember that the equivalence of each pair of actors is revised after each iteration ($t + 1$). We have specified three iterations, with the third serving as the measure of regular equivalence for each pair of countries. It is highly unlikely that any two nations would be exactly equivalent, so we apply a regular equivalence algorithm to the matrices of trade data. This produces an equivalence matrix in which the ij cell equals the regular equivalence between i and j designating maximally dissimilar as (0) and regularly equivalent as (1).

Although the analysis conducted here is limited to one type of relationship—total trade—both the method proposed here and the general approach of social network roles and positions is easily generalized to multiple relations. These relations may be economic or non-economic relations as in Snyder and Kick (1979), disaggregated commodity trade used by Nemeth and Smith (1985), or any other type of relationship. In short, when multiple relations are desired, you simply create a “super matrix” by stacking each relationship on top of one another before applying the equivalence criterion. The resulting equivalence matrix would then represent the equivalence between each country after taking into account the similarity / dissimilarity across all the relationships (see Romney, Moore and Brazill 1998 for a discussion of stacking in the context of correspondence analysis).

Given the equivalence matrix we derive in stage one, stage two involves methods to reduce the dimensionality of the cross-country equivalencies, both categorically and continuously. The analysis here uses the matrix of regular equivalencies as input for both a single link hierarchical clustering routine and correspondence analysis for each year. Because the matrix of regular equivalencies gives a measure of equivalence between each pair of actors, the hierarchical clustering routine is well suited to finding “cut points” that minimize the between group variance in regular equivalence (or maximize the within group similarity). Hierarchical clustering starts by (1) putting each actor in an $N \times N$ matrix into its own cluster so that the similarity between clusters equals the similarity between each actor. The procedure then (2) finds the most similar pair of actors and merges them into one cluster. Next (3), we compute similarities between the new cluster and each of the other actors. The process (4) continues with the second and third steps until all actors have been merged into a single cluster of size N (Borgatti 1994).⁹ In principle, an analyst could start out with some α criterion whereby actors i

⁹ Single link hierarchical clustering is one choice among three common approaches to hierarchical clustering—single link, complete link and average link. The single link routine defines the similarity between each cluster as the greatest similarity from any member of one cluster to any member of the other cluster. The complete link routine defines the similarity between one cluster and another as the smallest similarity from any member of one cluster to any member of the other cluster. The average link routine defines the similarity between one cluster and another cluster to be equal to the average similarity from any member of one cluster to any member of the other cluster. Some research has shown that the complete link routine may be less subject to “chaining,” whereby a large group results from the trivial successive additions of a single actor, and some argue that the average approach is the least likely to produce trivial results (see Wasserman and Faust 1999: 381, Krackhardt 1999). As a practical strategy, it is worth

and j would be considered regularly equivalent if $RE_{ij} \geq \alpha$. There is no a-priori theory, however, that favors one level of alpha over another, and large real world data sets are rarely broken down into discrete homogenous groups at any single threshold level. Our approach is to use the hierarchical clustering results in conjunction with correspondence analysis to determine the boundaries of each equivalence group.

Correspondence Analysis is one of a family of techniques that draw on a common computational foundation: the Singular Value Decomposition (SVD), and is widely available in statistical packages such as UCINET, Stata and Statnet.¹⁰ At a conceptual level, correspondence analysis represents the basic structure in a set of data by decomposing a matrix into its three component parts: a matrix \mathbf{U} that summarizes the information in the rows; a matrix \mathbf{V} that summarizes the information in the columns, and a diagonal matrix of singular values \mathbf{d} that weights each \mathbf{UV} vector by its importance to the overall structure. Thus, the size of the singular values in \mathbf{d} that correspond to \mathbf{U} and \mathbf{V} indicates how much variation is explained by each dimension (Weller and Romney 1990). Correspondence analysis routines are widely available (UCINET, Stata, SPSS, and R, for example, all contain implementations of correspondence analysis). A classic correspondence analysis consists of four steps that we explain here, but which are automated in standard computer packages.

The first step generates matrix \mathbf{H} , in which the cells of the original matrix have been transformed so that the row / column marginals are approximately 1, with the following equation:

$$(2) \quad h_{ij} = f_{ij} / \sqrt{f_i \cdot f_{\cdot j}},$$

where h_{ij} is the transformed value in \mathbf{H} , f_{ij} is the original value in the ij^{th} cell of the regular equivalence matrix, f_i is the row marginal, and $f_{\cdot j}$ is the column marginal. This step removes the effect of the row / column totals before performing the second step of SVD.

The third step in a classic correspondence analysis rescales the information in \mathbf{U} and \mathbf{V} to obtain “optimal” or “canonical” scores by multiplying both \mathbf{U} and \mathbf{V} by the square root of the ratio of the total marginals to the row / column marginals, respectively:

$$(3) \quad X_i = U_i \sqrt{f_{\cdot \cdot} / f_i} \quad \text{and} \quad Y_j = V_j \sqrt{f_{\cdot \cdot} / f_{\cdot j}}$$

The final step incorporates the singular value “weights” so that each dimension of \mathbf{X} and \mathbf{Y} is multiplied by the square root of its respective singular value, such that the size of each dimension of \mathbf{X} and \mathbf{Y} corresponds to the amount of variance explained by each.

investigating all three approaches to see if the resulting clusters are “robust” across the three approaches.

¹⁰ All of the positional / role analyses (i.e. all but the exponential distance model) were carried out with UCINET. We use UCINET because it is very user friendly, and we can reasonably expect a shallow learning curve for the novice. Pajek is also recommended, and has an excellent graphing function (see De Nooy, Wouter, Mrvar, and Batagelj 2005). The SNA package for R has multiple functions and is very versatile but requires the user learn R. For an excellent overview, see <http://erzuli.ss.uci.edu/R.stuff>.

In sum, correspondence analysis begins by generating \mathbf{H} , which is a transformation of the original matrix (in this case, a matrix of regular equivalencies) so that the marginals (or expected values) are removed. It then performs a singular value decomposition on \mathbf{H} to produce three matrices, \mathbf{U} , \mathbf{V} and \mathbf{d} . As a third step, correspondence analysis rescales \mathbf{U} and \mathbf{V} with equation 3 to produce the \mathbf{X} and \mathbf{Y} matrices. Finally, correspondence weights each \mathbf{X} and \mathbf{Y} dimension by their associated singular values to produce a multidimensional representation of the similarity between actors (in this case country regular equivalencies) in which each orthogonal dimension is successively “less important” to the overall structure.¹¹

These results can be easily visualized by plotting successive dimensions of either \mathbf{X} or \mathbf{Y} , or \mathbf{X} and \mathbf{Y} . Thus, correspondence analysis allows us to represent actors in a multi-dimensional Euclidian space by assigning coordinates (weighted dimensions of \mathbf{X} and \mathbf{Y}) to actors that place them close to those with whom they are similar and far from those with whom they are dissimilar (Weller and Romney 1990). Because our matrix of regular equivalencies is symmetric, e.g., $\mathbf{X} = \mathbf{Y}$, we can simply plot dimensions from one or the other and the distance between each point in the graph corresponds to the dissimilarity between their equivalencies with the whole network. One can then evaluate the “fit” between single or multiple dimensions with the following equation:

$$(4) \quad 100 \times \frac{\lambda_m^2}{\sum_{m=1}^M \lambda_m^2},$$

where M is Singular Value 1, 2, 3, ... M . Interpreting the results from correspondence analysis depends on the amount of variation explained by each singular value/dimension and the observed spatial pattern of objects in the Euclidian space. Thus, one can have a relatively simple structure (few significant dimensions) or a complex one (many significant dimensions) and proximate actors in the Euclidian space have similar relational patterns.

The final stage of our Role and Position analysis brings the results of the two complementary procedures—hierarchical clustering and correspondence analysis—together to derive a set of positions to describe the structure. This stage can be broken down into three steps. In the first step, we examine the hierarchical clustering results in the form of a dendrogram to give a first approximation of the groups from that analysis. A dendrogram is a visualization of the hierarchical clustering process described above, in which each step in the process is represented by the fusion of two or more actors into the same cluster. Dendrograms typically display each actor separately at the bottom, and the range of the equivalence criterion on a vertical axis where the values range from high to low as you read from bottom to top, terminating at the top when all actors are merged into a single group. Clearly, the bottom most clustering (every actor in a separate group) and the top most clustering (every actor in the same group) are trivial and uninteresting. Thus, “the ‘trick’ is to choose the point along the series that gives a useful and interpretable partition of actors into groups (Wasserman and Faust 1994: 383).

The second step displays the first and second dimensions of the CA results in the form of a scatterplot, and superimposes the first approximation of the HC results on top so that actors in the same group are the same color and / or shape. The final step of this procedure seeks

¹¹ The reader should note that the first dimension of \mathbf{U} and \mathbf{V} , and the first singular value in \mathbf{d} are considered trivial since they will always equal 1, by construction (see equation 2 above).

consistency between the correspondence analysis results and the hierarchical clustering results. The consistency should be high if the HC results are meaningful, and the final stage should consist of nothing more than minor changes in group assignments at the group boundaries depicted in the second stage. Because the hierarchical clustering procedure can be derived in several ways and therefore vary, while the CA results are always consistent, we rely on the blocks for which the CA analysis is most consistent.

In the next section, we apply recent advances in correspondence analysis (CA), referred to as the Exponential Distance Model (EDM), to the same data set. CA has many variants and derivations. It can be used both as an exploratory technique and as a method of fitting a statistical model. This approach corresponds to multiple correspondence analysis (MCA) or attribute seriation (LeBlanc, 1975; Duff 1996) and is incorporated in the R package homals (De Leeuw and Mair 2008). EDM represents the maximum likelihood fitting as opposed to the least squares fitting of CA models. The EDM is a data reduction and data representation method which shifts CA from multivariate exploration to model testing.

Exponential Distance Model:

This analysis also begins with a square matrix F , where the rows and columns of the table correspond with the number of countries in the study. Cell f_{ij} of the table indicates how much country i exports to country j , or, equivalently, how much country j imports from country i . The diagonal of the table usually consists of missing data, because countries do not import from or export to themselves. Thus, using terminology from Haberman (1974) and Bishop et al. (1975), the diagonal of the table has structural zeroes. The next step is to create our model. We start with the assumption that the f_{ij} are realizations of independent Poisson variables \underline{f}_{ij} , with $E(\underline{f}_{ij}) = \lambda_{ij}$. It is well known that by conditioning on the row marginals this model also covers the product multinomial model, in which rows are independent multinomials. The negative log likelihood for the Poisson model is

$$(5) \quad \Delta = \sum \sum \{ \lambda_{ij} - f_{ij} \log \lambda_{ij} \mid i \neq j \}$$

The assumption of independent Poisson cells is made for convenience, for the same reasons the assumption of normality is made in continuous multivariate analysis. Alternatively, one can simply think of (5) as a natural way to measure the distance between the observed frequencies f_{ij} and the expected frequencies λ_{ij} .

Base Models:

The two key specifications that we shall elaborate on in this paper are the quasi-independence model and the quasi-symmetry model. The quasi-independence model says that

$$(6a) \quad \lambda_{ij} = \alpha_i \beta_j \quad \forall i \neq j$$

where α_i is a row (export) parameter and β_j is a column (import) parameter.

The quasi-symmetry model says

$$(6b) \quad \lambda_{ij} = \alpha_i \beta_j \eta_{ij} \quad \forall i \neq j$$

where α_i and β_j are the same as above, and $\eta_{ij} = \eta_{ji}$. The η_{ji} are called *similarities*. Clearly quasi-independence is the special case of quasi-symmetry in which all similarities are equal. In the quasi-independence model, each country has an *export effect* α_i and an *import effect* β_j , and the amount of trade between countries is just determined by these export and import values.

In the quasi-symmetry model the trade is determined by both export and import values and the similarity. Both the quasi-symmetric and the quasi-independence model are base models, in the sense that we do not expect them to be even approximately true but we can use them as baselines with which to compare our hopefully more realistic models.

Geometric Models:

We can restrict the quasi-symmetry models further by requiring that the similarities are inversely related to distances on an unknown map. In particular we assume the *quadratic Euclidean model*:

$$(7a) \quad \eta_{ij} = \exp\left\{-\sum_{s=1}^p (x_{is} - x_{js})^2\right\}$$

The problem is now to recover the map, along with the import and export values of the countries. Alternatively, our software can also fit the *simple Euclidean model*:

$$(7b) \quad \eta_{ij} = \exp\left\{-\sqrt{\sum_{s=1}^p (x_{is} - x_{js})^2}\right\}$$

but for various reasons we will initially concentrate on the quadratic case in this paper. Geometric models of the form (7a) or (7b) have been proposed many times, and in many different contexts, in econometrics, psychometrics, and sociometrics.

Correspondence Analysis Approximation:

Let us look more closely at the quadratic Euclidean model. By expanding the squared distance we have

$$\eta_{ij} = \exp\left\{-\sum_{s=1}^p x_{is}^2\right\} \exp\left\{-\sum_{s=1}^p x_{js}^2\right\} \exp\left\{+2\sum_{s=1}^p x_{is}x_{js}\right\}$$

If we define

$$\bar{\alpha}_i = \alpha_i \exp\left\{-\sum_{s=1}^p x_{is}^2\right\},$$

$$\bar{\beta}_j = \beta_j \exp\left\{-\sum_{s=1}^p x_{js}^2\right\},$$

and $\bar{x}_{is} = \sqrt{2}x_{is}$ then for the squared Euclidean model

$$\lambda_{ij} = \mu\alpha_i\beta_j \exp\left\{-\sum_{s=1}^p (x_{is} - x_{js})^2\right\} = \mu\bar{\alpha}_i\bar{\beta}_j \exp\left\{\sum_{s=1}^p \bar{x}_{is}\bar{x}_{js}\right\}$$

which says that the squared Euclidean model is equivalent to the *inner product model*. Instead of fitting exponents of negative squared distances, we could also fit exponents of inner products, and obtain basically the same results (with an exactly equal goodness-of-fit. For the next step in the approximation, observe that if z is small, then $\exp(z) \approx 1 + z$. Thus if the inner products are small, then

$$\lambda_{ij} \approx \mu \bar{\alpha}_i \bar{\beta}_j \left\{ 1 + \sum_{s=1}^p \bar{x}_{is} \bar{x}_{js} \right\}$$

and this is the model used in the symmetric version of Correspondence Analysis (if you interpret Correspondence Analysis as a model fitting technique). In ordinary Correspondence Analysis one computes separate maps for rows and columns, which means that the squared Euclidean distance model is approximated by a Correspondence Analysis model with row and column scores equal. These approximations are also discussed in detail by Goodman (1991).

Note, that in both CA and EDM, we suppose that the frequency of interaction between row actors i and column actor j is a function of marginal effects and degree of similarity or degree of attraction between the actors. In other words, we draw a map of the actors such that distance in the map translates inversely to the degree of similarity--proximate actors are similar, and distant actors are dissimilar. In CA we use least squares techniques to fit the model, and in EDM we use maximum likelihood techniques, which are guaranteed to produce optimal estimates of the model.

Fitting:

Fitting the model means maximizing the Poisson likelihood. We have constructed convergent iterative algorithms, with corresponding computer implementations in the R programming language, based on the majorization principle (e.g. De Leeuw 1994). We shall not give the details of the algorithm here, but it amounts to solving a sequence of multidimensional scaling problems on transformed data.¹²

RESULTS

Results from Role and Position Analysis

The common question asked across the studies reviewed in Table 2 is does the network conform to a core / periphery structure. All of the studies we reviewed found this to be the case. Therefore, we expect that networks of trade will conform to a core / periphery structure in the present analysis. Figures 2 (1980) and 3 (2001) depict the first and second dimension from the correspondence analysis of regular equivalence, with the results of our hierarchical clustering routine superimposed on top. Due to the high number of actors, we do not include labels in these graphs, but tables A1a through A2b in the Appendix provide the information on the position of each country. The origin of the Euclidean space (the point on the graph where the x and y axis are 0) from our correspondence analysis reflects the average regular equivalence profile in the network.

¹² Code is available from the authors.

Figure 2: Correspondence Analysis of Regular Equivalence with Hierarchical Clustering Superimposed, 1980

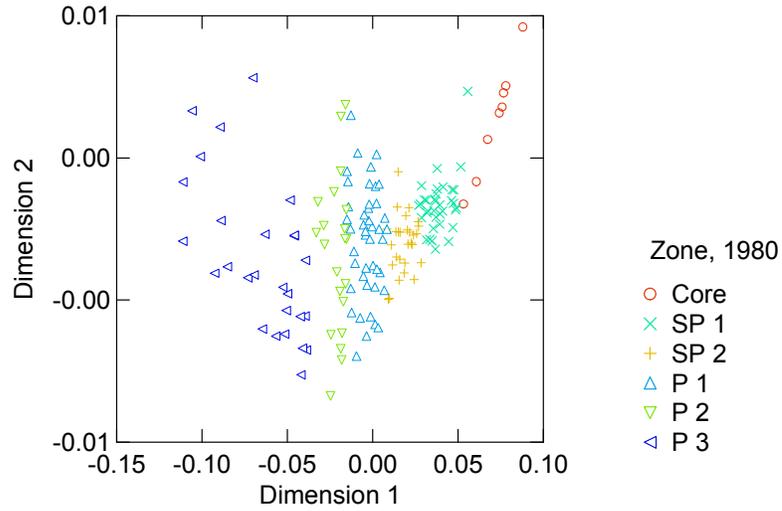
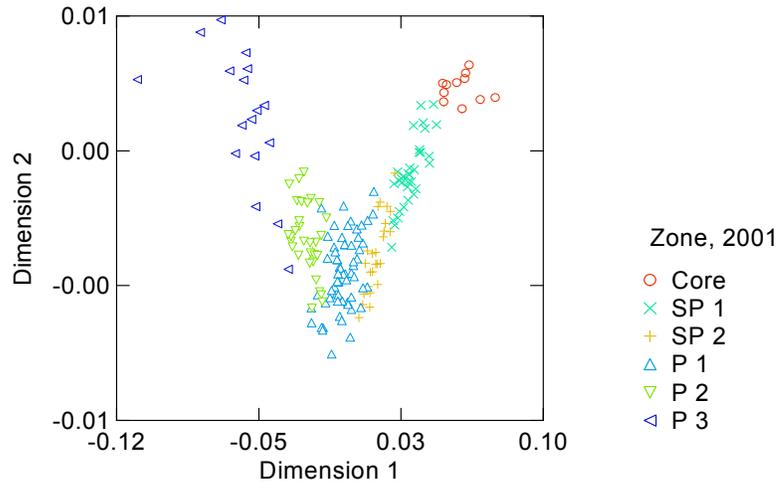


Figure 3: Correspondence Analysis of Regular Equivalence with Hierarchical Clustering Superimposed, 2001



In both Figures, the countries on the positive (right hand) side of the origin are more “core like” than those on the negative side of the origin. The most extreme positive group appears

to be the core. There are two groups between the core and the origin that we've labeled (2) Semi-periphery 1 and (3) Semi Periphery 2. Our fourth group—Periphery 1—straddles the origin in each year, and the three lowest groups—(5) Periphery 2 and (6) Periphery 3—correspond to an increasing negative distance from the origin. To summarize, we found six roughly equivalent positions in our data that we labeled Core, Semi-Peripheries 1 and 2, and Peripheries 1 – 3. Semi-periphery 2 is less equivalent to the core than semi-periphery 1; periphery 2 is less equivalent to the core than periphery 1, etc., such that these positions capture the extent to which each position is successively less “core-like” as you move from right to left in Figures 2 and 3.

In order to verify that the first dimension is a continuous measure of “coreness,” we can examine the reduced image matrix produced by collapsing the N x N trade matrices into a 6 x 6 matrix representing the six regularly equivalent positions. Recall that these positions simply represent “cut-points” along the continuous “coreness” dimension depicted in Figures 2 and 3. Thus, if the first dimension is a continuous measure of “coreness,” one would expect to observe an interaction pattern reminiscent of the ideal typical one portrayed in Table 1 when examining the flows between groups. In other words, one would expect that this dimension captures the extent to which these data conform to a core / periphery structure.

Table 3: Values Represent Average Trade Within and Between Blocks in Thousands of US Dollars

1980	Core	Semi-P 1	Semi-P 2	Periphery 1	Periphery 2	Periphery 3
Core	8,706,935	1,409,369	340,504	84,988	37,914	7,894
Semi-P 1	1,663,029	203,398	62,524	11,959	4,372	1,028
Semi-P 2	298,919	32,734	12,669	3,384	1,783	783
Periphery 1	80,192	6,394	2,292	1,133	366	151
Periphery 2	21,013	2,231	628	123	123	75
Periphery 3	2,791	212	107	26	53	1

Table 4: Values Represent Average Trade Within and Between Blocks in Thousands of US Dollars

2001	Core	Semi-P 1	Semi-P 2	Periphery 1	Periphery 2	Periphery 3
Core	10,272,420	1,367,748	305,078	108,559	25,059	6,156
Semi-P 1	1,553,314	353,354	107,583	33,481	12,437	1,498
Semi-P 2	383,887	88,335	21,573	8,545	5,087	132
Periphery 1	79,924	17,413	3,246	2,970	1,044	193
Periphery 2	9,969	3,211	649	447	421	208
Periphery 3	1,671	320	39	59	121	12

The cells in tables 3 (1980) and 4 (2001) represent the average trade within and between each position in our analysis. The diagonal cells represent within position trade, while the off diagonal cells represent between position trade. The tables reveal a classic core / periphery interaction pattern: the largest cell represents the trade within the core, the peripheral groups are much more dependent upon the core than the other way around (comparing the core to periphery cells with the periphery to core cells), the periphery has only very minor interaction with itself,

and the semi-periphery has interaction patterns that are at once more “core like” than the periphery, but less “core like” than the core.

Table 5: Explained Variance of Regular Equivalence Matrix with First Five Dimensions of Correspondence Analysis

	1980	2001
Dimension 1		
Singular Value	0.041	0.031
PRE	97.18	97.30
Dimension 2		
Singular Value	0.004	0.004
PRE	0.89	1.36
Dimension 3		
Singular Value	0.004	0.002
PRE	0.86	0.54
Dimension 4		
Singular Value	0.003	0.002
PRE	0.40	0.35
Dimension 5		
Singular Value	0.002	0.001
PRE	0.29	0.13

As the above description suggests, the first—horizontal—dimension is consistent with a continuous measure of “coreness,” where actors on the right are more “core like” than actors on the left. Thus, an obvious question to ask with this kind of information is how much of the variation in regular equivalencies can you explain with this simple one-dimensional core / periphery solution? Table 5 suggests that the variance explained by the first dimension—as shown in equation (4) above, is substantial, ranging from 97.18 % of the variance in 1980 to 97.30 % of the variance in 2001. In short, Figures 2 (1980) and 3 (2001), coupled with Tables 3 and 4, reveal that the data do in fact correspond to a core / periphery structure, and Table 3 suggests that the fit of this model was fairly constant over the 21 year period under investigation.

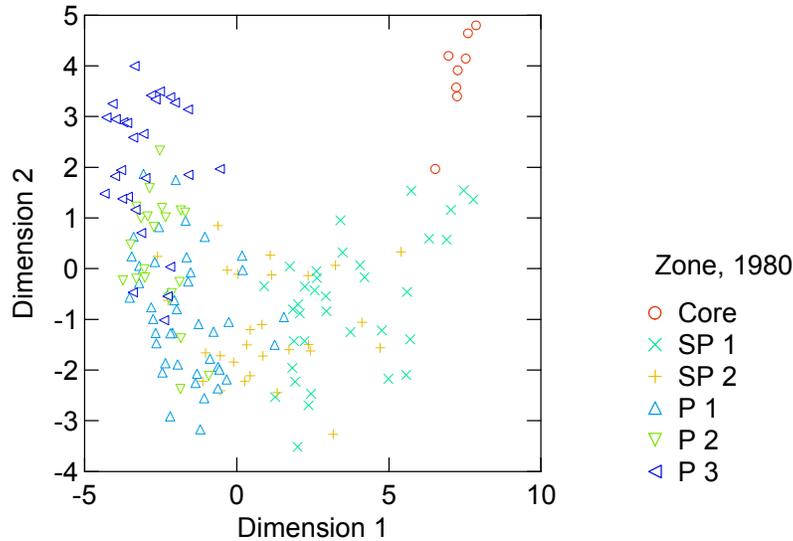
In sum, our results suggest that the structure of global trade at both time points—1981 and 2001—exhibits the expected core / periphery interaction pattern, that there is a relatively distinct boundary between the core, periphery and semi-periphery, and that these conclusions are fairly stable across both time points studied. The results also suggest that, at least with respect to the data studied here, the core / periphery distinction is more continuous than discrete. This follows from the continuous break down of groups along the one dimensional correspondence analysis scaling, and the high amount of explained variance accounted for by the continuous first dimension of the correspondence analysis. More importantly, the results are consistent with those from previous trade research which found that cross-national relational data tends to exhibit a core / periphery interaction pattern. The implication is that this may be a fundamental feature to such data. The generality of this claim can be furthered if our finding can be replicated by an

analysis that is distinct from the one performed above. We pursue this in our next section where we discuss the results of the exponential distance model, another structural approach to analyzing world trade data.

EDM Results

We begin our explanation of the results by returning to Figure 1 to differentiate this analysis from the Role and Position Analysis above. Step 1 in the first analysis involved taking the raw trade data and analyzing it with an equivalence criterion, e.g., regular equivalence (RE) in this case. Steps 2 and 3, the categorical and continuous scaling of the equivalence matrix, are carried out on the RE matrix as opposed to the raw trade matrix. Therefore, the underlying data applied to the CA above is an equivalence rather than a raw trade value. In other words, cells ij in an equivalence matrix quantifies how similar actor i 's overall trade pattern are to actor j 's overall trade pattern. In the EDM analysis, we skipped step 1 and submitted the raw trade data to the EDM. Thus the data analyzed by the EDM is frequency rather than similarity data. In other words, cell ij in the raw trade matrix quantifies the volume of trade between actors ij rather than the similarity in overall trade patterns between i and j .

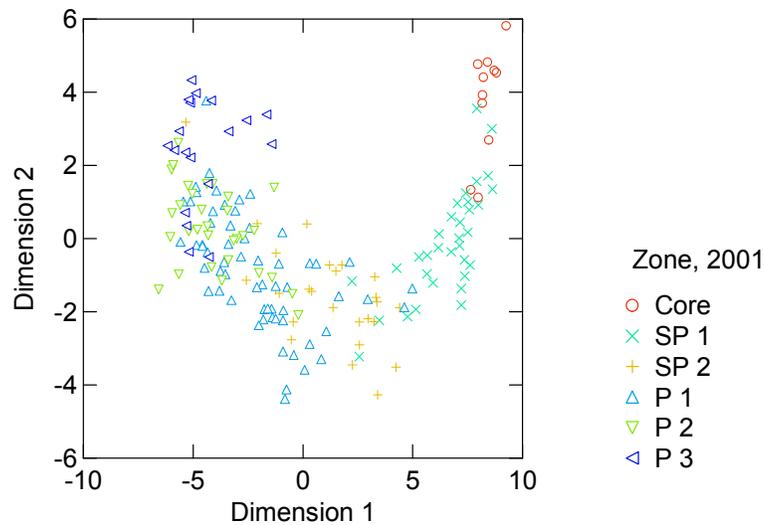
Figure 4: Exponential Distance Model with Hierarchical Clustering of Regular Equivalencies Superimposed, 1980



Figures 4 and 5 represent the first and second dimensions from the EDM analysis with the groups from the previous analysis superimposed. Similar to Figures 2 and 3, the distance between the points in the EDM graphs represent dissimilarity in patterns of trade. Figure 4 shows the results for the EDM for 1980. There is a clear cluster of core countries in the upper right hand

corner of the figure.¹³ Countries that were identified as members of semi-peripheries 1 and 2 in the Role and Positional Analysis are located in closest proximity to the core, followed by countries which were identified as part of the periphery (P1-P3 in the prior analysis). Figure 5, the graph for 2001, is very similar to that of the 1980 graph, suggesting very little change in global trade patterns in the post-Cold War era despite predictions to the contrary. What has changed of course is the composition of the groups as many countries (most notably China) have experienced mobility.

Figure 5: Exponential Distance Model with Hierarchical Clustering of Regular Equivalencies Superimposed, 2001



Comparing the Two Models

The correspondence between the results of these two very different analyses is striking. First, Figures 2 and 3 depicted a single coreness vector along the horizontal axis, and the superimposed groups gave a sense of the extent to which countries could be placed into relatively equivalent groups along the continuous core / periphery dimension. By looking at the correspondence between the groups from the first analysis and the first dimension from the EDM analysis, it becomes clear that the first dimension from the latter analysis is also capturing a continuous measure of “coreness.” The “core” groups are located in the upper right quadrant in both sets of graphs, and the groupings are increasingly “peripheral” as you move from right to left. In order to quantify the similarity, we correlated these two dimensions and report these values in Table 6. There was a correlation of .739 in 1980 and .872 in 2001, indicating high similarity in the underlying structure identified by these two methods.

¹³ See Table A1a through A2b in the Appendix for the full country names and EDM coordinates.

Table 6: Correlation Coefficients Between First Dimensions of the Exponential Distance Model and Correspondence Analysis of Regular Equivalencies

	CA of RE	
	Year	<i>r</i>
EDM	1980	0.739
	2001	0.872

CONCLUSION

This paper reviewed two lineages of empirical research on the world-system that utilize social network methods. Despite the fact that each lineage approaches their analyses with very different sources of data—either both economic and non-economic data or multiple commodity trade relations—our review highlighted much similarity across the two approaches in terms of the kinds of questions that are asked of the data, as well as the findings. In particular, they share a common approach to the identification of Roles and Positions from SNA and an evolution toward less restrictive equivalence criterion over time. Further, there is overwhelming support for the notion that cross-national trade data exhibit a core / periphery interaction pattern.

Our analyses of the trade data drew attention to various analytical decisions that need to be made including how to conduct a simultaneous analysis of multiple relations, the best way to choose between and implement different equivalence criterion, how to select and interpret clustering algorithms, and a practical approach to generating a consistent set of equivalence groupings. These analyses should provide a reasonable road map to future researchers interested in applying these methods or developing new ones, either in the tradition of the classic approach to network roles and positions or the tradition of reduced rank techniques that work well on raw frequency data such as the EDM model (e.g. Borgatti and Everett 1999; Boyd et al. 2006a; 2006b; De Leeuw and Mair, 2008; Handcock, Raftery and Tantrum 2007; Weller and Romney 1990).

Our results generally support the lineage of research dating back to 1979 across two different methodological approaches. Our findings suggest that the main dimension of cross-national variation in trade can be interpreted as a continuous core / periphery dimension, and the association between relative positions of countries along this core / periphery dimension across the two methods is high and growing over time. The implication is that a core / periphery interaction pattern appears to be a fundamental feature of cross-national trade data. Yet neither we, nor the papers we reviewed above, establish a “null hypothesis” to test whether or not a network conforms to a core / periphery structure. In other words, many networks may exhibit varying degrees of a core / periphery structure. It is not clear whether or not cross-national networks like those analyzed here are more like ideal core / periphery structures than one might observe on any randomly selected group of networks with similar characteristics (such as size, density, degree distribution, etc.).

While it is beyond the scope of this article to address the various statistical particularities of network data, it should suffice to say that relational data do not meet standard statistical

assumptions such as random sampling and independent units. Early statistical analyses focused on non-parametric approaches such as the quadratic assignment procedure (QAP). For example, a QAP could be used to “test” the fit of a core / periphery model as follows. Step one would involve computing a correlation coefficient between a derived block model such as illustrated in Tables 4 and 5, and an ideal-typical model similar to the one illustrated in Table 1. The next step would involve multiple iterations of random permutations of the derived block model. The last step would involve computing a new correlation coefficient at each permutation in order to determine the frequency of random correlations as large as the one observed, using standard thresholds as benchmarks. An alternative strategy for continuous core / periphery structures would simply be to correlate the matrix that results from the product of the derived “coreness” vector and its transpose (e.g., the product cc^T where c is a derived measure of “coreness”) with the observed matrix, and then continue through steps 2 and 3 by permuting the cc^T product matrix an appropriate number of times.¹⁴ Other non-parametric approaches, such as the jackknife and bootstrap, may be equally useful as a first approximation in either the categorical or continuous cases (e.g. Snijders and Borgatti 1999).

Another potential methodological advance for this literature may involve using the structural approach to predict the presence, absence or volume of some type of relationship between two actors as a function of their position in the global economic network. Early approaches such as QAP regression may work as a first approximation, but they rest on the somewhat dubious assumption of dyadic independence (see Alderson and Beckfield 2004 for a recent application). Recent advances in statistical network models including the exponential random graph (ERG) model (Anderson, Wasserman and Crouch 1999; Contractor, Wasserman and Faust 2006; Holland and Leinhardt 1975; 1981, Robins and Morris 2007) or the stochastic block model (see Wasserman and Faust 1994: 675-723 for a general introduction; Nowicki and Snijders 2001; Snijders and Nowicki 1997; Wang and Wong 1987) may take us in the right direction.

ERG models relax the assumption of dyadic independence, and provide a useful (but complicated) way to conceptualize a set of ties between actors as random variables that arise as a function of the interdependencies among the set of ties. The ERG family of models are fairly young in their development, however, and deciding upon a set of parameters can seem somewhat arbitrary (e.g. Goodreau 2007). Further, the models proposed in the literature generally focus on modeling individual level data such as friendship, such that their extension to cross-national data may not be obvious. Stochastic block models, on the other hand, may be useful in several ways. First, they may provide the means to engage in hypothesis testing with respect to the correct assignment of actors to subgroups (steps 2 and 3 in Figure 1 above), and whether or not the pattern of relations within and between subgroups bears a higher degree of association to an ideal-typical block model than one would expect by chance.¹⁵ Second, they may provide the means to determine how well a core / periphery model explains a set of cross-national relational

¹⁴ Recent work suggests that replacing the product cc^T with the product of the first UV from an SVD with imputed diagonal elements may make more sense in the presence of strong asymmetry (Boyd et al. 2006b).

¹⁵ Also see Handcock, Raftery, and Tantrum 2007 for a very recent model based clustering approach.

data compared to some other theoretically derived ideal-typical model. Both approaches suggest that future research should consider incorporating model based statistical network methods.

Another useful line of inquiry could be using the results of structural analyses as both dependent and independent variables in regression analyses. For example, scholars disagree on how the structure of the world economy is impacted by globalization processes. Future studies can further explore both the determinants of a country's position in the structure of the world-system, and the consequences of occupying a given position. The EDM model and other similar approaches may be useful for modeling the *determinants* of world-system position.

As we have seen, the EDM model for independent Poisson frequencies in a square table has expected values of the form

$$(8) \quad \lambda_{ij} = \exp\{\alpha_i + \beta_j - |x_i - x_j|\}$$

where the $|x_i - x_j|$ are Euclidean distances between points in "latent space". The software for the EDM method can also handle versions of the model where distances are replaced by either squared distances or inner products.

EDM is very similar to the model implemented in the latentnet package for social network analysis (Hoff et al. 2002, Shortreed et al. 2006, Krivitsly and Handcock 2008), although the algorithms used are completely different. The latentnet model in the Poisson case is

$$(9) \quad (\lambda_{ij} = \exp\{\sum_{p=1}^s y_{ijp} \beta_p - |x_i - x_j|\})$$

Thus latentnet is similar to EDM, because it allows for a geometrical representation of interaction in latent space, using the points x_i . In addition it incorporates regression on one or more external variables. In future versions of the EDM method we intend to implement similar linear restrictions, in addition to various linear restrictions on the coordinates in latent space. In short, either the procedure implemented in latentnet or our own future version will be amenable to including both attributes and relational data on the right hand side of an equation designed to understand what factors determine the placement of countries in the structure of the world-system.

Some of the classic questions addressed thus far in terms of the consequences for occupying a given structural position include economic growth (e.g. Snyder and Kick 1979; Nemeth and Smith 1985), within country inequality (Alderson and Nielsen 1999; Nemeth and Smith 1985), between-country inequality (Peacock, Hoover and Killian 1988) and other indicators of development. While some of the attribute-based quantitative efforts to pursue hypotheses related to world-systems analysis—such as the foreign capital penetration literatures (Firebaugh 1992; c.f. Dixon and Boswell 1996) or studies of global income inequality (Korzeniewicz and Moran 2000, 1997; c.f. Firebaugh 1999, 2003)—were met with critique on either empirical or substantive grounds, the relational approach of social network analysis has fared much better (c.f. Chase-Dunn and Grimes 1995: 398; Van Rossem 1997). Thus, new studies on the question of the developmental consequences for structural position may provide a means by which to revitalize the position of world-systems analysis in the social sciences.

Given that the world-system perspective gives causal priority to the position of countries in the structure of the world-system, one particularly important question could be that of mobility. Surprisingly, a small amount of research has been done on the question of upward / downward mobility in the world-system (Bornschieer and Trezzini 1997) from either a network analytic approach (c.f. Mahutga 2006; Smith and White 1992) or other approaches (c.f. Arrighi and Drangle 1986; Babones 2005; Terlouw 1993). While much of the explanation for this empirical gap may be found in the presumed stability of the world-system, we suggest that studying the question of mobility is an important direction for future research, and is already underway in some cases (e.g., Clark 2008). Indeed, understanding both the determinants of and consequences for mobility in the world-system could contribute to classic questions of interest to world-system theorists, including a mainstream explanation for development and underdevelopment in the world-economy and the rise and fall of hegemonic powers.

Finally, the increasing interdependence of nation-states beyond economic relations suggests additional avenues for research to examine hypotheses derived from world systems analysis. New substantive foci include world-polity embeddedness (Beckfield 2003) and human rights and geo-political alignments (Lloyd 2007).

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Appendix A: Countries Included in the Analysis, Position and Coordinates for the CA of RE and EDM models¹⁶

Table A1a: Core and Semi-Periphery in 1980, ranked by position and CA of RE

Country	Position	CA of RE	EDM	Country	Position	CA of RE	EDM
		1st Dim	1st Dim			1st Dim	1st Dim
United States	1	0.088	6.966	Argentina	2	0.031	2.003
Germany	1	0.078	7.873	South Africa	2	0.031	1.885
Japan	1	0.077	7.268	Malaysia	2	0.030	2.361
United Kingdom	1	0.076	7.223	Indonesia	2	0.029	1.833
France	1	0.074	7.532	Hungary	2	0.029	2.074
Italy	1	0.067	7.612	Algeria	2	0.028	2.018
Netherlands	1	0.061	7.251	Mexico	2	0.027	2.450
Belgium-Luxembourg	1	0.053	6.532	Romania	3	0.028	1.110
Saudi Arabia	2	0.056	5.601	Greece	3	0.027	5.402
Former USSR	2	0.052	3.485	Thailand	3	0.027	2.426
Brazil	2	0.049	2.620	Former E Germany	3	0.026	1.152
Singapore	2	0.048	3.740	Turkey	3	0.024	0.446
Iraq	2	0.048	1.846	Israel	3	0.024	1.727
Canada	2	0.047	7.042	Chile	3	0.023	-0.098
Spain	2	0.047	5.737	Netherlands Antilles	3	0.023	-2.258
Sweden	2	0.047	7.779	New Zealand	3	0.022	3.171
Switzerland	2	0.046	7.458	Philippines	3	0.021	1.335
China	2	0.044	2.636	Portugal	3	0.021	4.119
Australia	2	0.041	4.984	Ireland	3	0.020	4.718
Poland	2	0.041	2.563	Colombia	3	0.019	0.859
Iran	2	0.039	2.238	Egypt	3	0.019	2.358
Taiwan	2	0.039	4.200	Pakistan	3	0.018	3.244
Hong Kong	2	0.039	5.703	Peru	3	0.016	0.265
Venezuela	2	0.038	1.267	Qatar	3	0.016	-1.018
Nigeria	2	0.038	2.952	Bulgaria	3	0.015	0.050
Libya	2	0.037	0.904	Cote d'Ivoire	3	0.015	-0.306
United Arab Emirates	2	0.037	2.232	Bahamas	3	0.015	-2.604
Austria	2	0.037	6.323	Trinidad & Tobago	3	0.014	-1.110
Fmr. Czechoslovakia	2	0.037	1.747	Bahrain	3	0.014	-0.534
South Korea	2	0.036	2.928	Morocco	3	0.014	2.354
Kuwait	2	0.036	1.921	Syria	3	0.012	0.835
Denmark	2	0.035	6.899	Ecuador	3	0.011	-0.537

¹⁶ N = 164 for 1980; N = 181 for 2001

Table A1a continued

Former Yugoslavia	2	0.035	3.412	Cuba	3	0.010	-0.616
India	2	0.033	4.054	Kenya	3	0.009	0.447
Finland	2	0.032	4.769	Tunisia	3	0.008	0.333
Norway	2	0.032	5.571				

Table A1b: Periphery in 1980, ranked by position and CA of RE

Country	Position	CA of RE	EDM	Country	Position	CA of RE	EDM
		1st Dim	1st Dim			1st Dim	1st Dim
Guatemala	4	0.008	-2.050	Malawi	5	-0.016	-2.454
Oman	4	0.007	-0.879	Fiji	5	-0.016	-3.293
Angola	4	0.007	-1.043	Mali	5	-0.016	-2.722
Lebanon	4	0.006	1.555	Uganda	5	-0.017	-3.481
Uruguay	4	0.004	-1.349	Albania	5	-0.018	-2.522
Costa Rica	4	0.004	-1.938	Vietnam	5	-0.018	-1.690
Zaire	4	0.004	-1.595	Reunion	5	-0.019	-2.136
Sri Lanka	4	0.003	0.179	Barbados	5	-0.019	-2.226
Zambia	4	0.002	-1.647	Zimbabwe	5	-0.019	-2.927
Brunei	4	0.002	-3.451	Guinea	5	-0.019	-2.337
Cameroon	4	0.002	-0.611	Mauritania	5	-0.021	-3.141
Gabon	4	0.002	-2.442	Sierra Leone	5	-0.023	-3.015
Bangladesh	4	0.001	0.198	Afghanistan	5	-0.024	-3.290
Panama	4	0.001	-0.759	Yemen	5	-0.025	-0.917
Jordan	4	0.000	1.250	Burkina Faso	5	-0.028	-3.034
Honduras	4	0.000	-2.342	Somalia	5	-0.029	-1.835
Jamaica	4	-0.001	-2.744	Belize	5	-0.032	-2.856
Sudan	4	-0.001	-0.615	Central African Rep.	5	-0.033	-3.734
Paraguay	4	-0.002	-2.175	Benin	6	-0.039	-2.383
Dominican Republic	4	-0.002	-2.107	Kiribati	6	-0.040	-4.346
El Salvador	4	-0.002	-3.205	Burundi	6	-0.040	-3.995
Papua New Guinea	4	-0.002	-3.062	Rwanda	6	-0.041	-3.761
Bolivia	4	-0.002	-1.512	Nepal	6	-0.042	-3.134
Iceland	4	-0.003	-2.185	Guinea-Bissau	6	-0.043	-3.582
Tanzania	4	-0.004	-0.259	Mongolia	6	-0.046	-4.081
Liberia	4	-0.004	-0.332	Djibouti	6	-0.046	-2.274
Ghana	4	-0.004	-1.067	French Guiana	6	-0.048	-3.416
North Korea	4	-0.004	-2.002	Gibraltar	6	-0.050	-3.332
Senegal	4	-0.005	-1.302	Bermuda	6	-0.051	-2.191

Table A1b continued

Nicaragua	4	-0.005	-3.519	Solomon Islands	6	-0.052	-3.976
Cyprus	4	-0.006	-0.567	Chad	6	-0.053	-2.815
Myanmar	4	-0.007	-2.552	Gambia	6	-0.057	-2.999
Niger	4	-0.009	-2.806	Equatorial Guinea	6	-0.063	-2.669
Mozambique	4	-0.009	-1.680	Laos	6	-0.065	-3.597
Madagascar	4	-0.010	-2.229	Comoros	6	-0.070	-3.725
Suriname	4	-0.011	-3.098	Bhutan	6	-0.070	-3.061
Ethiopia	4	-0.012	-1.250	Cambodia	6	-0.073	-3.401
New Caledonia	4	-0.013	-2.640	Seychelles	6	-0.085	-3.799
Togo	4	-0.013	-1.967	St Pierre & Miquelon	6	-0.089	-2.522
Guyana	4	-0.013	-3.386	Falkland Islands	6	-0.090	-3.358
Malta	4	-0.014	-1.197	Maldives	6	-0.093	-4.279
Saint Kitts and Nevis	4	-0.015	-3.200	Western Sahara	6	-0.101	-0.556
Haiti	4	-0.015	-2.690	Turks & Caicos Is.	6	-0.106	-2.030
Mauritius	4	-0.015	-2.663	St Helena	6	-0.111	-2.182
Congo	5	-0.015	-1.874	Cayman Islands	6	-0.111	-1.593
Greenland	5	-0.015	-1.838	Br. Indian Ocean Ter	6	-0.224	-1.598
Guadeloupe	5	-0.016	-1.850				

Table A2a: Core and Semi-Periphery in 1980, ranked by position and CA of RE

Country	Position	CA of RE		Country	Position	CA of RE	
		1st Dim	1st Dim			1st Dim	1st Dim
United States	1	0.075	7.962	South Africa	2	0.027	7.413
Germany	1	0.068	9.255	Hungary	2	0.026	7.207
France	1	0.062	8.408	Argentina	2	0.026	2.561
Italy	1	0.060	8.224	Czech Republic	2	0.025	7.111
United Kingdom	1	0.059	8.818	Ukraine	2	0.025	3.477
Japan	1	0.058	8.464	Portugal	2	0.023	5.276
China	1	0.055	7.648	Greece	2	0.023	6.751
Netherlands	1	0.050	8.716	Venezuela	2	0.023	2.230
Belgium-Luxembourg	1	0.049	8.162	Philippines	2	0.022	4.760
South Korea	1	0.049	7.980	Iran	3	0.023	1.203
Spain	1	0.048	8.181	Chile	3	0.021	2.253
Singapore	1	0.041	7.908	Vietnam	3	0.021	3.370
Taiwan	2	0.045	4.269	Nigeria	3	0.019	1.502
Russia	2	0.043	5.661	Romania	3	0.019	4.241
Hong Kong	2	0.041	7.351	New Zealand	3	0.018	3.405
Switzerland	2	0.039	8.600	Algeria	3	0.018	0.397

Table A2a continued

Brazil	2	0.038	7.151	Colombia	3	0.016	3.325
India	2	0.037	6.759	Kuwait	3	0.016	-0.523
Sweden	2	0.037	8.621	Pakistan	3	0.015	4.398
Canada	2	0.036	7.918	Slovakia	3	0.015	2.959
Thailand	2	0.036	7.029	Slovenia	3	0.014	3.237
Malaysia	2	0.034	7.571	Morocco	3	0.014	3.268
Austria	2	0.033	8.429	Kazakhstan	3	0.014	0.177
Australia	2	0.033	7.369	Libya	3	0.012	-2.084
Turkey	2	0.033	7.504	Peru	3	0.012	1.376
Mexico	2	0.032	7.518	Oman	3	0.011	-0.448
Finland	2	0.031	5.643	Tunisia	3	0.011	1.770
Denmark	2	0.031	7.991	Belarus	3	0.010	-1.220
Indonesia	2	0.030	6.196	Croatia	3	0.010	2.574
Saudi Arabia	2	0.030	7.208	Bulgaria	3	0.009	2.570
Ireland	2	0.030	7.607	Lithuania	3	0.008	-1.088
Poland	2	0.030	6.170	Ecuador	3	0.008	0.277
Norway	2	0.028	5.916	Trinidad & Tobago	3	0.007	-1.238
United Arab Emirates	2	0.028	7.129	Netherlands Antilles	3	0.005	-2.573
Israel	2	0.027	5.134	Equatorial Guinea	3	-0.019	-5.331

Table A2b: Periphery in 2001, ranked by position and CA of RE

Country	Position	CA of RE	EDM	Country	Position	CA of RE	EDM
		1st Dim	1st Dim			1st Dim	1st Dim
Iraq	4	0.013	-2.436	Nepal	4	-0.014	-5.579
Egypt	4	0.012	4.978	Cambodia	4	-0.014	-4.301
Syria	4	0.009	-0.713	Mongolia	4	-0.016	-4.853
Costa Rica	4	0.009	-1.095	Greenland	4	-0.020	-4.403
Estonia	4	0.007	1.057	Haiti	4	-0.020	-4.573
Qatar	4	0.007	-0.910	Tanzania	5	-0.012	-0.483
Bangladesh	4	0.006	1.633	Mozambique	5	-0.014	-3.688
Guatemala	4	0.006	-0.892	Madagascar	5	-0.014	-3.154
Cote d'Ivoire	4	0.005	-2.658	Saint Kitts and Nevis	5	-0.015	-1.407
Sri Lanka	4	0.005	0.073	Fiji	5	-0.015	-4.616
Jordan	4	0.004	0.829	Ethiopia	5	-0.015	-0.210
Bahrain	4	0.004	-0.421	Uganda	5	-0.016	-2.221
Yugoslavia	4	0.002	2.127	Guyana	5	-0.017	-5.655
Latvia	4	0.002	-1.607	Guinea	5	-0.017	-3.014
Angola	4	0.002	-1.758	Georgia	5	-0.018	-3.443

Table A2b continued

Panama	4	0.001	-1.390	Mali	5	-0.019	-1.307
Uzbekistan	4	0.001	-4.103	Laos	5	-0.019	-5.226
El Salvador	4	0.001	-2.040	Bermuda	5	-0.020	-4.343
Uruguay	4	0.001	-0.906	Barbados	5	-0.020	-2.730
Kenya	4	0.001	-1.433	Malawi	5	-0.020	-4.373
Honduras	4	0.000	-2.893	Suriname	5	-0.020	-6.570
Jamaica	4	-0.001	-3.260	Mauritania	5	-0.022	-4.115
Yemen	4	-0.001	-2.009	Cayman Islands	5	-0.023	-3.404
Dominican Republic	4	-0.001	-0.939	Tajikistan	5	-0.023	-5.913
Myanmar	4	-0.001	-4.845	Gibraltar	5	-0.025	-5.202
Ghana	4	-0.002	0.305	Kyrgyzstan	5	-0.026	-6.041
Cuba	4	-0.002	0.305	Togo	5	-0.026	-3.401
Malta	4	-0.002	-0.745	Benin	5	-0.026	-1.999
Lebanon	4	-0.003	4.616	Belize	5	-0.026	-5.587
Cyprus	4	-0.003	2.947	Armenia	5	-0.027	-5.038
Sudan	4	-0.004	0.596	Niger	5	-0.028	-4.160
Iceland	4	-0.004	-0.819	Kiribati	5	-0.029	-5.978
Paraguay	4	-0.004	-3.587	Guinea-Bissau	5	-0.030	-4.809
Cameroon	4	-0.005	-1.802	Afghanistan	5	-0.030	-5.962
Papua New Guinea	4	-0.005	-4.880	Burkina Faso	5	-0.031	-4.314
Bosnia & Herzegovina	4	-0.005	-3.937	Somalia	5	-0.031	-5.673
Mauritius	4	-0.006	-1.855	Djibouti	6	-0.032	-5.309
North Korea	4	-0.006	-2.098	Rwanda	6	-0.037	-4.323
Macau	4	-0.006	-3.081	Maldives	6	-0.041	-5.354
Turkmenistan	4	-0.006	-5.443	Sierra Leone	6	-0.044	-4.279
Zimbabwe	4	-0.006	-3.527	Chad	6	-0.048	-5.126
Liberia	4	-0.006	-4.340	Br. Indian Ocean Ter.	6	-0.048	-3.388
Macedonia	4	-0.006	-2.405	Falkland Islands	6	-0.049	-4.176
Bolivia	4	-0.007	-3.799	Comoros	6	-0.050	-6.169
Senegal	4	-0.007	-1.244	Gambia	6	-0.053	-5.197
Gabon	4	-0.008	-4.482	Seychelles	6	-0.053	-5.375
Azerbaijan	4	-0.008	-3.302	Burundi	6	-0.055	-5.862
Brunei	4	-0.008	-5.117	Central African Rep.	6	-0.055	-5.642
Zambia	4	-0.009	-4.574	Bhutan	6	-0.059	-5.144
Nicaragua	4	-0.009	-4.208	Turks & Caicos Is.	6	-0.062	-5.215
Moldova	4	-0.010	-3.562	Solomon Islands	6	-0.066	-4.881
Congo	4	-0.011	-3.392	St Pierre & Miquelon	6	-0.077	-2.575
New Caledonia	4	-0.011	-1.242	St Helena	6	-0.095	-5.083
Bahamas	4	-0.012	-2.822	Western Sahara	6	-0.109	-1.426
Albania	4	-0.014	-3.758	Reunion	6	-0.166	-1.660
Zaire	4	-0.014	-4.260				

MODELING ERROR IN QUANTITATIVE MACRO-COMPARATIVE RESEARCH

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ABSTRACT

Much quantitative macro-comparative research (QMCR) relies on a common set of published data sources to answer similar research questions using a limited number of statistical tools. Since all researchers have access to much the same data, one might expect quick convergence of opinion on most topics. In reality, of course, differences of opinion abound and persist. Many of these differences can be traced, implicitly or explicitly, to the different ways researchers choose to model error in their analyses. Much careful attention has been paid in the political science literature to the error structures characteristic of time series cross-sectional (TSCE) data, but much less attention has been paid to the modeling of error in broadly cross-national research involving large panels of countries observed at limited numbers of time points. Here, and especially in the sociology literature, multilevel modeling has become a hegemonic – but often poorly understood – research tool. I argue that widely-used types of multilevel models, commonly known as fixed effects models (FEMs) and random effects models (REMs), can produce wildly spurious results when applied to trended data due to mis-specification of error. I suggest that in most commonly-encountered scenarios, difference models are more appropriate for use in QMC.

INTRODUCTION

Quantitative macro-comparative research (QMCR) involves the statistical analysis of quantitative data about countries. Undertaking QMCR is at the same time both much simpler and much more difficult than engaging in other kinds of social research. It is simple because it often involves the application of off-the-shelf statistical techniques to widely-available published data; a typical study can be completed in a few months from start to finish by a lone researcher working in isolation on a desktop computer. It is difficult for the same reasons: low barriers to entry mean that almost every combination of variables that can be studied has been studied. Moreover, we generally only get one additional year of new data points every year, and worse, each new year's data points look much like those from the year before. Progress in most branches of social research relies on the constant generation of new data – both new cases and new variables – to drive forward both research and theory, but QMCR practitioners are doomed to pick over the same well-studied datasets again and again. New ideas arise all the time, but all too often there

are no new data on which to test them. Instead of filling old bottles with new wine, we fill new bottles with the same old wine year after year.

A large and recurring area of controversy in QMCR is the appropriate modeling of error. Error in a statistical model is the sum total of all other factors not explicitly accounted for in the model. It is reflected in the degree to which the observed values of dependent variables differ from their predicted values. Every statistical model includes implicit or explicit assumptions about the sources, distributions, and structures – in short, the behavior – of error. Misspecification of the error in a statistical model can lead to reported coefficients that are lower or higher than the true effects they are intended measure (biases). It can also lead to reported standard errors for coefficients that are lower or higher than they should be (resulting in overconfidence or underconfidence in results). I suspect that most researchers spend much more time thinking about what variables to include in their models than about how to specify the error structures of their models. This is a mistake. Error assumptions can have enormous impacts on statistical results and their interpretation. Moreover, even the choice of variables for inclusion in a model can be thought of as a form of error modeling. We must pay greater attention to the role played by (statistical) error in QMCR.

Toward that end, this paper is meant to serve as a guide to (and critique of) the treatment of error in QMCR. Though the mathematics underlying the statistical models used in QMCR are well-understood (by the statistical software writers, if not by the statistical software users), the implications of applying these models in typical QMCR settings are not. Unfortunately, most methodological guidebooks rely heavily on mathematical, rather than verbal, explication, which leaves a major gap in most researchers' understandings of the implications of error modeling. After all, it is usually not the mathematics but the appropriate verbal expression of the mathematics that is at the heart of both substantive and methodological debates in QMCR. Accordingly, this paper focuses on methods, not mathematics.

Throughout the paper, concepts are illustrated using data on the relationship between national income per capita and infant mortality rates as an applied example. The national income and infant mortality data used are taken from the World Bank's World Development Indicators 2007 database. National income is operationalized as gross domestic product per capita evaluated at market foreign exchange rates. Both series have been logged to correct for positive skew. The correlation between national income and infant mortality for 167 countries for 2005 is $r = -.89$. As would be expected, there is a very close connection between the two variables: higher levels of national income per capita are associated with lower levels of infant mortality. It should be noted that even in the 2005 panel, 41 countries, or almost a fifth of the world's total, are missing data. For earlier years, levels of missing data are of course higher.

The remainder of this paper is divided into seven sections. I begin by discussing the role played by error in statistical models and the consequent relevance of significance testing in QMCR. I then review typical dependence structures in that error and ways that control variables can be used to model those structures. This discussion of control variables leads into a wider discussion of causality in QMCR. Appropriate error modeling is central to the credibility of causal claims. Model results are rarely if ever problematic in their own right; controversies only arise when coefficients are interpreted causally. An important class of models that I believe have regularly given rise to inappropriate causal claims is that of multilevel models (MLMs), which include what are often called fixed effects models (FEMs) and random effects models (REMs). I devote a full section to these models, then a second section to comparing and contrasting them to

a competing class of models, difference models. I conclude with recommendations for sound methodology in QMCR, particularly concerning the treatment of time.

THE RELEVANCE OF SIGNIFICANCE TESTING

At the core of the modern practice of QMCR is the determination of the statistical significance of independent variables when used in modeling dependent variables of interest. Many of the hottest debates in QMCR hinge not on the magnitudes of the effects of independent variables, but on the question of whether or not such effects could have arisen by chance. Typically, QMCR operates within a Neyman-Pearson framework of testing null hypotheses that independent variables are unrelated to dependent variables; when these null hypotheses are rejected, the relationships between independent and dependent variables are inferred to be statistically significant.

Many scholars argue that it is inappropriate to make statistical inferences in QMCR settings, though few make such arguments in print (Berk 2004:51-56 is a notable exception). Occasionally, even macro-comparative researchers repeat this claim (e.g., Ebbinghaus 2005). Their argument, in a nutshell, is that since the data used in QMCR usually constitute entire populations of cases, rather than random samples from larger populations, there is nothing to make inferences about. They argue that the parameters estimated in QMCR are not sample estimates of population parameters but are themselves population parameters, and thus not subject to error. A regression line, in this view, represents nothing more than the mean of the dependent variable when conditioned on the independent variable. They argue that it is inappropriate to say that the slope of this line is "significantly" different from zero: the slope is what it is, but with no further implications. To claim otherwise, they argue, is to posit that there exists some "imaginary superpopulation" (Berk 2004:51) of countries that exhibits a true, population regression line, and that the observed population of countries is merely one of many possible samples of this superpopulation. The argument that statistical significance testing is inappropriate when the underlying data constitute a population is completely without merit.

Key to the argument that statistical inference is inappropriate when analyzing populations is the (mis-) conceptualization of regression coefficients as sample estimates of population parameters. They are not. Regression coefficients are random variables, but not sample estimates. Confusion arises because sample estimates of population parameters are random variables, but there are many other kinds of random variables. According to the *Cambridge Dictionary of Statistics*, a random variable is a "variable, the values of which occur according to some specified probability distribution" (Everitt 2002:313). So, for example, the sample mean of a variable randomly drawn from a larger population of cases for which that variable has been measured is known to follow a Normal distribution with mean equal to the population mean and variance equal to the population variance divided by the size of the sample. The sample mean of a variable won't always equal the population mean, but it will vary around it normally, regardless of the underlying distribution of the variable. It is a random variable – it is "a variable, the values of which occur according to some specified probability distribution" – in this case, a Normal distribution of known parameters.

The dependent and independent variables in regression analyses are emphatically NOT random variables. In everyday language they may be labeled as such, and even some standard reference sources do not distinguish between the mathematical definition of a random variable

and its common usage (e.g. Vogt 1999:235). Clearly, however, none of the variables used in QMCR (or, indeed, any social science research) follow known, pre-specified probability distributions. This is true whether the data on which a regression is estimated constitute a population or a sample from a larger population. In regression analyses, the key random variables are the regression error term and the regression coefficients. Other regression estimates, like R-squared, are also random variables. Regression error is a random variable by construction: remember that regression error is assumed to be a Normal random variable with mean zero.

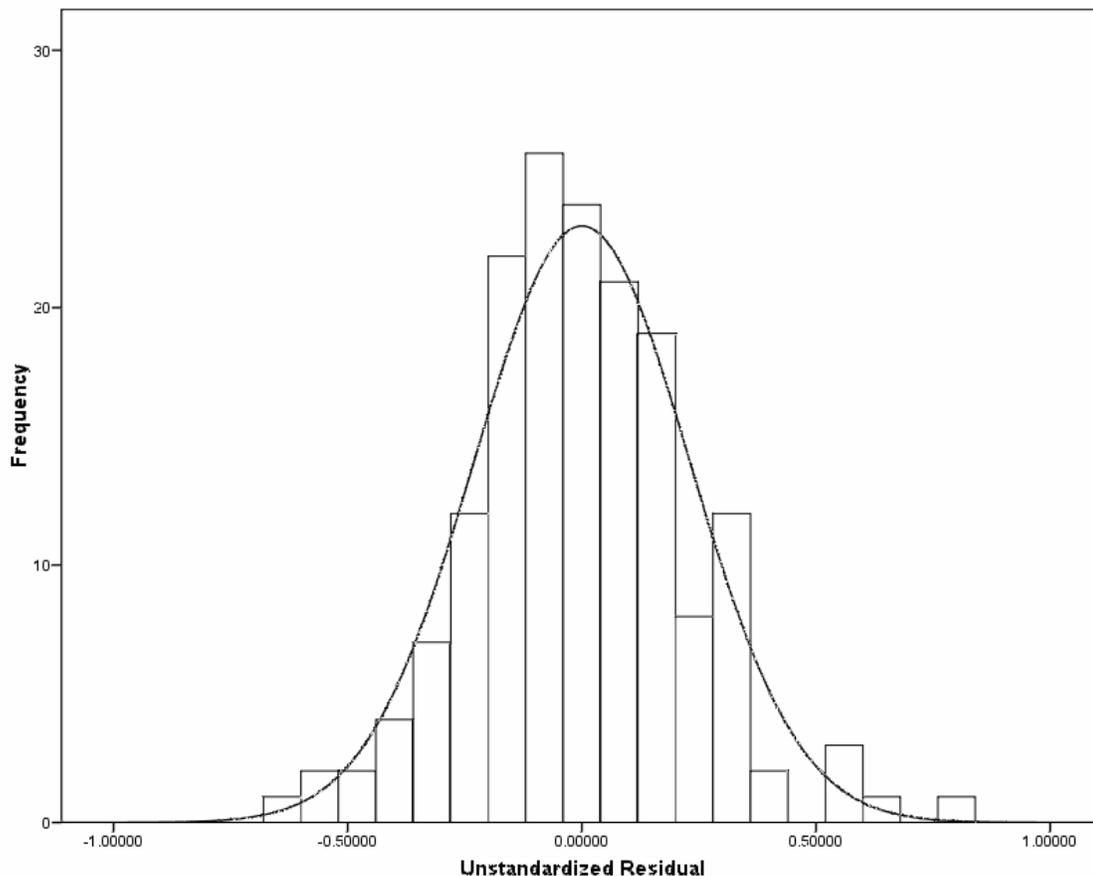
The dependent variable in a regression analysis is modeled as a linear function of the independent variables plus this random variable. Since the values of the independent variables are fixed (not random), the Normal distribution of the regression error propagates through the model to the coefficients, which, due to the finite number of cases, are distributed *t* rather than Normal. (When the independent variables are not fixed, but are instead measured with error, attenuation adjustments have to be made to the estimated regression coefficients.) A simple *t* test can then be used to test each estimated regression coefficient against the null hypothesis that the mean of its sampling distribution is actually zero. If the null hypothesis is rejected, a coefficient is said to be "significant." What this means is that a model with coefficients equal to zero would have been unlikely to have produced the observed data, given the assumption that the regression errors are independent normally distributed random variables with zero mean and constant variance. Testing the significance of a coefficient does not in any way imply the existence of a larger population of cases to which the coefficient applies.

Other than its coincidental presence in the statistical term "sampling distributions" of the regression coefficients, nowhere in this process is sampling involved. Quite the contrary: an implicit assumption of regression modeling is that the data being analyzed constitute a population, not a sample from some larger population. In some cases (for example, when variables have been chosen for inclusion in the model based on a stepwise selection algorithm) regression coefficients estimated using sample data may not even be unbiased estimates of the corresponding population coefficients. In all cases, the sample *r*-squared statistic is a biased estimate of the population *R*-squared, due to the phenomenon of "fitting to the sample." This is why so-called "adjusted *R*-squared" statistics (Lucke and Whitely 1984) are used to estimate the percent of variance in the population dependent variable that would be achieved by applying sample-based estimates of the regression coefficients to the population independent variables. Note that such "adjusted *R*-squared" statistics are generally not applicable in most QMCR analyses, since QMCR data structures almost always constitute complete populations, not samples drawn from some larger population. Though often used inappropriately, "adjusted *R*-squared" is completely irrelevant in typical QMCR settings. Overfitting can be a problem in QMCR, but it should be evaluated using the *F* statistic, not by making an inappropriate adjustment to the reported *R*-squared.

If there's no sampling involved and regression errors are not in any sense sample estimates of population means, why should regression errors be expected to be normally distributed? As Berk acknowledges (2004:54) the Central Limit Theorem of probability theory predicts that they should. As discussed above, the Central Limit Theorem ensures that any random variable that is the sum of a large number of other random variables will follow a Normal distribution. The number of summed variables need not even be very large if they are reasonably well-behaved. Empirically, when regression variables are properly specified (with appropriate transformations) the regression errors are almost always credible realizations of a Normal

distribution. For example, the distribution of realized regression errors from the regression of infant mortality on national income is summarized in Figure 1. This realized distribution is very nearly Normal. In fact, a Kolmogorov-Smirnov test emphatically fails to reject the Normal distribution as the origin of these realizations ($p = .940$). It is clear that the assumption that regression errors are drawn from a Normal random variable is not only theoretically well-grounded, but empirically reasonable as well.

Figure 1: Distribution of Realized Regression Error in a Model for Infant Mortality



ERROR DEPENDENCE STRUCTURES

It is important to understand the role played by error in statistical modeling because different model designs either implicitly or explicitly contain assumptions about the behavior of error. In the simplest statistical models, the error associated with each case is assumed to be independent of the errors associated with every other case. Such independent errors exhibit no patterns across cases. There are many ways in which the peculiarities of QMCR data structures, however, lead to highly patterned forms of error. For starters, countries are not independent cases: the United

States and Canada, for example, are strongly linked in almost every way. Even worse, when the same countries are included multiple times in the same dataset, their multiple realizations (US 2000 versus US 2005) can hardly be considered to be independent of each other. For these and other reasons, regression models in QMCR often exhibit dependence in their regression error structures.

There are two broad classes of regression error dependence: mean dependence and variance dependence. Mean dependence occurs when the expected value of the regression error is not zero for a class of cases; for example, East Asian countries have systematically lower than expected levels of infant mortality, conditional on their income levels. Variance dependence occurs when the variability of the regression error is not constant across all classes of cases; for example, a dependent variable like infant mortality may be much more poorly measured in sub-Saharan Africa than in the rest of the world, resulting in a systematically higher error variance in sub-Saharan African countries than in others. In general, mean dependence is easier to identify and address than variance dependence, and is the more serious problem, since it directly affects the estimation of regression coefficients (both their levels and their standard errors). Variance dependence, on the other hand, typically affects only the standard errors of coefficients, not their levels.

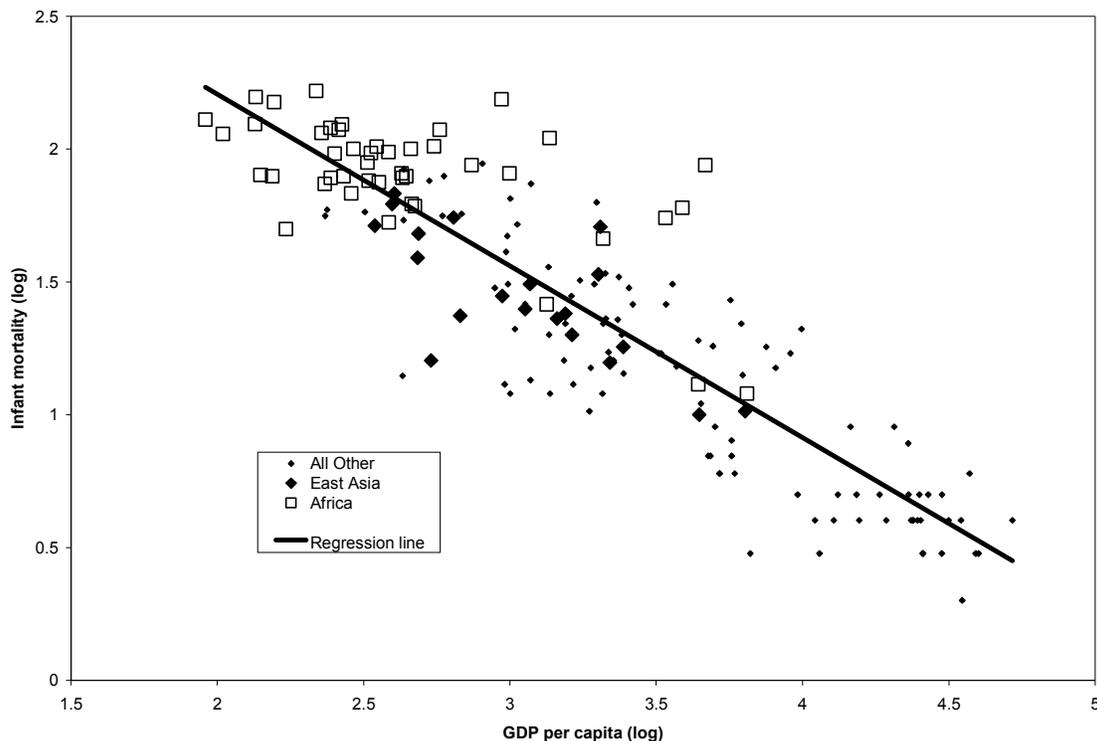
The simplest form of mean dependence results from the questionable treatment of countries as representing statistically independent cases, as discussed above. Entire blocs of countries may behave in statistically similar ways, despite their nominal independence as distinct countries. For example, all oil-dependent economies might be expected to depart from their modeled rates of economic growth in the same ways at the same times: higher than modeled when oil prices are high and lower than modeled when oil prices are low. As a result, controls for OPEC membership or oil dependence are common in growth regressions. Oil dependence, however, is simply an extreme example of a much more general phenomenon. There are probably as many ways for countries to covary as there are countries, and probably more, since the number of possible combinations of country covariation is far larger than the number of countries.

Ironically, the plethora of possible country dependence "clubs" is perhaps a blessing in disguise. Countries are members of so many potential dependence clubs that their overall influence might, in most cases, aggregate to a normally distributed contribution to general background error. Consider: the Europa World database lists 95 United Nations and "major" non-UN intergovernmental organizations that countries may belong to. Add to these continental clubs and clubs based on economic characteristics (natural resource dependence, susceptibility to agricultural shocks, participation in global commodity chains, etc.) and the typical country may be found to belong to dozens of potential dependence clubs. It may be necessary to adjust only for the club memberships that are most directly relevant to any given analysis. Luckily, this is easy to do: including a dummy variable for club membership will in most cases eliminate any associated error dependence. Even this simple adjustment is not always necessary. So long as countries representing many dependence clubs are included in the data on which a model is estimated, error dependence structures affecting just a few will have no measurable impact on the broader results. This is true even when such club dummies turn out to have statistically significant coefficients.

For example, an OLS regression of infant mortality in 2005 on national income in 2005 (N=167) yields a metric coefficient of $b = -.646$ for national income, with a standard error of $SE_b = .026$. Controlling for East Asian location as a dependence club results in no change at all in the coefficient (to three decimal places) and only a trivial reduction in its standard error (to .023).

This is despite the fact that the effect of East Asian status is highly significant ($t = -4.798$). East Asian countries have systematically lower than modeled infant mortality rates, but this does not substantively affect the overall evaluation of the effect of national income on life expectancy. The reason for this is illustrated in Figure 2. Although there is a clear East Asian error dependence (16 of 20 East Asian countries' infant mortality rates fall below their modeled values), its impact is spread evenly across the range of the regression. Consequently, it affects the intercept of the regression line, but not its slope. Sub-Saharan African countries, on the other hand, form an equally obvious dependence club, but one with a structure that does not fit so neatly into the overall pattern of the worldwide relationship between national income and infant mortality. Controlling for sub-Saharan African location does substantially affect the coefficient for national income, reducing the estimated magnitude of the slope to $b = -.558$ ($SE_b = .029$).

Figure 2: Illustration of East Asian and African Dependence Clubs, 2005

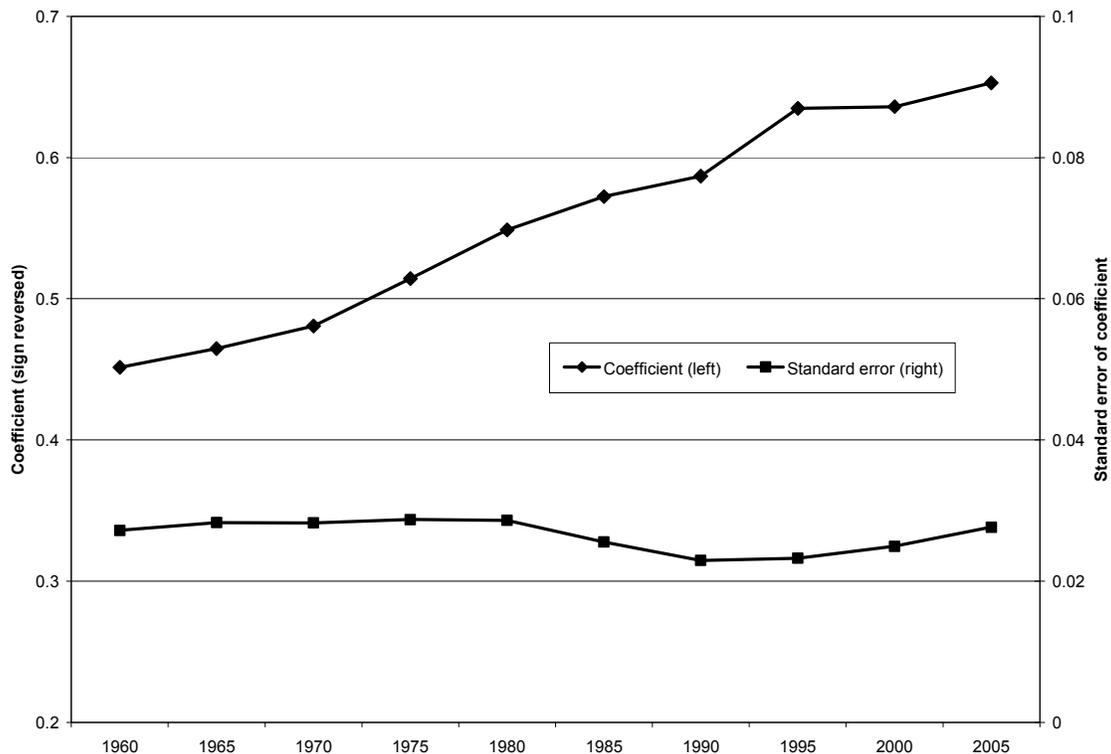


A similar type of mean dependence occurs when the same country appears multiple times as a case in the same regression analysis. This is, in a way, similar to the dependence club situation, only instead of several countries being expected to exhibit similar errors one country is expected to exhibit similar errors each time it is used as a case. This situation occurs when some attribute of a country, rather than the country itself, is the unit of analysis. For example, countries may be listed multiple times in the same data structure when each case represents an ethnic group within a country. In a study of how the average educational levels of ethnic groups affect their

average income levels, it is important to remove overall country biases in income levels: all ethnic groups in rich country would be expected to have higher incomes than all ethnic groups in poor countries, regardless of their relative educational levels. This can be accomplished by including country dummy variables in the regression model. This is mathematically equivalent to a fixed effects model design (discussed below), though the data structure is distinct (since the multiple entries per country represent distinct cases, not repeated measures of the same case).

Country dummies are emphatically not appropriate for eliminating country-related mean dependence in regression error in repeated measures designs. In fact, mean dependence does not even bias regression coefficients in repeated measures designs when the panels are balanced – biases are only introduced when some countries are represented more times than others, which is really a case of sample selection bias due to missing data in unbalanced panels. This can be illustrated by extending the infant mortality – national income example. Coefficients from regressing infant mortality on national income for each of the ten five-year intervals 1960-2005 yields coefficients ranging from $b = -.451$ (1960) to $b = -.653$ (2005). These coefficients and their standard errors, estimated on the constant panel of $N=77$ countries for which data are available for both variables for all years, are plotted in Figure 3.

Figure 3: Coefficients from Cross-Sectional Regressions of Infant Mortality on GDP per capita, 1960-2005 (constant panel $N=77$)



A pooled regression of infant mortality on national income, including data from all years in a balanced panel of $N=770$, yields a coefficient for national income of $b = .587$, well within the range of the ten cross-sectional coefficients. In fact, it is very near their simple arithmetic mean of $-.554$. Adding country dummies to the model, however, dramatically changes the estimated coefficient for national income; it nearly doubles to $b = -1.034$. Obviously, an adjustment meant to reduce country dependence in regression error should not have such a dramatic effect on a coefficient. Country-related mean dependence should not pull the slope of the national income – infant mortality relationship in the pooled data so far outside the range of slopes observed in the constituent cross-sections. What happens when country dummies are included in the pooled model is not a correction for potential country-related mean dependence but a complete change in the character (and meaning) of the model. The actual mechanics behind this are explored below in the discussion of multilevel models.

Returning to the regression of infant mortality on national income (without country dummies), the main difference between the pooled analysis and the ten cross-sectional analyses is not the magnitude of the coefficient but the size of its standard error. Standard errors for the national income coefficient in the ten cross-sectional models range narrowly from $SE_b = .023$ to $SE_b = .029$. The standard error for the national income coefficient in the pooled cross-sectional model, by contrast, is only $SE_b = .011$. This is because the sample size has been increased by a factor of ten without a corresponding increase in the total variability of the data (since the variability of the data is constrained by the fact that they are drawn from the same 77 cases, even measured if at different points in time). Were data for infant mortality available annually, instead of every five years, the number of cases could be further multiplied by a factor of five, again without any corresponding increase in the scope of coverage. Of course, there is no philosophical reason for stopping at annual increments. The *reductio ad absurdum* would be to include every country as a case every fraction of a second, to yield millions of "cases" for analysis each identical or nearly identical to the one before. This would drive standard errors for regression coefficients down toward zero as the number of "cases" rises toward infinity.

Repeated measures designs incorporate a serious country-related mean dependence in their regression error structures, but it is not as simple as a broad, country-wise mean bias. Repeated measures from the same country are not just correlated with each other; their dependence is highly structured. Each realization of country data is directly conditioned on the one immediately preceding it, but not directly conditioned on further prior realizations. The dependence structure is not universally mutual, but instead is structured into a directed chain running from earliest to latest realization. This kind of dependence structure is called "Markovian" dependence. Assuming the dependence between adjacent realizations is linear and of the same magnitude for all time points, it is not only Markovian but specifically autoregressive with order 1. Such AR(1) error structures are very common in QMCR but cannot be estimated using OLS regression. They can, however, be estimated using iterative MLE; procedures for doing so have existed for over fifty years. Since MLE techniques are approximations (as opposed to OLS estimates, which are exact), different statistical software can give slightly different solutions to regression models estimated using MLE.

Using SPSS PROC MIXED to estimate the MLE solution for the national income coefficient in a model for infant mortality with AR(1) errors within countries is $b = -.609$, with $SE_b = .026$ (balanced panel of $N=770$ cases). These figures are in line with the cross-sectional results of $b = -.451$ to $b = -.653$ and $SE_b = .023$ to $SE_b = .029$. It turns out that including ten panels

of very similar data for multiple years improves very little on simply analyzing a single cross-section, once appropriate specification has been made to the error term.

Other forms of sequentially organized dependence structures are possible, but are much less commonly encountered in QMCR. When annual data are used, however, more complex error modeling is required. The ordinary business cycle of 3-8 years introduces error dependence structures into many QMCR variables (national income, investment, international trade, etc.) that are not Markovian at the annual level. Periodic sinusoidal regression error structures correspond to order 2 autoregressive processes; asymmetrical cycles like the business cycle are best modeled using autoregressive - moving average (ARMA) models. Quarterly and monthly data that incorporate seasonality take "integrated" ARIMA models. Such complex econometric models are rarely encountered in QMCR, but models based on data structures that include annual observations must take them into account. Since fine-grained annual variability is rarely the focus of QMCR, a reasonable fudge is simply to work with more widely-spaced data. As Chase-Dunn (1989) points out, the substantively relevant "width of a time point" (321) in QMCR may not necessarily be one year.

Compounding these difficulties of mean dependence in QMCR regression error structures is the much more subtle problem of variance dependence. In a repeated measures panel consisting of multiple countries measured at multiple time points, it is possible that different countries will exhibit systematically greater or lesser regression error variance than others. In such cases, panel-weighted least squares (PWLS) estimation can be used to adjust for variance dependence. Beck and Katz (1996), however, show that PWLS is only effective when the number of repeated observations for each country is large (20 or more time points). Such time-series cross-sectional (TSCS) data structures based on annual observations of countries are not, however, typical of QMCR of the kind being examined here. Beck and Katz (1996) go on to show that OLS estimates with panel-corrected standard error (PCSE) adjustments, developed and discussed at length in Beck and Katz (1995), produces much better estimates than PWLS when the number of time points is "small," or, in their examples, as few as five.

Of course, in much QMCR having as many as five repeated measures per country is a rare luxury. Nonetheless, the PCSE approach is a major methodological advance, and should be applied whenever repeated measures of the same cases are included in QMCR. As Beck and Katz (1996) show, PCSE adjustments systematically reduce the underestimation of the standard errors of regression coefficients due to country-related variance dependence. They also have the gratuitous effect of reducing or eliminating biases due to mean dependence structures in which countries' regression errors are correlated to each other in a fixed pattern that is the same for all time points (Beck and Katz 1995). This may sound esoteric, but it is in fact a very common condition. As discussed above, spatial correlation (including neighbor correlation) among countries is almost certainly a feature of all QMCR data structures. In cross-sectional analyses, such correlation structures cannot be detected or adjusted for, but in panels with multiple repeated measures, the PCSE approach provides an effective correction.

The PCSE approach, though developed for use with OLS regression models, can also be applied to GLM designs. An effective strategy is to estimate a GLM regression with an AR(1) error structure, then correct the results for spatial correlation and variance dependence in the remaining regression error using a PCSE adjustment. This two-step approach corrects for most of the common complications that apply when using repeated measures of countries as cases in QMCR. An alternative strategy is to use OLS regression including lagged dependent variables

(Beck et al. 1993), but this strategy introduces downward biases when the autocorrelation of the dependent variable is high (Keele and Kelly 2005). Another alternative is to use OLS regression based on change scores (Beck and Katz 1995) to eliminate the autoregressive error structure within countries, but this strategy can also lead to large downward biases in the estimation of coefficients (Wawro 2002). See Wilson and Butler (2007) for a comparison of competing methods for dealing with TSCS data.

COMPLEMENTARY, COMPETING, AND ORTHOGONAL CONTROLS

In substantive terms, it is reasonable to think of the regression error as the effect on the dependent variable of "all other factors" not included in the model. The vast majority of possible QMCR variables, of course, are orthogonal (not linearly related) to the dependent variable of interest in any particular analysis, and thus can safely be ignored. They do not contribute to regression error as "other factors." Many variables, however, are related to the dependent variable, but are also colinear with the independent variables of interest, competing with them for explanatory power in the regression model. To the extent that they compete with or "partial" variables that are already in the model, they are not "other factors" that contribute to regression error but more like alternative operationalizations of the independent variables. A third class of variables that are independently related to the dependent variable can, however, be often identified. When not explicitly included in the regression model, these complementary variables are clear examples of the "other factors" that are subsumed into the regression residual. Including them directly reduces the variance of the regression residual, illustrating the reasonableness of its interpretation as a sum of "all other factors" not included in the model.

These three classes of potential control variables – complementary, competing, and orthogonal – affect the results of statistical models in distinctive ways and are subject to different rationales for inclusion as controls. All social scientists struggle with the question of what variables to include in (and, implicitly, what variables to exclude from) their statistical models. This question is made particularly difficult in QMCR by the fact that the number of variables available in published data compilations far exceeds the number of countries available for analysis as cases. Parsimony is thus at a premium. In research based on sample surveys with thousands of respondents, the effects of dozens of independent variables can be estimated simultaneously, and though this may present serious problems of interpretation, it is typically not a problem from the standpoint of estimation. In QMCR, with relatively few countries available as cases, sufficient degrees of freedom usually exist for estimating the effects of no more than a dozen or so variables, and often far fewer. As a result, practitioners of QMCR typically must show far greater care than other social scientists in their choices of variables to include in their statistical models.

Complementary controls, though hard to identify, are almost always desirable in a model, since they serve mainly to "soak up" error that would otherwise tend to obscure the relationships between the independent variables of interest and the dependent variable. An illustration of the effective use of a complementary control is given in Table 1, Models 1 and 2. One might reasonably surmise that countries with greater female labor force participation (LFP) would tend to have lower levels of infant mortality. Infant mortality is regressed on female LFP in Model 1. The coefficient, as expected, is negative, but it is not statistically significant. When urbanization

is introduced as a control, however, the coefficient for female LFP becomes highly significant (Model 2). From the standpoint of female LFP, urbanization is a complementary control. Its inclusion in the model dramatically reduces the model's residual error variance, thus clarifying the (relatively weak) effect of female LFP on infant mortality. The inclusion of urbanization increases the signal-to-noise ratio in the relationship between female LFP and infant mortality not by increasing the strength of the signal but by reducing the volume of the noise. Complementary controls can be thought of as very useful error filters.

Table 1: Illustration of Complementary, Competing, and Orthogonal Controls – Models for Infant Mortality (log), 2005

	Model 1	Model 2	Model 3	Model 4
[Constant]	1.556 (0.213)	2.575 (0.182)	3.797 (0.132)	3.816 (0.211)
Female LFP (%)	-0.005 (0.005)	-0.010 (0.004)	-0.007 (0.002)	-0.007 (0.002)
Urban population (%)		-0.014 (0.001)	0.000 (0.001)	0.000 (0.001)
GDP/capita - F/X (log)			-0.662 (0.040)	-0.662 (0.040)
Population (log)				-0.003 (0.024)
R-squared	.005	.450	.801	.801
N	162	162	162	162

Note: Entries in table are metric coefficients (standard errors in parentheses)

Competing controls are not always so obviously desirable. What if we were interested in the relationship between urbanization and infant mortality? Model 3 reveals an incredibly significant negative relationship between the two variables. This makes sense, since we would expect highly urbanized countries to have lower infant mortality rates than predominantly rural countries. So far so good. But controlling for national income (as nearly all GMCR studies do) brings the estimated effect of urbanization on infant mortality down to zero. National income is clearly a competing control vis-à-vis urbanization (though not vis-à-vis women's LFP). Urbanization is completely unrelated to infant mortality at any given level of national income, but it seems highly unlikely that urbanization does not reduce infant mortality. This is a difficult conundrum. The use and interpretation of competing controls are tied up with assumptions about concepts, causality, and causal order.

If urbanization is hypothesized to be a proximate cause of infant mortality, and if national income is thought to be a concept completely distinct from urbanization, then it makes sense to control for national income and to conclude that urbanization has no detectable effect. On the other hand, it could be that urbanization affects infant mortality through a range of intermediating variables (such as the availability of medical staff, electricity, and clean water) and that national income acts as a proxy for these very variables, in which case it might be inappropriate to control for national income (despite the fact that it has such a strong effect on the dependent variable). In this case it is probably appropriate to control for national income, but in many cases involving potential competing controls it might not be appropriate to include them.

The third class of potential control variables, orthogonal controls, have little effect on the coefficients of the rest of the variables in a model. An example of an orthogonal control is given in Model 4. Here, population size has virtually no effect on infant mortality and virtually no effect on the coefficients of the other variables in the model. The only reason to include an orthogonal control is to demonstrate that it is, in fact, orthogonal. Once this has been demonstrated, Occam's razor suggests they be eliminated from the model. The judicious use of competing controls combined with the elimination of orthogonal controls would lead to much simpler, more easily grasped models in QMCR.

CAUSALITY AND MODEL DESIGN

The most basic model design and still the one most widely used in QMCR is the cross-sectional multiple linear regression model: a single dependent variable is regressed on one or (usually) more independent variables. Models 1-4 are all examples of cross-sectional models. Cross-sectional models are easy to interpret largely because their error structures are so straightforward: errors are typically assumed to be independent across cases and normally distributed with constant variance. Because of this simplicity, the cross-sectional model is probably the model design best suited to answering the simple question "does the dependent variable covary across countries with the independent variable?" Most early QMCR (Bornschiefer, Chase-Dunn and Rubinson 1978) and many QMCR studies today use a simple cross-sectional design. As new data come available for such QMCR topics as state structure (Evans and Rauch 1999), the environment (Marquart-Pyatt 2004), and political corruption (Sandholtz and Taagepera 2005), cross-sectional models are usually the first kind of model analyzed because initially only cross-sectional data are available.

As fields mature and over-time data begins to accumulate, more complex model designs typically follow. Cross-sectional designs are effective for establishing the existence of a relationship between two variables, but they are almost useless for establishing its causality. In fact, cross-sectional designs are highly vulnerable to reverse causality and endogeneity biases.

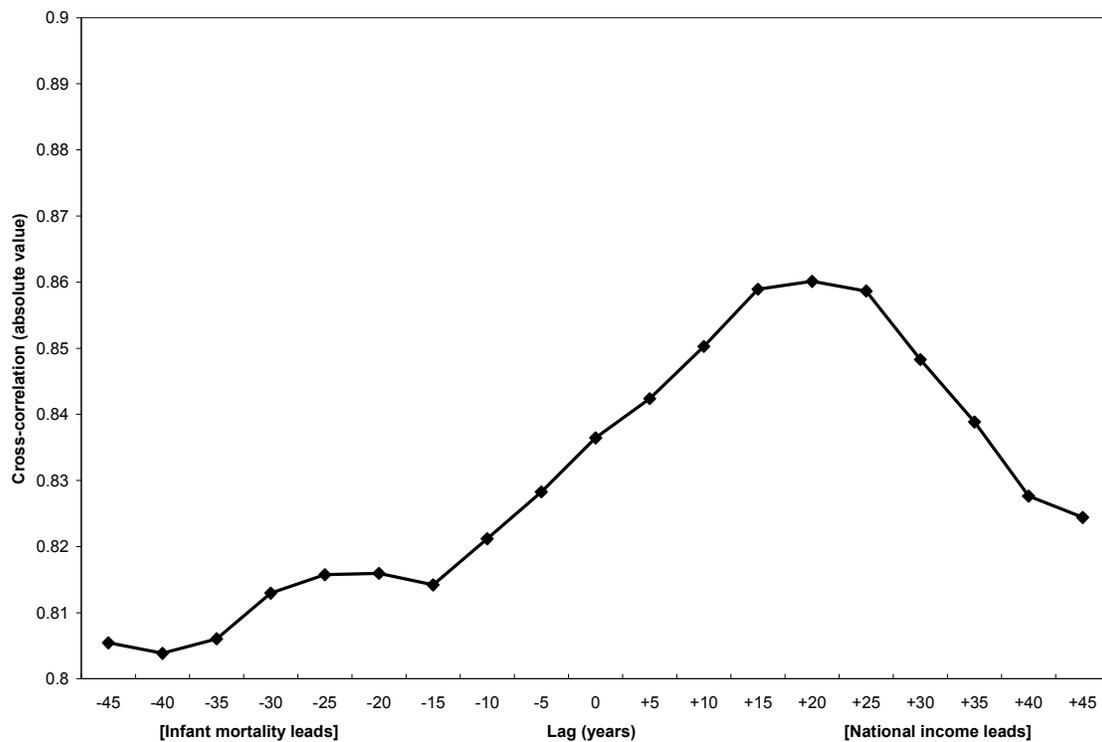
It has been broadly accepted for at least three decades that three conditions must be met to establish the causality of a relationship between two variables: correlation, temporal precedence, and non-spuriousness (Kenny 1979). Establishing correlation is usually not a problem in cross-sectional models. Temporal precedence and non-spuriousness, however, can be much more difficult to establish.

The temporal order governing the relationship between two variables is sometimes obvious (as with national income and infant mortality). At other times it can be established empirically through investigation of the lag structure connecting them. As an illustration, Figure 4 plots the lag structure of the correlation between national income and infant mortality. The maximum correlation between the two variables occurs when national income is compared with infant mortality rates 20 years later. This is strong circumstantial evidence that national income causally precedes infant mortality, though it does not eliminate the possibility that reverse causality occurs as well.

Note that simply lagging the dependent variable by a year (or more) is not sufficient to establish the temporal precedence of the independent variable, since most QMCR variables are highly autocorrelated within countries. Thus, for example, the regression of national income in

2000 on infant mortality in 1995 would not ensure the temporal precedence of infant mortality in the analysis, since infant mortality in 1995 is also a very good proxy for infant mortality in, say, 2005. This is obvious from Figure 4. In fact, infant mortality is strongly correlated ($r > .80$) with national income over 40 years later! A regression of national income in 2000 on infant mortality in 1960 would produce highly significant results without in any way establishing the causal precedence of infant mortality. Nonetheless, short- and long-term lags (sometimes as short at one year) have long been (incorrectly) claimed to establish temporal precedence in QMCR.

Figure 4: Lag Structure of the Relationship between National Income and Infant Mortality, Demonstrating the Temporal Precedence of National Income



An alternative use of lags to establish temporal precedence is implemented in the confusingly-monikered "panel" or lagged dependent variable (LDV) design. In LDV models, the dependent variable is measured at a long time lag (often decades) after the independent variables, which include among them an early realization of the dependent variable itself. The dependent variable is thus regressed on both itself and a set of independent variables all measured at an earlier time period. Note that this is a very different usage from short-term LDV designs used in the political science literature to control for error autocorrelation. The LDV design "provides an estimate of the effect of the independent variable which is 'independent' of variance in the dependent variable" (Chase-Dunn 1975:726-727). Though the LDV design has been strongly criticized for not eliminating the possibility of spurious causation (Firebaugh and Beck 1994), it

is effective at establishing precedence, since any (contemporaneous) reverse causality of the independent variable on the dependent variables is partialled out. The main pitfall of LDV models is that they are often misinterpreted.

For example, when national income is the dependent variable, the LDV model is often mis-read as a model for growth, when it is in reality it is only a model for national income. Since income, not growth, is the dependent variable, the model only establishes the temporal precedence of the independent variables vis-à-vis national income, not vis-à-vis growth. This may seem a pedantic distinction, but it is important to keep in mind. Imagine the existence of a panel of countries starting with identical national income and foreign capital penetration levels at Time 0. Some subsequently grow at a fast rate, while others grow at a slow rate, with growth rates highly stable over time. Assume that fast growth leads causally to reductions in foreign capital penetration. At each subsequent time point (1, 2, ...) national income would come to be ever more (negatively) correlated with foreign capital penetration. Regressing national income at Time 2 on foreign capital penetration at Time 1 would give a negative coefficient for penetration, even controlling for national income at Time 1. Though loading negatively on income, however, foreign capital penetration in such a scenario would have no causal effect on growth. Temporally preceding income is not the same as temporally preceding growth.

Another approach to establishing temporal precedence is the use of instrumental variables. Instrumental variables are clearly exogenous variables that are correlated with the dependent variable at least in part through their relationship with the independent variable, with no possibility of reverse causation. Thus, the correlation between the instrumental variables and the dependent variable can be used as evidence of the directionality of the relationship between the independent variable and the dependent variable. Instrumental variables can be used either in a structural equation modeling setting (for example, Kentor 2001) or a two-stage least-squares setting (for example, You and Khagram 2005). Instrumental variables usually pertain to time periods well before the study period (to ensure their exogeneity). In a famously creative (perhaps even notorious) example, Acemoglu, Johnson and Robinson (2001) use nineteenth century colonial settler mortality as an instrument for the strength of civil society today. The main shortcoming with the instrumental approach is the difficulty of finding good instruments.

Ensuring non-spuriousness, however, presents far greater difficulties. It is often claimed that instrumental variables can be used to establish non-spuriousness, but this is only true under the assumption that instrumental variables can only affect the dependent variable through the independent variable, not via any other causal path through any potential (and potentially unmeasured) common-cause variable (Angrist and Krueger 2001). This assumption is so heroic as to be meaningless. The root problem is that any observed correlation between two variables A and B could actually be the result of their common correlation with an omitted variable C. Such a situation results in "omitted variable bias": the observed correlation between A and B becomes larger than the true effect of A on B. Of course, it is more likely that instead of one there are many omitted common-cause variables that are responsible for the relationships observed in the data. It can even be argued that nearly all observed relationships are spurious at their root, and only approximately causal. For example, national income as such almost certainly doesn't "cause" infant mortality. Instead, the individual and societal wealth and productivity that lead to national income also lead to infant mortality. It is the near-identification of national income with "wealth," "productivity," "development," and the like that makes the statement "national income causes infant mortality" reasonable, though only as an approximation.

There are two general methods of establishing non-spuriousness in cross-sectional models. One is to control for all potential common-cause variables. This is difficult to accomplish in practice, for several reasons. First, actual data for the necessary variables may not exist. For example, human capital may be a common cause of national income and infant mortality, but the closest we come to measuring human capital is education data, which really don't adequately capture the concept. Second, there may be too many potential common-cause variables to test them all with the limited number of cases (and thus degrees of freedom) available. Third, potential common cause variables may simply go unnoticed. These difficulties make it impossible to guarantee non-spuriousness through the use of appropriate statistical controls, though a credible case for non-spuriousness might still be made. For example, if controlling for the most highly suspect common-cause variables has little effect on the proposed causal relationship, it can be argued that other, unconsidered potential common-cause variables would be unlikely to account for the observed relationship, even though they are not tested.

The other is to use fully longitudinal data (for both the dependent and independent variables) to eliminate at least some pathways through which a spurious relationship may arise between the variables of interest. Two classes of longitudinal models used in QMCR are multilevel models and difference models. Both are designed to account specifically for spuriousness due to the presence of time-invariant omitted variables. This is a very large, though not exhaustive class of potential common-cause variables, including any factor relating to a case that is constant across the study period. For example, countries' political geographies, topographies, climates, cultures, economic systems, and forms of government are all typical time-invariant (or nearly time-invariant) variables. Multilevel models and difference models both control for time-invariant omitted variables by focusing their statistical power on changes over time in the variables of interest within countries. This eliminates the effects of all time-invariant factors, since within countries they, by definition, do not change over time.

The cost in statistical power of doing this is, however, very steep, since multilevel models and difference models sacrifice the power to make inferences based on cross-national variation in the overall levels of the variables of interest. Still, the elimination of time-invariant common-cause alternatives makes for a dramatic advance in the causal credibility of purported relationships, which is often worth the accompanying sacrifice in statistical power. Moreover, when relationships can still be shown to be significant even in such low-power models as multilevel models and difference models, they are much more likely to be accepted as robust and important phenomena. Unfortunately, even so they must always remain subject to some degree of skepticism, since there are no off-the-shelf statistical models for ruling out spuriousness arising from omitted common-cause variables that do vary over time.

MULTILEVEL MODELS

In 1994, Firebaugh and Beck bemoaned the fact that "cross-national research in sociology currently is dominated" by lagged dependent variable models, which "are so common in cross-national research in sociology that practitioners refer to them as 'panel analyses'" (637-638), by which they meant that LDV designs had become synonymous with the analysis of panel data. Today, the same might be said for multilevel models (MLMs), sometimes also called hierarchical or (now) panel models, which have become so ubiquitous in QMCR over the past ten years that it

could be argued that there exists an a priori assumption that they should be used in all cases where it is possible to do so. Critics and reviewers often demand MLM evidence even where sufficient longitudinal data do not exist with which to estimate a multilevel model. Perhaps partially as a result, MLMs are very often applied in ways that are entirely inappropriate to the data and research questions at hand. Statistical handbooks often serve to compound this problem, since they are generally written from the standpoint of users with very different analytical objectives than those found in QMCR.

The MLM design was initially developed for use in experimental settings, and is essentially an ANOVA (analysis of variance) model with covariates. In a standard ANOVA model, subjects are divided into groups, with each group receiving a different treatment. The ANOVA F test indicates whether or not outcomes for the subjects as a whole differ significantly across the treatment groups. Even in cases where there are no significant differences in response between specific pairs of groups, the overall ANOVA F test may detect significant differences among all groups analyzed collectively. The garden-variety ANOVA model is a multilevel design because error is introduced to the model at two distinct levels. There is a Level 1 sub-model, in which the subject's outcome is influenced both by participation in an experimental group and by random error idiosyncratic to the subject, and a Level 2 sub-model for the effect of participation in a group (which in this trivial case is an array of constants). This simple ANOVA model is an example of a fixed effects model (FEM), since the treatment effects are fixed (modeled without error). In a random effects model (REM), both the treatment effects and the individual subject outcomes are subject to random error.

The fundamental difference between FEMs and REMs is in how the error is apportioned, to the subject or to the treatment. In the simple one-way ANOVA setting these two sources of error cannot be distinguished, so the FEM and REM specifications yield identical estimates of the treatment effects. In less trivial models, however, the question of fixed versus random effects becomes important. For example, if the Level 1 sub-model includes covariates in addition to the treatment effects, the apportionment of error between Level 1 and Level 2 of the model helps determine the standard errors of the coefficients of these variables. From the standpoint of the significance of the Level 1 covariates, FEMs are generally more conservative than REMs. Conversely, from the standpoint of the significance of the Level 2 treatment effects, REMs are generally more conservative. In fact, REMs were developed for precisely this reason: in many situations, FEMs produce upwardly biased estimates of the effectiveness of Level 2 treatments. Where group treatment effects are the primary interest of modeling (as in drug trials), the REM design is preferred because it is both better-specified and more conservative than the FEM. In drug trials the primary interest is in the sizes of the group effects, not in the covariates, which are included simply as controls.

An early social science application for the REM was school effectiveness research (Rumberger and Palardy 2004). In a typical school effectiveness research setting, students (subjects) are arranged into schools (treatments) to study the effect of these treatments on the students' standardized test scores (outcomes). A simple one-way ANOVA of school means overstates the effect of schools as treatments, since their student populations may be heterogeneous on the dimensions that affect student performance (for example, student ability, family resources, family structure, etc.). Estimates of school effects are reduced by including student and family variables as covariates in a Level 1 equation for individual student performance and estimating an FEM instead of a simple ANOVA model. In the FEM design,

schools take credit for all of the cross-school differences in student performance remaining after controlling for individual student attributes. This is a problem, though, because some of the school differences are themselves due to individual student attributes: after all, students (or their families) can, to some extent, choose what schools they attend (for example, though choosing to live in expensive neighborhoods). In any situation in which the subjects choose their own treatments, the effectiveness of those treatments may be overstated. The REM corrects for this bias by making the choice of treatment – in this case, which school is attended by the student – endogenous to the model.

Both FEM and REM variants of MLM designs are used in QMCR models. Although the mathematics for these models are identical to the mathematics used in experimental and quasi-experimental research, the focus in QMCR is entirely different. In QMCR, the notional "treatment" groups are countries and the "subjects" are country-years of observation. For example, in the illustrative data used below on infant mortality and national income, there are a total of 1320 distinct observations arising from 208 countries observed over ten time points (5-year intervals 1960-2005; not all countries report data for all time periods). There are thus 208 possible "treatments" (countries), each of which is experienced by up to ten "subjects" (country-years) per country. In experimental designs and quasi-experimental settings like school effectiveness research, the researchers' primary interest is in the group treatment effects. In QMCR, however, the treatment (country) effects are unimportant; only the covariates (independent variables) matter.

The appeal of MLM designs for QMCR is that in studying the effects of the covariates (independent variables), the effects of the treatments (countries) are controlled for. In controlling for country, the MLM implicitly controls for any factor that does not differ by country: i.e., all variables that are time-invariant within countries over the course of the study period. Thus, all time-invariant common-cause variables that might give rise to a spurious correlation between the dependent variable and the independent variables of interest are implicitly accounted for. This doesn't entirely establish the non-spuriousness of the relationship of interest, but it does go a long way toward eliminating plausible alternatives.

The FEM design absolutely eliminates any time-invariant alternative explanations of the dependent variable, period. The REM design, however, does not. In the REM design, only a portion of the treatment (country) effect is assigned to the country itself, since the REM design assumes that selection into treatments (countries) is endogenous, not assigned exogenously. Just how much of the country-level variability remains is a bit of a mystery, since REMs were not designed for this purpose. Empirically, it seems that very little is left over: REMs usually give very similar results to the equivalent FEMs. Why, then, does anyone use them?

The REM design is used because REMs allow the estimation of the effects of covariates even if those covariates do not change over time. To my knowledge, this is not done in drugs or schools research using REM designs, where the focus of the research is on the group treatment effect, not on the covariates. In QMCR, however, we are primarily interested in the covariates, not the group effects, and often the covariates do not vary for particular treatment groups. For example, countries' latitudes are constant over time. Estimates of the effect of latitude on QMCR outcomes cannot be estimated in an FEM design because the fixed effect of "country" subsumes everything unchanging about a country. In a REM, however, not all of the country effects are assigned to the countries themselves, leaving some variance left over that can be assigned to time-invariant variables like latitude. Exactly how much of the between-group variance in REMs is

assigned to country effects and how much to time-invariant covariates is, to my knowledge, unknown. It is not, in principle, unknowable, but REMs simply weren't designed for the eventuality that users might try to estimate the effects of time-invariant covariates in models treating countries as if they were endogenously chosen as treatments by country-years. Much further research involving Monte Carlo simulation would be necessary before we could understand the meaning and significance of the coefficients in REMs used in QMCR.

From this standpoint, the use of REMs is a case of having one's cake and eating it too (Halaby 2004). If the objective is to eliminate the possibility of spurious causality due to unmeasured time-invariant variables, the FEM design can be used. If the objective is to estimate the effects of measured time-invariant variables, a simple cross-sectional model is the most effective design. The REM design notionally allows both to be done in the same model, but only partially and to unknown degrees. The REM corrects to some extent for unobserved time-invariant variables and allows the estimation to some extent of the effects of measured variables that are time-invariant, with the extent of each determined by the hypothetical degree of endogeneity of the treatment (country) choices exercised by the subjects (country-years), were such choices theoretically possible, which they aren't. Were it not for the convenience that REMs produce some kind of estimate of the effects of time-invariant covariates, it seems doubtful that they would ever be used.

Both kinds of MLM, however, present a much greater challenge where data exhibit trends over time. Analyses using MLMs are highly sensitive to trended data. The MLM design is fantastic for eliminating time-invariant variables, but time itself is, of course, not time-invariant. This fact seems to be poorly understood in the QMCR literature.

The basic logic of MLM designs is that they draw their statistical power from correlating (within countries) the deviations of both the dependent and the independent variables in each period from their overall country means. If the dependent variable tends to be (relatively) high when the independent variable is (relatively) high, and low when the independent variable is low, this is evidence of a relationship between the two. This is in itself a problem: it assumes that there is no lag between changes in an independent variable and consequent effects on the dependent variable. It is highly likely in most QMCR settings that lags are substantial. As a case-in-point, consider the roughly 20-year lag in the effects of national income on infant mortality suggested by Figure 4. Such lags could be incorporated in MLM designs but rarely are. I myself have never seen a published example of QMCR using MLMs that systematically investigated the appropriate lag to be used between the timing of the independent and dependent variables, though some have used pro forma 1 or 5 year lags.

A much bigger problem, however, is that many QMCR data series are strongly time-trended. National income, for example, can be correlated up to $r = .98$ with time (Babones 2007). Infant mortality, on the other hand, has generally fallen over time. In an MLM design without time adjustments, national income and infant mortality appear to be closely related because in years when national income is higher than average (the later years in a four-decade study), infant mortality will tend to be low, and vice versa. The two variables may in fact be related (and in this case almost certainly are), but the point is that any two time-trended variables will appear to be related in MLMs, even when they're not. Even worse, QMCR variables are time-trended at different rates in different countries. National income rises strongly over time in South Korea (which has been growing rapidly since 1960), moderately in the United States (which has been growing less rapidly), and slowly in Ghana (which has hardly grown at all). Moreover these time

trends are not even constant over long periods within countries: China's national income growth was low in the period 1960-1980, moderately high in the period 1980-1995, and very high in the period 1995-2005. Similarly, most developed country national income series inflect in the mid-1970s, and formerly Communist country series around 1990. On top of all this, there are short-term business cycles. Time effects are everywhere.

Such time trends wreck havoc on the estimation of MLM coefficients, and are very difficult to account for through the explicit modeling of error structures. Period effects, time covariates, and autoregressive error structure corrections are all inadequate to account for the kinds of time trends inherent in QMCR data structures. The fact that QMCR variables are trended at different rates in different countries (and at different times) means that time trends must be dealt with on a country-specific basis, and sometimes in complex ways even within countries. One-size-fits-all controls of the kind typically found in QMCR completely fail to adequately adjust for the effects of trended data. As a result, the coefficients on independent variables estimated based on QMCR data are, in almost all cases, heavily biased. It is quite possible that most QMCR studies based on cointegrated or highly trended variables that have used MLM designs have done nothing more than model time. This possibility can be illustrated with an applied example.

Infant mortality is well-known to be closely related to national income per capita (Ross 2006; Babones 2008). They are certainly highly correlated ($r = -.84$). There is every reason to believe that this relationship is causal: higher national incomes allow countries to purchase improved maternal and newborn health. The cross-sectional estimate derived above in Models 3 and 4 for the relationship between national income on infant mortality was $b = -.662$, with $SE_b = .040$. The statistical significance of this relationship is astronomical. National income and infant mortality are about as closely related as any QMCR variables can be.

Nonetheless, a typical MLM of the relationship between national income and infant mortality should yield a non-significant or marginally-significant result. Why? Because we know logically (and, based on Figure 4, empirically) that any causal relationship between national income and infant mortality is not primarily contemporaneous. It is highly unlikely that changes in national income could instantaneously reduce infant mortality. Therefore, wherever national income exhibits a strongly significant contemporaneous effect, we can be reasonably sure that it is due to the strong correlation of both national income and infant mortality with time (or some other common-cause variable), not due to any contemporaneous causal relationship between the variables themselves. In short, the contemporaneous correlation between national income and infant mortality is spurious.

The results of a series of MLM estimates of the contemporaneous relationship between national income and infant mortality are reported in Table 2. A series of commonly used MLM time-trend corrections have been implemented. In particular, five distinct model configurations are considered:

Model 5: GDP is a covariate;

Model 6: GDP covariate plus fixed effect period dummies (early/late);

Model 7: GDP covariate plus fixed effects for each of the 10 time points;

Model 8: GDP covariate plus a continuous time covariate;

Model 9: GDP covariate, both GDP and IM detrended for each country.

Each of these model configurations is estimated in four variants:

(A) a model with fixed country effects with unstructured errors;

- (B) a model with fixed country effects with an AR(1) error structure;
- (C) a model with random country effects with unstructured errors;
- (D) a model with random country effects with an AR(1) error structure.

Only the coefficient of interest (that for national income) and its standard error are reported for each model. All models are estimated on the same unbalanced panel of 1320 cases (country-years spanning the period 1960-2005 at five-year intervals).

Table 2: Comparison of MLMs for Infant Mortality (log), 1960-2005

Model	Configuration	Fixed effects for country		Random effects for country	
		Unstructured (A)	AR(1) structure (B)	Unstructured (C)	AR(1) structure (D)
(5)	GDP	-0.917 (0.027)	-0.452 (0.024)	-0.723 (0.021)	-0.454 (0.020)
(6)	GDP, Period dummy (early/late FE)	-0.586 (0.022)	-0.465 (0.022)	-0.571 (0.017)	-0.456 (0.018)
(7)	GDP, Time (FE)	-0.392 (0.020)	-0.189 (0.018)	-0.465 (0.016)	-0.238 (0.016)
(8)	GDP, Time (covariate)	-0.367 (0.020)	-0.162 (0.017)	-0.449 (0.016)	-0.206 (0.016)
(9)	GDP (both GDP and IM detrended)	-0.026 (0.019)	-0.034 (0.016)	-0.026 (0.018)	-0.035 (0.016)

Notes: Entries in table are metric coefficients (standard errors in parentheses); N=1320

As would be expected, the coefficients for national income are in all cases more significant in the REM variant than in the corresponding FEM, though they are broadly similar in magnitude. This is because the REM design assigns less of the total variability in infant mortality to the country (treatment group) effects and thus more to the variable of interest. This is an example of how the use of REMs can lead to biases in the estimated effects of covariates. The discussion of trend effects to follow focuses on the FEM variants, but the same patterns apply in the REM variants.

The initial model, Model 5(A), shows a fantastically strong relationship between national income and infant mortality ($t = -33.599$); this is not surprising, since Model 5(A) includes no trend adjustment whatsoever. Allowing for just an AR(1) error autocorrelation in Model 5(B) cuts the estimated effect of national income in half, but still leaves it extraordinarily highly significant ($t = -18.875$). A popular method of adjusting for trends in QMCR data is to use a period dummy; Models 6(A) and 6(B) show that splitting the observations here into two groups (1960-1975, 1980-2005) has virtually no effect on the estimated coefficient for national income. Even using

ten period dummies (one for each time period) has only a minor effect, marginally reducing the coefficients for national income in Models 7(A) and 7(B) but leaving them nonetheless highly significant.

Model 8(A) introduces a linear time covariate; combined with an AR(1) error structure as in Model 8(B) this is a more aggressive trend adjustment than any typically found in the QMCR literature. Even though coefficient for national income reaches its minimum significance yet in Model 8(B), it is still very highly significant. The t-statistic for this model ($t = -9.277$) corresponds to a significance level of $p < 0.00000000000000000001$. All of these models would lead one to conclude that there is a strong contemporaneous relationship between national income and infant mortality. Were they published in the literature, it is unlikely that anyone would question the results, since it seems on the face of it that the two variables should be related. In fact, however, as I have argued above they are not -- at least, not contemporaneously.

Model 9(A) demonstrates this. In Model 9(A) the linear time trends in national income and infant mortality have been removed on a country-by-country basis before analysis. Thus, in Model 9(A) we're truly asking whether infant mortality has been higher than average in years that national income has been higher than average, leaving aside the secular trends in each. In Model 9(A) the FEM estimate of the contemporaneous effect of national income on infant mortality is negative, but weak and non-significant ($t = -1.403$). Model 9(B) shows the corresponding estimate when allowing for an AR(1) error structure. It is slightly larger, and weakly significant ($t = -2.085$). The staggeringly significant cross-sectional correlation between national income and infant mortality all but disappears in a MLM framework when time trends are eliminated.

The coefficients in Model 9 aren't weak because of some legerdemain in the detrending process; they are weak because the real contemporaneous effect of national income on infant mortality is so weak as to be almost undetectable. Even the small effects that do remain in Model 9 are probably due to imperfect detrending resulting from the fact that the actual time trends in national income and infant mortality may not be linear. The fact that the AR(1) error model improves the significance of the relationship is evidence for this interpretation, since the AR(1) model would allow for any residual trend to decay over time. That is to say: if both series trended until 1990 then flattened out, the linear detrending of the data would actually create a small trend for the period after 1990. An AR(1) error model would better capture this change in trend than would an unstructured error model.

If such a strong effect as that of national income on infant mortality ($t = -33.599$) disappears when time is removed from the analysis, what of the robustness of other, far more tenuous QMCR relationships? Nearly all QMCR variables trend over time, at least within countries. National income, income inequality, carbon emissions, population health, birth rates, educational levels, industrial output, agricultural employment, unionization, labor productivity, portfolio investment, investment dependence, political freedoms, military spending, tax efficiency, memberships in international organizations, frequencies of protest events, and the like all exhibit secular time trends within countries. As with national income and infant mortality, it is likely that their observed relationships in MLMs are partially or largely the result of inadequate controls for time.

The simple fact is that MLM designs are generally inappropriate for answering the questions that most QMCR practitioners want to ask. They are much better suited to studying the kinds of time-series data structures for which they are commonly used in the economics literature. The basic question asked by MLMs applied to repeated-measures panel data – do X and Y rise

and fall together at the same time? – is not a question typically asked by scholars working in the QMCR tradition. Most QMCR is concerned with long-term changes in structural relationships, not short-term fluctuations in annual data. Accordingly, models for QMCR should focus on broad changes over long time periods, not on period-to-period variability, as in MLM designs. It is difficult to imagine a scenario in which QMCR practitioners using MLMs are actually interested in the annual variability in their data. I have never seen such an example published in the QMCR literature.

DIFFERENCE MODELS

Rather than MLMs, Firebaugh and Beck (1994) promoted the use of difference models as an alternative to "panel" models for eliminating spurious causality. In a difference model change in the dependent variable over time is regressed on change in the independent variables. The reasoning is that if variables systematically rise together and fall together, they are related in some way. Any time-invariant covariates of the dependent and independent variables can be safely omitted, since their own difference scores over time will be zero (by construction). Time itself can also be omitted as a confounding influence, since the time difference between the two periods being studied is a constant for all countries in the analysis. The difference model thus at a stroke solves both the time-invariant omitted variable problem inherent in panel designs and the time trend problem inherent in MLM designs.

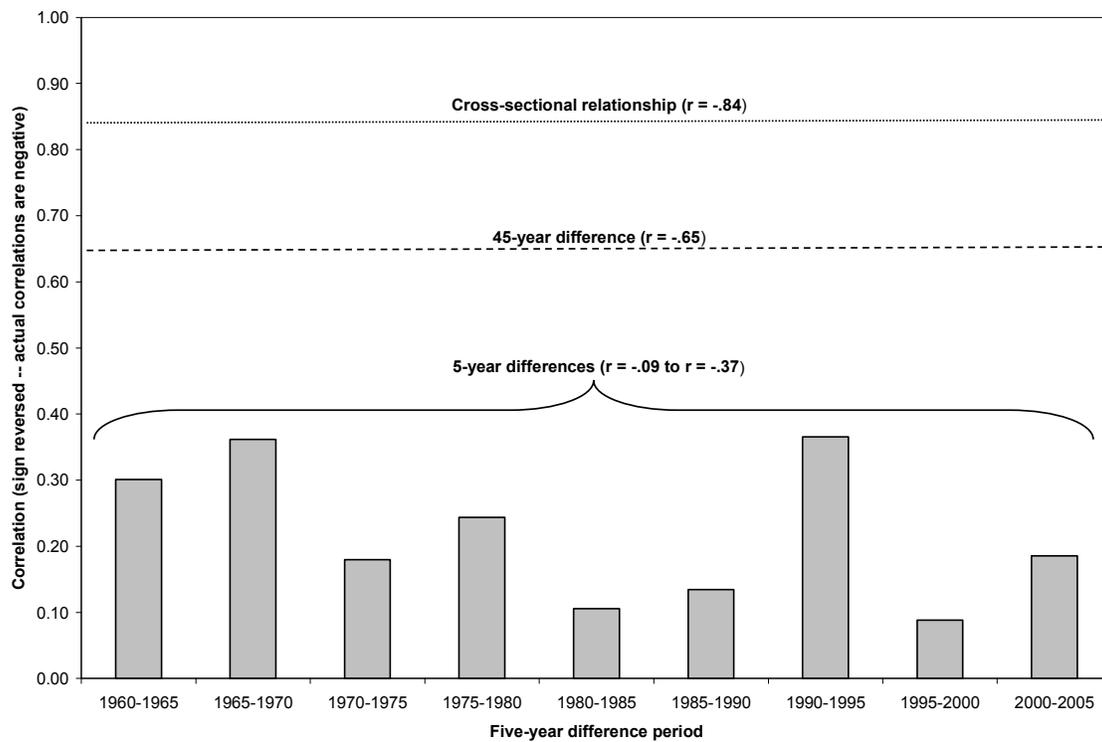
Since variables in the difference model are differenced at the country level, it doesn't (much) matter whether or not variables trend at different rates in different countries. When variables trend at different rates in different countries in the same way across variables (e.g. both slow in Country A but both fast in Country B), trends have no effect on the results. When variables trend in different ways across variables (e.g., the first variable fast and the second variable slow in Country A, but the first variable slow and the second variable fast in Country B), the pattern of trends will reduce the observed relationship between the variables. Thus, difference models are generally conservative from the perspective of time trends.

The price of these advantages is low power and poor data availability. The difference model approach is only effective when the differences can be computed over relatively long time periods (i.e., long enough for meaningful changes to occur in the variables being studied), meaning that often relatively small numbers of cases are available for analysis. The long time periods studied, however, ensure that even lagged relationships can be captured in difference models, since a study period of several decades will encompass the lag periods of most relationships. It is, however, possible to study differences of shorter time periods if desired. It is even possible to construct MLMs out of many short difference periods, though such designs would require extreme care, given their complexity.

A widely quoted myth about the difference model is that it is equivalent to a FEM design with two time periods. As correctly noted by Halaby (2004:515), this is only true when the independent variable in the difference model is a binary (0/1) variable. In nearly all QMCR settings of interest, both independent and dependent variables are continuous. In such cases, the difference model and the FEM can give widely varying results, especially when both the independent and dependent variables are trended over time.

Applying a 45-year difference model (1960-2005) to the regression of infant mortality on national income yields a highly significant effect of $b = -0.589$ ($SE_b = 0.074$) based on $N=88$ cases. This means that long-term increases in national income are strongly related to long-term decreases in infant mortality. This coefficient is not far off that reported in Model 6(A), but the similarity is purely coincidental. The coefficient in Model 6(A) has been shown to have been generated by the time trends in the data; the coefficient for the difference model is unaffected by such trends. In fact, a difference model based on the detrended data differs only in the constant term; the slopes is mathematically identical. The difference model thus clearly and robustly indicates a non-artefactual relationship between national income and infant mortality, independent of potential time-invariant common-cause factors and independent of time trends in the variables. The rate of improvement in national income is strongly related to the rate of improvement in infant mortality across countries, independent of constant country characteristics and independent of the fact that both have generally improved over time.

Figure 5: Attenuation and Variability of Difference Model Results using Short Time Intervals

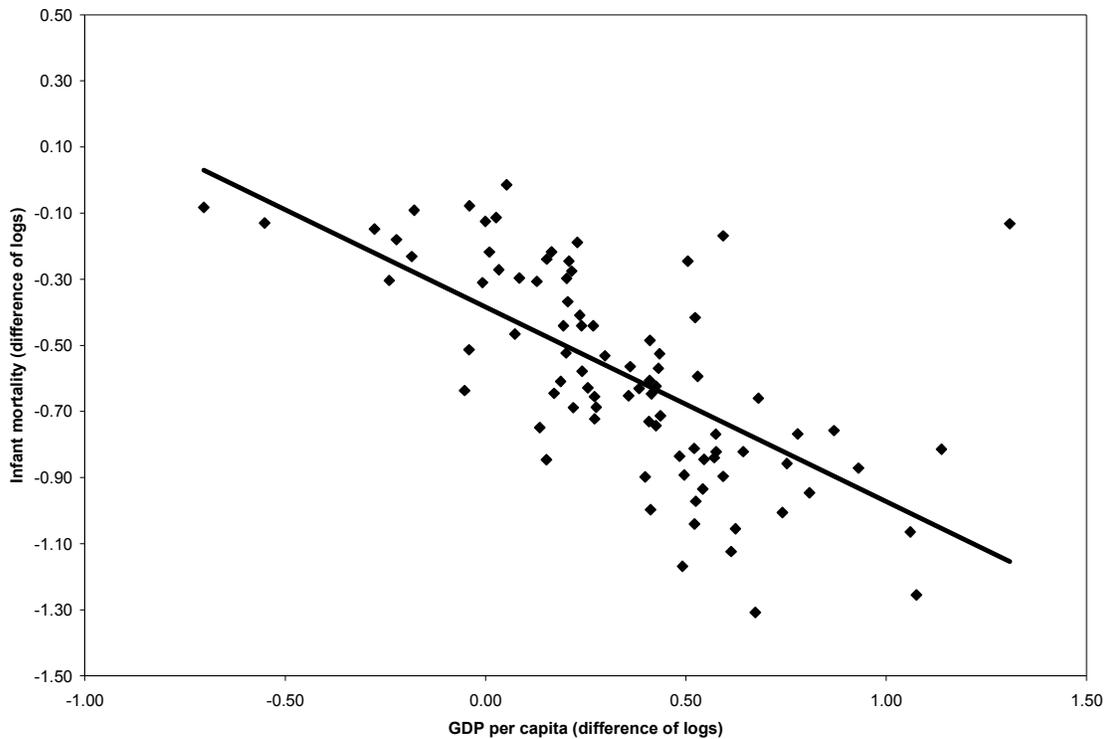


Difference models are not common in sociology, though Firebaugh and Beck (1994) make a strong case for their use. As Firebaugh and Beck argue, the difference model is the direct longitudinal analog of the straightforward cross-sectional model. In a properly-specified cross-sectional model, a dependent variable is regressed on dependent variables that are thought to influence the dependent variable. This model implies that changes in the independent variables

should be reflected in changes in the dependent variable. The difference model captures this expected relationship. The key is that the difference model must be estimated over a sufficiently long time period for changes in the independent variables to have an opportunity to manifest their effects on the dependent variable. If the time period over which the variables are differenced is long enough, causal relationships can be detected even when the causal lags are long, as with the relationship between national income and infant mortality.

Differences over short time periods do not adequately capture the over-time relationship between changes in national income and changes in infant mortality, despite the very strong cross-sectional relationship between the variables. For example, the correlation between changes in national income and changes in infant mortality over the 45-year interval examined above is $r = -.65$. This does not quite reach the $r = .84$ recorded in the pooled cross-sectional design, but it is still reasonably strong and highly significant statistically. When national income and infant mortality are differenced over 5-year intervals, however, their correlation drops substantially. Their correlations over the nine five-year intervals 1960-2005 are plotted in Figure 5. They range from a high of $r = -.37$ for the 1990-1995 difference model to a low of $r = -.09$ in the period immediately following. Out of the nine time intervals studied, the correlations in four are not statistically significant at the $p < .05$ level. Three are not significant even at the more relaxed $p < .10$ level.

Figure 6: Difference Model (1960-2005) Regressing Changes in Infant Mortality (log) on Changes in National Income (log)



Even for such a powerful relationship as that between national income and infant mortality, reasonably long time intervals are called for. Note that simply pooling all available 5-year intervals does not have the same effect as using a 40-year interval: the correlation of all available 5-year differences of national income with all available 5-year differences of infant mortality is just $r = -.16$ ($N=974$). This is a relatively small correlation, though highly significant due to the large number of intervals aggregated.

Some analysts argue that difference models are wasteful in that they ignore large amounts of data that could be analyzed. The data thrown out in difference models, though, are for the most part data that are analytically irrelevant to the problem at hand. Viewed in this way, difference models are not wasteful but parsimonious. The one major practical shortcoming of difference models is that the differencing process often generates outliers and leverage points. The 45-year difference in logged infant mortality is plotted against the 45-year difference in logged national income per capita in Figure 6. There is one obvious leverage point in the data, Botswana, in which national income grew rapidly over the study period but infant mortality declined only slowly. Deleting Botswana from the model yields an even stronger estimate of the effect of national income of $b = -0.695$ ($SE_b = 0.067$). Dealing with leverage points in difference models is no more nor less difficult than dealing with leverage points generally.

RECOMMENDATIONS AND CONCLUSIONS

I conclude with a plea for simplicity. Quantitative macro-comparative research should focus on the effects of as few variables as possible in any one study. Control variables should be used judiciously, with explicit attention given to their actual roles in the models being estimated. It may be possible to estimate the coefficients of twelve variables using data on just twenty countries observed at five time points each, but this doesn't mean that it's advisable. This paper has paid less attention to the virtues parsimony than I might have liked, but hopefully it will be clear from the challenges examined here of correctly understanding the relationship between just two variables that the challenges of correctly understanding the relationships among dozens of variables might be near-insurmountable.

The data used in quantitative macro-comparative research are highly structured in idiosyncratic ways that create many pitfalls for those who analyze them. Obvious and hidden temporal effects are embedded everywhere in QMCR data structures. From this perspective, the difference model is an extremely effective design for QMCR because it eliminates the problem of (linear) time trends in variables and returns analysis full-circle back to a simple cross-sectional model (albeit a cross-sectional model of changes over time). Consequently, it is hard to mess up a difference model. Simple cross-sectional models, including appropriate control variables, should be estimated first, then difference models run as back-ups to help substantiate any claims of causality.

Multilevel models, on the other hand, should be approached with extreme caution. It is my considered opinion that it is quite possible that all reported multilevel model results reported to date in the quantitative macro-comparative research literature are nothing more than spuriously attributed effects of time. I have not explicitly replicated existing MLMs from the published literature, and so I cannot comment on them directly. Published results represent only the tip of the iceberg of the generally careful and comprehensive analyses that underlie any published work.

Researchers who have used these models in the past may in fact have privately tested detrended versions of their analyses, found that the results simply confirmed what they had found in other models, and as a result not considered them publication-worthy. In light of the evidence presented in this paper, however, anyone using such models in the future should certainly examine the possibility that their results are driven by nothing more than trends in their data.

The MLM design, especially in its REM variant, is extremely seductive because it offers boxes with labels for almost everything anyone might want to do in QMCR: control for omitted variables, estimate the effects of time-invariant variables, estimate interaction effects, account for the effects of time, etc. The problem is that the labels on the boxes often bear no intuitive relationship to what the boxes actually do. Researchers should be extraordinarily careful when using MLM designs to assure both themselves and their readers that the coefficients attached to the variables of interest actually mean what they purport to mean. A productive avenue for future research might be the application of Monte Carlo techniques to simulate the behavior of the coefficients of covariates and their standard errors in QMCR using REM designs.

The methodological challenge of undertaking quantitative macro-comparative research is a large part of what makes it exciting and intellectually stimulating. The ultimate reward for most QMCR practitioners, though, is the possibility of changing the world through better understanding how it works. Fortunately or unfortunately, we only have one world to work with, so the burden of better understanding that world largely falls back on methodology. Exculpatory data are rarely forthcoming, so we are generally constrained to argue, rather than experiment or survey, our way out of our problems. Nonetheless, it is important to remember that methodological virtuosity should be exhibited only in the service of improved substantive understanding, and not for its own sake. There is no need for an extensive toolbox when a hammer will do just fine.

Critics and reviewers especially should keep this in mind. A paper that effectively makes its substantive case with a minimum of complexity should be preferred over one that makes the same case with superfluous virtuosity. Very few practitioners of quantitative macro-comparative research fully grasp the mathematical properties of the error models they implicitly assume in their research. It's even less likely that without hands-on access to the data reviewers are equipped to pass judgment on the suitability of highly complicated models. As the examples presented in this paper illustrate, it's probably just as true of quantitative macro-comparative research as it is of the transwarp drive that "the more they overthink the plumbing, the easier it is to stop up the drain."

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**STUDYING LONG-TERM LARGE-SCALE CHANGE:
CONCLUDING REFLECTIONS ON THE RELEVANT UNIT OF ANALYSIS¹**

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So oft in theologic wars,
The disputants, I ween,
Rail on in utter ignorance
Of what each other mean,
And prate about an Elephant
Not one of them has seen!

-- John Godfrey Saxe
"The Blind Men and the Elephant"

When I first received the papers as discussant for the ASA panel, Methodology for World-Systems Analysis (the papers that would come to constitute this journal's special issue), I didn't quite know how to proceed. In large part, the papers used innovative methodology to answer interesting substantive questions, so quality was not a question. The author's were rigorous in how this methodology was applied, and creative in searching for and manipulating worldwide data, so no need to comment there. The problem was semantic. Not in the "petty dispute over word choice" sense of semantic, but in the ontological relationship between distinctive concepts and their intended (in this case theoretical) meaning.

The motivation for most of the authors in this volume is the extent to which we, as scholars interested in large-scale change, can overcome various methodological problems associated with collecting and analyzing worldwide data, whether quantitative or qualitative in character. Whether we refer to this type of research as "global," "comparative," or by the latest trendy term "transnational," social science is of little semantic (or substantive) consequence. It's all macrosociology in one form or another.

But these motivations are entirely different than methodologically advancement within the theoretical framework that has come to be called "world-systems analysis." While perhaps overlapping, these are not necessarily identical sets of issues. This is not to say that one set of issues is more salient than the other, but it is, I will argue, increasingly important to remember that they *are* different sets of issues, and the difference is more important than a debate over semantics might suggest. In one of his essays that, when taken together, became the major

¹ This essay draws heavily from a forthcoming book, *Unveiling Inequality: A World-Historical Perspective*, co-authored with Roberto Patricio Korzeniewicz.

methodological statements in the world-systems tradition, Terence Hopkins (1982b:149) frames the issue like this:

There is now a growing number of good articles on method which show how...a given method may be made use of in world-systems studies as they understand them. (But a different line of attack is): given the directions that world-system studies are taking, what basic problems of method do they seem to raise? I imagine the two lines of attack will in due course meet and prove complementary, but they are still very far apart now. (Hopkins 1982a:38)

The difference between macro comparative social science in various forms and world-systems analysis in particular can be illustrated by looking at how each frames relevant questions for analyzing inequality, for example. Scholars of the former are currently asking questions like: why do the countries of Latin America have higher levels of inequality than those in say Western Europe? This directs inquiry inward, toward the nation-states themselves, as in: What are the conditions within the countries of Western Europe in contrast to the countries in Latin America that allow the former to have relatively egalitarian income distributions and the others to not? From a world-system perspective, inequality goes from being a condition to a process, and the questions are restated: How did the countries of Western Europe come to occupy the level of inequality they have in the world and the countries of Latin America the level of inequality they have? More interestingly, are the two related? At fundamental issue is the unit of analysis.

PARTS AND WHOLES IN THEORY AND METHOD

In the Indian parable “The Blind Men and the Elephant,” each man touches a different part of the animal, but only one part. When they then compare notes on what they felt, they learn they are in complete disagreement. Each believes they are describing the whole of the elephant, however in reality they are only describing a part of it, thus while each is “in the right” in what they are explaining, each only has a partial understanding of the overall whole. Conceptualizing the relationship between parts and wholes – that is, specifying the relevant unit of analysis – is the key epistemological innovation that distinguishes world-systems analysis from other forms of macro comparative social science.

While much has been written about determining the proper unit of analysis in social scientific study, let’s start with the preposition that at least three criteria are involved, however implicitly to the analyst. First, and primarily, the unit of analysis must have theoretical relevance. As Max Weber (1996 [1905]) argued, choice of the proper unit of analysis should be guided by theoretical criteria, as its boundaries should contain within them all the processes that are relevant for understanding the phenomenon under investigation. Second, the units themselves must exhibit independence from each other. If the unit is not independent of other instances of the processes under investigation, then no new information can be added to test the theory. Third, the units must be neither indivisible nor combinable. If some broader structure is made up of several independent instances of a process, or a process occurs within a space above the level of interest, then the need to divide or combine units could possibly nullify the phenomenon that was under investigation from the start.

For the most part, sociology as a social scientific discipline has developed over the last two centuries without much awareness of the fact that different social problems require different units of analysis. Until very recently, the underlying assumption, embedded in the discipline from its onset, is that the nation-state (or national society) constitutes the crucial and only possible unit of analysis. With reference to the criteria above, the overarching assumption is that society takes place within distinct national borders and over their particular geography; inside these boundaries processes are “internal” and outside them processes are “external.” Nation-states are assumed to be self-evident and discreet social units, independent and indivisible, and therefore comparable for both theoretical and statistical purposes, as given loci within which social change takes place. In many studies, the choice of nations as the privileged unit of analysis is not even theoretically informed, but often driven by a combination of academic custom and the format in which data are most easily available.

Part and parcel of this uncritical attachment to a notion of society as a discreet, bounded, “national” unit is the fact that we also tend to investigate the happenings in a very small percentage of these units – the most wealthy dozen or so – and then export these observations as if this accurately represents the happenings in all of them, or in “society writ large.” In research published in the main journals, for example, it is extremely common to find that studies that draw exclusively on data from the United States (or some combination of the other wealthy nations) seek to make conclusive statements about the universal character of the phenomena they are investigating, with neither a caveat or even acknowledgment of the limited scope of such inquiry. In other words, sociology, but also the contemporary social sciences more broadly, have the curious particularity of claiming to view the world through theoretical perspectives that are purportedly universal, but in fact have been constructed by focusing mainly, in some cases even solely, on the experience of a very small fraction of the world’s population.²

As an example, take the argument that predominated in sociology, via the leading subfield of stratification and mobility studies, for much of the twentieth century: the development of a more complex division of labor entails a shift from a system of stratification based around ascribed characteristics (one’s origin of birth) to one based around achieved characteristics (especially education). Observing processes of social inequality and mobility solely or primarily as they take place within the borders of wealthy countries indeed appears to confirm such a shift, and many studies in the social sciences have been dedicated to documenting, over and over again, such a transition. But the vast majority of these studies draws their observations from a handful of wealthy nations, and deems this to be a sufficient basis to make conclusive statements about social mobility regimes writ large. Such unacknowledged biases are ingrained in the very foundations of much of the social sciences, and continue to permeate the social sciences as they are constructed day to day.

In macro comparative research, then, the tendency is to reify the nation-state, or national societies, as the sole independent unit of analysis. They are independent units whose level of development was determined by the presence or absence of certain conditions. Conceptualized in this way, nation-states are primarily wholes (national societies) that can be combined as parts to form an international system of states, or an international trading regime, and so forth. The simple

² The population of the richest 22 countries accounts for a little less than 13 percent of the world’s total population.

aggregation done in this manner implies that the whole is nothing more than the sum of its parts. Thus, we could translate from the whole to the parts and back again with ease, and therefore no special meaning is given to the social whole (see Bach 1982 for an extended discussion of this point).

For much of the twentieth century, analysis of large-scale social change (or macro comparative research), came to constitute the “modernization paradigm” that became orthodoxy across the post-World War II social sciences. The modernization paradigm was interdisciplinary and encompassed a broad range of social phenomena; two of its components were an evolutionary theory of large-scale social transformation (used to understand economic development) and a functionalist theory of the working of modern societies (which came to dominate mainstream sociology). Both of these components privileged the nation-state as the site within whose boundaries were contained all the fundamental processes needed to understand the phenomena being studied – be it the presence or absence of wealth, long-term changes in social inequality, or patterns of individual social mobility.

For understanding societal change, three fundamental assumptions of the modernization paradigm are of particular importance: 1) modernization is a long-term process of (ultimately progressive) social change that is “internal” (i.e., occurs within the nation-state); 2) this change represents a singular, overarching transition from traditional to modern; and 3) the transition brings homogeneity, as it ultimately results in growing convergence (of incomes, political institutions, systems of meritocracy, and the like). Likewise, it was generally assumed, if at times only implicitly, that modernization entailed the interdependent and simultaneous transformation of the economic (i.e., through industrialization and/or urbanization), the social (i.e., through the growing importance of achievement in shaping stratification), and the political (i.e., through democratization). In this sense, the modernization paradigm carried normative connotations: the process is “good” in that it entails the virtuous co-development of all three spheres of modern organization.

World-system analysis was born in the early 1970s as a direct critique of the modernization paradigm. Not in professing to offer better theories of development or wiser industrialization policies, but as a transformative framework that would constitute a “fundamental protest” against the ways we think we know the world (Wallerstein 2004:xi).² At the very heart of this “protest” was a panoptic reconceptualization of the relevant unit of analysis. World-systems analysts questioned the extent to which national histories, national economies, or national societies really existed. Modern social change, they argued, could not be understood using local, national, or even continental social science. The important questions can only be studied in the context of a historically conceived world-system:

If there is one thing that distinguishes a world-system perspective from any other, it is its insistence that the unit of analysis is a world-system defined in terms of economic processes and links, and not any units defined in terms of juridical,

² Wallerstein (2004:xi) continues the thought by noting that this mode of analysis is also a “reflection of, an expression of, the real protest about deep inequalities of the world-system are so politically central to our current times.”

political, geographical, or other criteria. (Hopkins and Wallerstein, et al. 1982:72)

World-systems analysis is a fundamental reconceptualization of the whole relative to the whole's parts. Processes extend not only beyond nation-state boundaries, but often, irrespective of them. More importantly, these processes are seen as *relational*, so the parts may not be identified independently of their occurrence in the whole, and the whole cannot be reduced to each part. If in macro comparative research, the goal is to analyze the parts and maintain their additive ordering, in world-systems research the goal is to analyze how the whole is constantly being formed and reformed by relations between parts.³ As Hopkins (1982b:149) states it:

I think the methodological directive with which we work is that acting units or agencies can only be thought of as formed and continually reformed, by the relations between them. Perversely, we often think of the relations as only going between the end points, the units or the acting agencies, as if the latter made the relations instead of the relations making the units.

Thus, world-systems analysis can be seen as a theoretical movement that necessarily gave rise to methodological issues in virtue of the theoretical movement, and that for the most part center around the fundamental unit of analysis.⁴ Nation-states are seen as partial institutions of the whole, a singular world-economy. Since the world-economy in this framework is systemic *and* historical, it will remain the same over time while simultaneously changing from one moment to the next. Wallerstein (2004: 22) calls this a “paradox, but not a contradiction,” and puts forward that the fundamental task of world-systems analysis is to successfully deal with this paradox.

It is in this sense that theory and method are one in the same: the world-systems perspective is not a unified “theory,” it is a way of doing social science, an angle of vision. In the Hopkins/Wallerstein formulation, the whole is the capitalist world economy – a single integrated and expansionary system of production generating and reproducing inequality through the axial division of labor between core and peripheral zones. But scholars can conduct systemic analysis without necessarily assuming the all-encompassing world system. Wallerstein and Hopkins presume a “whole” that governs its “parts.” But one can just as easily make comparisons by progressively constructing a whole by bringing in successive parts, in constant relations with themselves. McMichael (1990:386) calls this strategy “incorporated comparison” where the whole emerges as an historical configuration via comparative analysis of parts posited as “moments in a self-forming whole.” In other words, a world-historical perspective means taking the world as whole as the relevant unit of analysis, a methodological program that need not necessarily coincide with the Wallerstein/Hopkins theoretical conceptualization of what this whole looks like.

³ As Hopkins (1982a) explains The object of inquiry becomes sets of *processes* (that are abstract in contrast to the concrete whole), not *conditions*. For example, inequality is produced by relations among the acting units (nation-states), and is not a condition of them.

⁴ In this sense, the “protest” that Wallerstein speaks is also the questioning of the methodological and epistemological assumptions of social science.

RETHINKING INEQUALITY FROM A WORLD-HISTORICAL PERSPECTIVE

Today, the study of social inequality and mobility constitutes a highly fragmented area of inquiry, with very little dialogue between the many subfields of specialization – some focus on between-nation inequality, and others on within-nation, some employ data from a particular part of the world, and others from somewhere else, and so forth. As discussed above, “The Blind Men and the Elephant” metaphor is an apt description of existing studies of inequality and mobility that can be “partly in the right” in describing what they perceive. But they are “in the right” only within the particular boundaries of the sphere they choose to describe, and with the specific scopes (e.g., data, techniques, assumptions) through which observations are made. Hence, various “parts” are adequately described, but what is missing is an account of the whole.

Like other areas of inquiry in the social sciences, inequality and stratification have been conceived primarily as processes that occur within national boundaries. Such a focus has produced a number of influential overarching narratives. One such narrative is that the relative well-being of people is shaped most fundamentally by the capacity of home-grown institutions to promote economic growth and/or equity. Another, that people over time have become more stratified by their relative achievement and effort rather than by the characteristics with which they are born. A third one, a corollary of the other two, is that upward social mobility is fundamentally the outcome of the adoption of better domestic institutions by countries, and/or the acquisition of greater human capital by individuals.

A world-historical perspective calls these narratives into question. In a forthcoming book *Unveiling Inequality: A World-Historical Perspective*, we argue that such a perspective reveals key processes of inequality that are otherwise hidden from nation-state analysis (Korzeniewicz and Moran, Forthcoming). Such a perspective reveals that the institutional arrangements shaping inequality within- and between-countries have always been *relational*, that is simultaneously national and global. In particular, the development of relatively low levels of inequality seen within wealthy countries is directly related to the persistence of very high levels of inequality seen between countries: the former has been sustained through institutional arrangements that limited competitive pressures within wealthy countries, while simultaneously transferring these competitive pressures (and the inequality that goes with them) abroad.

So what appears to be the product of individual achievement and effort in wealthy countries has gone hand-in-hand with global constraints that accentuated and entrenched high levels of inequality between countries. Thus, just as ascribed criteria like race play a key role in forming high levels of inequality within-countries like Brazil and South Africa, such criteria continue to play a fundamental role in sustaining inequality at a global level as well. Over the last two centuries, nationality has become the crucial ascribed characteristic shaping the status of people within global stratification.

From a world-historical perspective, beyond the status that can be acquired by achievement within one’s own country, the key determinant of one’s relative position within a global system of stratification is the relative level of wealth of the country within which one is born. Thus, a much as markets have become “globalized” over recent decades, social stratification and mobility continue to revolve around institutional arrangements that unequally distribute resources on the basis of ascriptive categorical differences. The current uneven distribution of income and wealth in the world today would unlikely exist in the absence of the institutional arrangements that limit access to markets and political rights on the basis of national

borders. In this sense, while it is not the case that the populations of wealthy nations have attained their privileges by making much of the rest of the world poor, we contend that the relative privileges characterizing high-income nations historically required the existence of institutional arrangements ensuring the exclusion of the vast majority of others from access to opportunity.

Such insights are not apparent when focusing on inequality and stratification using nation-states as the unit of analysis. From such a standpoint, each nation is perceived to have wealthier and poorer populations, engaged in negotiation and/or conflict with one another over the distribution of resources, and institutions and political actions have the effect of *either* enhancing or reducing overall levels of inequality and social mobility. Outcomes appear as fairly clear, and limited to the national spaces within which institutions are assumed to operate, and the criteria used for the exclusion of non-national populations comes to be perceived as legitimate and natural (of course, even within a national population, deciding who should be included has been a contested terrain: for example, most social scientists in the United States focused on male workers and employers when thinking about relative levels of equity in the 1950s, but had to rethink the role of interactions between men and women after the 1960s).

In these accounts, our past, present and future are marked by the growing triumph of individual achievement over ascription as the crucial criteria shaping growing opportunity. Such might indeed be the patterns revealed when the field of vision is limited to discrete, “nationally bounded” “societies:” The actual patterns of inequality, stratification and mobility experienced by most people, as well as the persistence of ascriptive criteria as a basis of stratification, only become unveiled when the world as a whole is taken as the proper unit of analysis.

CONCLUSION

In the 1970s, the claims (and terminology) of world-systems scholars were seen as bold, radical, and, some argued even pernicious, interventions into the ways we conduct macro comparative research. Today, we live in a time where something called “globalization” has demonstrated the validity of the world-systems perspective, at least on its face. Does anyone other than politicians running for national office still take seriously an independent “national economy?” Or perhaps it is just that world-system terminology – “world-economy,” “world-market,” even “core and periphery” – has been semantically appropriated for common use? It is this latter scenario that Wallerstein warns of, that using the language “for other, indeed opposite purposes (than those of world-systems analysis)...can cause serious confusion in the general scholarly public, and even worse, may lead to confusion on our own part, thus undermining the tasks we have set for ourselves” (quoted in Arrighi 1999:121).

The importance of semantics surrounding the relevant unit of analysis thus takes two forms. On the one hand, it is important to recognize, and continue to distinguish between, different methodological frameworks used to conduct macro comparative social science. This involves above all, a careful theoretical determination of the relevant unit of analysis. But also can be reflected in the ways in which language is employed, especially in a time when world-systems analysis – at least its terms and major concepts – are being acculturated across the social sciences. Thus, the title of an ASA panel originally called “Methodology for World-Systems Analysis” became a special-issue in this journal called “Methodological Issues in Macro Comparative Research.”

On the other hand, debates surrounding the relevant unit of analysis have much broader implications. As Beck (2007:701) explains, taking the world as a whole as the relevant unit of analysis is not a simple project of learning about others, some sort of open-minded realization: “It is an integral part of our understanding of the reality of ourselves. It is a complex project of unthinking the ontological premises and dualisms of nation-state sociology – such as national and international, internal and external, citizen and alien, us and them.” If we rethink the boundaries that contain the processes relevant to understanding sociological phenomena – that is, rethink the relevant unit of analysis – we can start to “see” things previously “hidden” by nation-state sociology. And this methodological shift directly leads to the rethinking of such boundaries for social action, social activism, and social justice.

Just as shifting the boundaries of the unit of analysis from the nation-state to the world as a whole forces us to reconsider the empirical processes that are relevant to understand existing inequality, the same shift in boundaries makes it incumbent upon us to redefine who is included in the relevant community when morally assessing social justice. Just as we might decide that race or gender should not be criteria limiting access to opportunity, from a world-historical perspective we might challenge the naturalization of national citizenship as a justification for categorical inequality. It becomes upon us to decide whether the life chances of people should be restricted by the blind luck of their place of birth.

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BOOK REVIEWS

Clifton, Judith, Francisco Comin, and Daniel Díaz-Fuentes (eds.) 2007. *Transforming Public Enterprise in Europe and North America*. New York, Palgrave Macmillan. 239 pages. ISBN-13: 978-0-262-68161-2 Paper (\$26.00).

Readers who are concerned about globalization need to study the experience of the privatization of public enterprise in Europe and North America very carefully. What forces cause the nationalization and privatization of firms? Does privatization lead to greater foreign control? Do foreign owned or privatized firms behave differently than locally owned or public ones? Are any of these configurations better or worse for public welfare? If there are differentials in privatization, what explains these, and are these differentials consequential? These are all important questions in the study of the world system.

Answering such a system requires a clear description of the spread of nationalization or privatization – a map of the causes or effects of such differences – and some general explanations about why some firms privatize and others do not. With these elements one could construct a theory of privatization that would be a critical component of world systems theory. This book is not that theory – and does not intend to be that theory. It is the first step in the construction of that theory. Nearly all books on comparative capitalism, be they about comparative labor relations, comparative democracy or comparative ethnic relations, need to start with a simple laying out of the facts regarding the nations of advanced capitalism. The first books in the field tend to be edited collections of chapters – with one chapter per nation describing the dependent variable – in this case the balance between public and private enterprise.

Like other books of this type, there are a lot of facts to get on the table. This book requires no fewer than fourteen chapters to do a basic description of the balance between public and private enterprise in various capitalist nations. Fourteen chapters is obviously just a beginning, since there are no treatments of Japan, Switzerland, the Netherlands, Israel, or some of the wealthier Eastern European nations such as Slovenia. That said, fourteen is a lot for a collection of this kind. The chapters are written by different authors – and it is notoriously hard to get authors in a collection to follow a standardized formula. In comparison with other collections, this is fairly disciplined with the chapters more or less covering similar materials and addressing similar schemes.

The reader should not expect a generalized theory of privatization here. The reader does get a final essay by the editors marking about eight or nine different themes from the reading. These are discursive and general (Examples: Privatization is increasing – Experiences of different nations are different – More prestigious firms fare better in their treatment by foreign investors) The summary statements are probably mostly true, but they are hardly tested, and there is no attempt to see if *all* or even most of the cases fit the generalities.

This is fine for what the book is trying to accomplish. It is hard to come up with a general theory that fits all of the amazingly complex twists and turns that can characterize inter-European differences in institutional structure. (Labor relations theorists have been tried and failed many times to explain North European vs South European systems of industrial relations.) All the book is trying to accomplish is establish the basic chronologies so that somebody else can come up with the grand theory.

That said, the authors and editors could have done a lot more to assist in the task of collective description. Summary tables and simplifying displays of complex data are entirely absent. We get long, long lists in the form of A is private, B was public until 1980, C has been private for a long time, D has always been public, with the lists going on and on interminably. It is like having an election summarized by hearing the results of every precinct listed one at a time with no repeats and nobody showing you a map of who is winning where or even adding up the totals to give you the final results.

Maybe the editors could have provided a table of nations broken down into typical types – or maybe a set of tables of nations subbroken down by industries? Maybe some description could have been made of general industrial trends – with the nations who are the most obvious exceptions being marked as distinctive cases?

This book is a tremendous raw material source for any one who wants to do a study of the causes and consequences of privatization under capitalism. But the scholar using this book had better have some strong coffee, and a legal pad. They will have to keep score themselves if they want to follow the game.

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Mielants, Eric H. 2007. *The Origins of Capitalism and the Rise of the West*. Philadelphia, PA: Temple University Press. 256 pages, ISBN-10 1592135757 Cloth (64.50). ISBN-10 1592135765 Paper (\$25.95).

Why Europe? is among the most persistent and vexing questions in all of social science: *The Origins of Capitalism and the Rise of the West* explains European ascendance through a comparative-historical analysis of the relationship between political structures and capitalist development. The central argument of the book is that Europe's rare political structures enabled its rise to worldwide predominance. Specifically, only in Europe were urban merchant elites able to wrest enough political and military power away from nobilities to allow for the use of state infrastructures to broaden and deepen capital accumulation both within Europe and abroad.

The opening chapter of the book presents the theoretical foundation for the comparative analysis in a review and critique of four theoretical perspectives on the origins of capitalism: orthodox Marxism, neo-Marxist "Brennerism" (p. 2), modernization theory, and world-systems analysis. The review is more than merely a passing genuflection before the alter of grand theory. Mielants concisely, but carefully and in some detail, evaluates each of these frameworks. Among other criticisms, orthodox Marxism and modernization theory are too short-sighted. Each overemphasizes the Industrial Revolution as the basis of modern economic growth, while failing to recognize that it was but "...the outcome of different processes that must be traced back to the period before the Industrial Revolution" (p. 10). Brennerism? It is overly preoccupied with "...class struggle between the *exploited* (the peasants) and the *exploiters* (the nobility) within a given territorial unit (the nation-state)" (p. 2-3).

And what of world-systems analysis? It needs to be modified. Specifically, Wallerstein's world-systems analysis neglects "historical continuity" (p. 23) in the transition to capitalism. Mielants contends that the origins of capitalism are found not in the long 16th century as is more customary in the world-systems literature, but in the late-12th century Western European city-states. He argues that many of the capitalist features commonly identified as specific to the 16th century world-economy were already present in many of the late-12th century urban centers of Western Europe: "...for example, wage labor, the specialization of industries, a complex division of labor, class struggles, profits from trade (derived from the fact that entrepreneurs who own the means of production are involved in specialized production and competition), complex financial techniques, and the systematic construction of an exploitable periphery to further the ceaseless accumulation of capital" (pp. 31-32). Thus, medieval European cities were early "power containers of the bourgeoisie" (p. 42) that would eventually yield to nation-states, which would later intensify and expand the scale of capital accumulation: "The same policies and techniques of domination and exploitation experimented with and implemented by elites in the medieval European city-state system were later used by the elites of nation-states during the 16th and 17th centuries to foster their ceaseless accumulation of capital" (p. 43).

Similarly, Mielants contends that Wallerstein's world-systems analysis minimizes the importance of the non-European world in Europe's transition to capitalism. By emphasizing an "extreme break with the past" (p. 23) *within* Europe in the 16th century, Wallerstein does not recognize the myriad impacts of the external, non-European, arena on medieval Western European city-states. And in these city-states, Mielants argues, interactions with non-Europe were shaping the preconditions for the large-scale transition to capitalism in the long 16th century. Thus, world-systems theory also leaves the question unanswered: Why Europe?

For Mielants, the answer lies in the unusual political systems of medieval Western European city-states. From the theoretical review and critique, he sets out into the heart of the book, which is a series of comparative-historical investigations into the political economies of China, South Asia, and North Africa. Why Europe and not China or South Asia? Europe was certainly not exceptional in the size of its cities, the extensity of its urban networks, the intensity of its division of labor, the scale of trade, nor in its propensity to generate wealth. Compared to Europe, however, the Chinese state, and the gentry, was much more powerful vis-à-vis merchant elites. The Chinese Empire was able to extract revenue from the taxation of land, making it relatively "self-sufficient" (p. 74). The South Asian nobility was in a similarly powerful position. In contrast, European political structures were much less centralized, which meant that European nobilities were much weaker vis-à-vis merchant elites: "It was the European nobility's relative poverty...that restricted their political power; their need to call on credit and financiers was structurally unavoidable...charters and concessions had to be given in return for the financial (and at times, political or military) support of the urban-based elites in charge of the city-state politically, economically, and judicially" (p. 79). Thus, unlike their counterparts in Europe, economic elites in China and South Asia were never able to effectively use the state to advance their interests at home or abroad.

So why not North Africa? Islam was not particularly antithetical to capitalist development (p. 126, footnote 2) and North African cities also achieved particularly high levels of economic development as early as the 10th century. Again, however, the existence of autonomous city-states controlled by a mercantile bourgeoisie was much more limited in North Africa when compared to Europe. North African political institutions were frequently

characterized by tribalism, which was “not quite conducive to state-building formation or capitalist development in the long run” (p. 150). If relatively independent city-states would emerge, such as Timbuktu, it was “...more as a consequence of geographical opportunities and economic needs than through political decisions” (p. 133). Without a stable, institutionalized state structure, economic elites were not able to effectively form a “commune or bourgeoisie” (p. 133) that could achieve power “as a class” (p. 143).

As one might imagine, identifying the origins of capitalism is a cumbersome task. The sheer scope of the topic makes it possible to digress into any number of potentially tangential topics. Mielants, however, manages to adhere closely to his thesis, confining many of the less consequential issues to the footnotes. Mielants marshals an impressive collection of research from across the social sciences to investigate the question at hand. In doing so, Mielants makes a convincing case both for the utility of alternative (i.e., non-nation-state) units of analysis and a longer time horizon in world-systems analysis.

Beyond challenging the more conventional views on the appropriate unit of analysis and time horizon, Mielants also wants to help dispel the “Eurocentric biases” that tend to pervade world-systems analysis (p. 43). On this count, the book is marginally less effective. The analyses of China, South Asia, and especially North Africa, do indeed serve to bring them out of “history’s dustbin” (p. 125), and the comparisons with Europe are very revealing. However, if this was an important objective, then the analyses could have investigated the impacts of non-Europe on the transition to capitalism in Europe more systematically. For example, only in the comparison of China and Europe are these impacts of ‘non-Europe’ most clearly articulated. Mielants provides an enlightening, and perhaps somewhat provocative, account of how Pax Mongolica (ca. 1250-1350), by providing relatively stable political-economic contexts in East Asia, facilitated an expansion in the division of labor between Western European city-states and their hinterlands. Even here, however, while it is quite convincing, the analysis is given surprisingly brief treatment. In short, the comparative analyses could have been strengthened with a more thorough explanation of how non-European regions were also consequential for capitalist development in medieval Western European city-states.

This relatively minor reservation aside, *The Origins of Capitalism and the Rise of the West* is an important contribution both to world history and to world-systems analysis. Countless others have examined the relationship between political structures and capitalist development in attempting to grapple with the ‘rise of the west’ question. Thus, the book treads on well-worn ground. However, Mielants is able to clear a relatively novel path by assembling a holistic and coherent account that challenges some of the prevailing wisdom regarding why, how, and when capitalism emerged in Europe. In doing so, he provides an intriguing explanation of why Europe ultimately gained ascendance in the world-system.

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Loveman, Brian, ed. 2004. *Strategy for Empire: U.S. Regional Security Policy in the Post-Cold War Era*. Lanham, MD: SR Books. 339 pages, ISBN 0-8420-5176-7 Cloth, ISBN 0-8420-5177-5 Paper (\$29.95).

Strategy for Empire is a collection of essays by academics and policy makers who have specialized for decades in assessing the consequences of US foreign policy domestically and internationally. These essays address the *practical* results of a recent turn in US foreign policy that sees leading foreign policy establishment figures openly embracing the idea that the US *should* be exerting its power openly as a hegemon. Especially in the post-911 period (though the “War” on “Terror” seems to preclude there ever existing such a time in the near or long-term future), this is advocated unapologetically and declared to be inevitable and urgently necessary. The book is divided into two halves; essays in the first half by foreign policy experts who regard the consequences of this ostensible sea change in a positive light followed by those who take a critical perspective.

The book begins with a series of official policy statements from the White House in the aftermath of 911. Contradictory calls for unilateralism and multilateralism deliver what might appear a schizophrenic message. However, there is in fact no clash of identities, since the latter is plainly subordinated to the former by the present administration. The first chapter is a speech on “The National Security Strategy of the United States of America, September, 2002,” which George Bush delivered in the runup to the US invasion and occupation of Iraq. The message contains an air of confidence in American military capacity (that now five years into the Iraqi *and* Afghani quagmires has come to be regarded as the stuff of bluster) to destroy once and for all terrorist organizations with ‘global reach.’ Leaving aside what is to be done about terrorist organizations without a ‘global reach,’ it is the renewed commitment of the US to an imperial role in not only defeating ‘enemies,’ but in rewarding ‘friends’ of ‘freedom’ who organize their markets in line with a neo-liberal vision of deregulation.

Ralph Peters immediately spells out the dilemma for the US, namely that for most of its history the US has committed itself to ‘break the imperial hierarchies that held mankind in bondage’ (pg. 35). Indeed, there are times that, if put side by side, policy statements on colonialism by the Roosevelt and Truman administrations in the early post-War years echoed those you would find from Ho Chi-Minh or Frantz Fanon. Even such a rhetorical posture, forget substantive commitment to such principles, is now seen as an impediment since the fall of the Berlin Wall. Now the need for a UN is decried as hosting ‘backward, vicious regimes, such as those of China or Russia’ (pg. 41). Instead, new organizations (backed by an assertive US military) are needed such as a “Union of Democratic Nations.” Michael Ignatieff laments the ‘lack of stomach’ America has for a fight, which he fears has given terrorists resolve since the Somalia ‘debacle’ of 1993. America has since been forced to ‘awaken to the barbarians’ (pg. 45). The rationale for global regime-change is, for Ignatieff, self-evident. The dilemma that remains (witness Afghanistan/Pakistan and Iraq!) is how to create nation state stability in post-colonial zones. This is no small problem, since the logics of nation-building and neo-liberal regimens of cost-cutting rarely work well together.

Robert Wilkie argues for the need to bring Europe on board in this global pursuit of post-911 hidden and dispersed enemies, yet it is a Europe that needs to recognize its interest in submitting to the military leadership of the US. “Europe” lacks the capacity to reorganize its economy along a militarist line, which the unilateralists insist is the only way terror can be fought

while sustaining neo-liberal patterns of global economic restructuring. Modernization of European militaries, it is argued, is now key to Europe's ability to contribute to the global search and destroy missions for terrorists. A win-win if there ever were one for the US defense sector.

In Asia, Andrew Scobell asserts that the US Empire must play a more active role in the training of militaries spanning the geographical stretch from Tokyo to Jakarta, especially to countervail the presumably growing threat of North Korea. The need to deal with 'powderkeg' states (Indonesia, China, and Pakistan) requires more involvement on the part of the US army in the training friendly nation-states armies. It is here where the unilateralist vision seems most oblivious to the complex ways in which the developmental priorities of states such as South Korea relies on future cooperation with and investment in North Korea and a less militaristic approach to the resolution of the half-century old Korean War. The unilateralist approach not only avoids historical context, it even seems unaware of how real world patterns of capitalist restructuring shape the interests of developing economies vis a vis relationships with markets that the US deems not ready to be opened until the US deems them so.

James A. Russell's contribution, written on the eve of the US invasion and occupation of Iraq, perhaps most honestly states what is at stake from the unilateralist perspective, namely how intervention in a post-Saddam would affect regional stability. This matter is considered primarily (and unashamedly) in terms of 'reasonably priced' oil access for the US. Yet how to reconstruct a "Post-Saddam" Iraq in a way that would bring stability and the growth of Iraq's contribution to oil development is regarded by Russell as a sticking point that neither he nor the Pentagon/State Department have figured out how to execute going on over 5 years now.

It is a lack of confidence in US capacity to pull off this hat trick that drives the authors featured in the volume's second half. Their perspectives range from skeptical toward unilateralists' underdeveloped sense of practicality to more critical of the underlying motives of US foreign policy that drive pre- (subtle) and post-911 (out of the closet) strategies for empire. And it is in this section that more serious effort is finally spent in theorizing matters of complexity. As Celeste Wallander notes, no serious theory of US foreign policy can take place without acknowledging the *limits* of US influence in key regions of the global political-economy, instead of *apriori* taking for granted a 'need' for increased exertion of US military power in 'hotspots.'

Other contributors likewise point out the glaring contradictions in the presumptions of the unilateralist empire 're'building project. Stephen Zunes points out that the US might argue that it is defending against the expansion of "WMD"s in the Mideast, but this holds little purchase for those in the Mideast who know the role of the US as the leading arms (i.e., WMD) seller in that region and the rest of the world. Zunes argues, in line with other contributors who address contradictions and fault-lines in the unilateralist project in Latin America and South Asia, that it is the departure from the values of 'human rights, democracy, the rule of law,...' that prevents the US from "increas(ing) its influence and promote its strategic and economic interests in this important region" (pg. 249). This seems, as a Noam Chomsky would remind us, to beg the unanswered matter of whether those interests are necessarily as given or desirable for the majority of the world's peoples as Zunes seems to take for granted.

In any event, it is not at this level that the 'alternative' perspectives chosen for this volume are strongest. Rather, it is where we find due attention the banal reality that inequality of economic power within and among nation-states shapes US strategies of empire in both its 'softer' and 'harder' forms. Chalmers Johnson's essay is one of the most powerful contributions

in the sense of the word ‘alternative.’ Johnson contends that today’s policy of military expansionism merely continues the Cold War US strategy of developing ‘thousands of overseas military installations’ to encircle the Soviet Union and China. Noting the case of Japan, Johnson argues that US military bases were exchanged for Japan’s access to US markets. Such access was denied to most of the developing economies during the Cold War and that contradiction was hardly lost on their peoples.

Herein lays the source of much of the ‘instability’ and ‘resentment’ facing the US as it strategizes not how to build empire anew, but how to *maintain* that empire. While the global treaties maintained ‘peace’ during the Cold War, US foreign policy makers face a number of real threats, all tied to the instability of world capitalist markets. Inequalities between nations grow as ‘foreign aid’ declines, resentment and conflicts over resources emerge between rich and poor internationally, intensified global competition among transnational capital in deregulated markets, and violent responses from non-state entities to the volatile consequences of these as combined social processes. We live in an age when US capitalism faces not only heightened concerns about new economic competitors in (ever more volatile) global markets, but unpredictable forms of political violence in the form of ‘international’ terrorism. Thus, Johnson correctly warns, panics about terrorism and ‘rogue states’ now replace the Red Scare as the means to distract the American populace from how the political-economic origins of global instability impact their lives in ways made immediately palpable on September 11th, 2001.

Yet Johnson also seems to assume that the US foreign policy making establishment acts in a self-delusional fashion, declaring that “There is no reason why an economically prosperous China should threaten American vital interests” (pg. 177). I am not so sure. Presumably this would depend on the definition of such interests, in which case the link between strategies for global hegemony and itchy matters such as secular declines in rates of profits, overproduction, and the attendant conflicts that these give rise to have to also come into consideration before assessing such *interests* objectively; i.e. global capitalist markets must be studied not as they *should be*, but as they *are*.

It is here that this collection, whether in its ‘unilateralist’ or ‘alternative’ halves falls short. Of all of the contributors, Johnson takes most seriously the need to explore the link between global political economy and foreign policy choices. It might well be that the seeming disasters created by the unilateralists have merely reflected *realistic* concerns about maintaining US dominance in a world economy that generates decreasing rates of growth, overcapacity, greater levels of inequality, *and* the possibility down the road that China will, via ‘unfair’ means, challenge US hegemony in global markets. And it is here where the ‘alternative’ vision expressed in this book misses the point; this heightened state of anxiety is one shared by the anti-unilateralist wing of the US foreign policy making establishment. One need only listen to the rhetoric of the Democratic presidential candidates on the ‘China problem’ or their uniform support for Columbia’s recent incursion into Ecuador to see that the architects of empire in the post-911 period, whether unilateralist or multi-lateralists, are not listening to (or taking seriously) alternative visions of US interests. A discussion that debates the origins and consequences of both pre-and post-911 empire making projects needs to ask which powerful actors and institutions in US capitalism both uni- and multi-lateralists and listen to and why. Then, and only then, can we make alternatives to pre- and post-911 empire projects, whether they are dressed up in unilateralist or multi-lateralist sheepskins.

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Stahler-Sholk, Richard, Harry E. Vanden, and Glen David Kuecker (eds.) *Latin American Social Movements in the Twenty-First Century: Resistance, Power and Democracy*. Boulder: Rowman & Littlefield Publishers. 393 pages, ISBN 0-7425-5646-8 Cloth (\$85) 0-7425-5647-6 Paper (\$34.95).

As I read this book, the global financial meltdown was beginning to unfold, providing an intriguing backdrop to the analyses raised by the authors of this volume. *Latin American Social Movements* provides multiple lenses on questions about popular mobilization in a region that has become a source of hope and a catalyst for change in the global economic order. Given that Latin Americans have long experience with the downsides of economic globalization, we in the United States may find ourselves looking to accounts such as those in this book for inspiration and guidance through this time of economic turmoil in the core of the world economy.

The book is a collection of three reprints from two 1994 special issues of *Latin American Perspectives* (edited by David Slater), nine papers from a 2007 special issue of the same journal, and seven original chapters. The papers have been edited from their original versions into short and concise chapters that raise important themes in the study of social movements, Latin American politics, and economic globalization. The editors provide a short introduction to each of the book's seven sections. It is fascinating to see how much the chapters written in the early nineties anticipated the developments and themes of today's movements for economic justice. And in many ways, it is also frustrating to see that we still have some distance to go in confronting what Slater called the "ethnocentric universalism or 'Euro-Americanism'" of Western social science (p. 28). Latin American social movements have, according to the editors, "challenged scholars to rethink old categories, push debates deeper, and advance new ideas for understanding [social realities]" (p. 340).

Slater's chapter situates Latin American movements in a global and historical context, while problematizing basic analytical categories such as class, identity, and social movements. His discussion sets the stage for the subsequent analyses of particular movements. While each chapter engages with important theoretical debates—such as the use of ritual and culture in social movements (Issa), race, gender, and identity processes (Mora, Becker, Dixon, Perry), democratization (Vanden, Hellman) and the learning and socialization processes (Schild, Swanger, Villalón, Alcañiz and Scheier), the people and movements of Latin America are at the forefront of this rich collection. Each chapter situates the case within the global historical context, but some chapters go further than others to show the relationships between global economic and political processes and local contexts. Overall, though, the book reflects a good balance of theory and case material that strengthens its value as a course text. In addition, while a central goal of the editors was to explore Latin American movements in light of their relationship to the world-system and the contemporary globalization project, important meso- and micro-level dynamics within movements—such as those related to identity-building and learning—remain the focus of several chapters.

The book includes multiple different chapters on some of the more prominent movements, such as Brazil's landless worker movement, the MST, and the Zapatistas of southern Mexico. And many readers will be familiar with the piquiteros, cacerolazos, neighborhood assemblies, and barter clubs that arose in response to the financial crises in Argentina in the 1990s and 2001. Readers in the U.S. will also be familiar with the femicides in Ciudad Juárez, and will in any case be drawn in by Swanger's account of an anti-domestic violence group and its role in advancing alternatives to neoliberal models of economic and social life. In addition, *Latin American Social Movements* provides accounts of less well-known indigenous, environmental, and resource-based movements in Ecuador and Bolivia.

The book's final section on transnational dimensions of social movements explores the global networks of the Zapatistas and the emergence of Via Campesina as well as the regional transnationalism reflected in the Foro Mesoamericano. Swords's analysis of how groups operating within the broader Zapatista network maintained their autonomy and distinct programmatic identities while still engaging with and supporting the larger struggle provides insights into contemporary global justice networks. Indeed, the challenge of cultivating local ownership and commitment to struggles defined in global terms is central to the success of any attempt to transform global capitalism, and the Zapatista movement has been an important source of insights and models for action in this regard.

Via Campesina is a particularly interesting organization, based as it is on a social category that was to have disappeared with advanced capitalism. This global peasant movement has succeeded during its rather short existence in advancing the notion of "food sovereignty" to the global policy agenda. As the global food crisis we are now seeing continues, the conceptual and organizational resources of Via Campesina may prove to be invaluable for both communities and policy makers.

The editors provide a helpful summary of the lessons from the cases in *Latin American Social Movements* as they identify important challenges ahead. Numerous cases explored in the volume emphasize the role of historical memory and learning in the evolution of social movements. Another prominent theme in the book's cases is the ways challenges to neoliberal globalization expand activists' "political imaginations" (p. 339), considering the ways global structures and processes impact local experiences and possibilities for social change. Although enmeshed in sometimes overwhelmingly oppressive social relations, activists were able to imagine alternatives and identify steps towards their realization. Their work continues in contemporary struggles in Latin America and elsewhere. Finally, the most important consequence of the movements explored in this volume is that they helped challenge proponents of global neoliberalism by demanding a place at the table. They were claiming "the right to have rights," (p. 339) and in doing so they both inspired and helped pave the way for future movements against global capitalism.

One question that I was left with after reading this book is how these streams of activism across Latin America have contributed to ongoing struggles against neoliberalism that extend far beyond this region. Clearly Latin American activists and movements have played key roles in the contemporary global justice movement and the Zapatistas and the World Social Forum process are the most prominent, but certainly not the only, examples of this. A task for a future volume may be to trace the connections between Latin American feminist, indigenous, anti-racist, and economic justice movements and the global networks that help constitute the counter-hegemonic bloc known as the global justice movement.

Latin American Social Movements is an enjoyable book to read. As part of Rowman & Littlefield's Latin American Perspectives in the Classroom series, and I believe it will be a very useful and highly accessible text for a variety of courses.

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