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Contents, From the Chair & Essays

"In reality, so far as I know there is no sociology worthy of the name which does not possess a historical character."
—Emile Durkheim

FROM THE CHAIR

New Social Forms in World Society: Beyond State and Class

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The field of comparative-historical sociology was founded on two core concepts: social class and the nation state. Although they seldom operated at the exclusion of other concepts, the categories of state and class have been elemental building blocks for decades of research, and they continue to motivate some of the best work in our field. Yet, unless we augment these concepts with a new analytic repertoire, we will approach our own *fin de siecle* burdened by the legacy of social theory from the previous century's turn. In our call for papers for our section panels on "New Directions in Comparative and Historical Sociology" at the 1999 ASA meeting in Chicago, therefore, we especially invite contributions that examine new social forms in world society. Among these, we think not only of racial, ethnic, and gendered forms in addition or opposition to the structures of class, but also of new organizational forms that are emerging at the interstices of the structures of states.

Illustrative, and far from exhaustive: going beyond the problematic of the multinational corporation, economic sociologists in our field are questioning the firm as the unit of analysis as they move beyond economies of scale and scope to explore the economies of speed that link fashion designers on Seventh Avenue to producers in East Asia and back to the Gap or Nike stores on Broadway through complex subcontracting networks. The nation state is not displaced, but its sovereignty now coexists with the new sovereign structures of supranational entities (e.g., the European Union), subnational regional authorities that cross national borders (e.g., river authorities or economic development agencies), and new rule-making entities such as NAFTA, GATT, and the WTO.

But our attention should not remain only on the acronymic trade organizations and protocols that are glorified or vilified in the rhetoric of globalization. We should also investigate the new social forms that are emerging from the Environmental Treaty in Kyoto, the World Conference on Women in Beijing, and the new human rights agreement on International Criminal Courts in Rome. Our turn of the century finds international criminal networks and new transnational policing agencies; new patterns of migration and new forms of marginality; global arms trafficking and International War Crimes Tribunals.

If sovereignty is being restructured, no less is territoriality. Where is an Internet transaction? What is the geography of the Web? That these questions are oxymoronic only illustrates the conceptual challenges to comparative sociology at century's end. The historically minded sociologists among us, of course, will question whether all of this is really so "new" and "emergent." The Catholic Church has been a powerful transnational organization for centuries, they could note. And what's so novel about international criminal networks? Such questioning goes to the very strength of our section --our comparative and historical section. Together, we can develop categories that are not simply newer but, more importantly, better for understanding the momentous social transformations in which we are living.

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"NEW" DIRECTIONS IN COMPARATIVE AND HISTORICAL SOCIOLOGY

This and the next issue of Comparative & Historical Sociology are (loosely inspired by our section's ASA sessions) devoted to 'new' directions in comparative and historical sociology. Discussed are a variety of themes and perspectives that offer a glance at the rich and exciting field of on-going sociological research with an explicit comparative and/or historical orientation. This is the first in a two-part series, with contributions from:

- Edward Tiryakian and from
- Mustafa Emirbayer and Mimi Sheller

Please check out the hard-copy versions as the footnotes appear to have been deleted! Sorry...

FROM LE PLAY TO TODAY

by ***Edward A. Tiryakian****,*
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Given the abundance of excellent methodological volumes available, commensurate with the growing inter-nationalization of the discipline, I will not elaborate on the methodology of comparative and historical sociology (hereafter CHS). Rather, I will briefly sketch some of my comparative research projects and their context along what is by now a rather lengthy professional career. I will select one project from each of three career stages.

At the onset, I have no recipes, no secret ingredients as to what makes a good or a bad CHS. Perhaps the wisest admonition to neophytes is the answer that S.N. Eisenstadt -- one of the most prolific comparativists of our times-- gave at a faculty seminar at Duke some years ago when I asked him the secret of his success: "Be informed of major interesting controversies and follow your nose!" As a supplement to Eisenstadt's advice, I would urge students to keep a valid passport if overseas research opportunities arise and to have read: (a) Sombart's essay "Why No Socialism in the United States?" and the literature it has spawned; (b) R. H. Tawney's masterful *Religion and the Rise of Protestantism*; and (c) the mid-19th Century studies of European workers and their family organization conducted by Frédéric Le Play (the unsung pioneer of *in vivo* CHS).

As a graduate student in Harvard's Department of Social Relations in the 1950s, I was in an interdisciplinary ethos where most of the faculty and many fellow students were interested in, engaging in, and supportive of interdisciplinary, comparative research (among my fellow students were Robert Bellah, Neil Smelser, Charles Tilly, Ezra Vogel and Joseph Elder). In his seminar on race relations, Gordon Allport approved my doing a term paper on race relations in South Africa. I became fascinated with historical similarities in the development of two Calvinist settler societies, both an economic colossus in their continent, one that had opted a few years earlier for a policy of 'apartheid,' the other, beginning with Truman's desegregation of the military, for a policy of liberalization. What accounted for the difference? I "followed my nose" and thought that the same core Protestant doctrines might be the source of contrasting ideologies, that is, that the 'Protestant ethic' might have taken different roads in the two settings. Unfortunately, I had to put in abeyance going to that part of Africa for the mundane reason that in the mid-1950s there were no funding opportunities for doctoral dissertation research in South Africa.

However, opportunity knocked at the door in the form of a Fulbright fellowship to the Philippines. Having taken courses as an undergraduate and as a graduate student on the comparative social structure of East Asia, the opportunity of doing a field study in that region was appealing. One of my graduate areas of specialization was industrial sociology, and there had been a spate of surveys of occupational stratification indicating remarkable consistency from one country to the other. However, all the surveys had been conducted in Western societies. Would the convergence hold in a non-Western, largely agrarian 'developing' society? That was the empirical question that framed my field research in Central Luzon in 1954-55. I got invaluable experience conducting field interviews in urban and rural areas, some close to heavily contested zones where Huk guerrilla and government troops were clashing. Years later, an unexpected opportunity to publish the dissertation led to taking inventory as to the course of socio-economic development in the Philippines since my stay there. In a sense, it is what has <u>not</u> taken place (at least, not through the immediate post-Marcos years) that constitutes the comparativeand still for me unresolved *problem*.

To state the problem as a set of interrelated questions, why is it that the Philippines, a staunch ally of the United States, with much greater natural resources than Japan, had not developed at anywhere the level of Japan, why was its growth rate lagging other East Asian countries, why was there the same gaping inequalities between social strata in the 1980s as there was when I was doing my field research? What were the critical factors holding back the modernization of the Philippines which, unlike Japan, Korea and Taiwan, had had nearly continuous exposure to the West since the 16th Century, and in particular to American education and democratic practices as an American commonwealth territory until 1941, with independence in 1946? Why, in spite of this, including a very high rate of college education, was the Philippines a laggard in the take-off of East Asia in the 1960s and 1970s and behaving more internally like a Central American country such as Guatemala and San Salvador? Under what conditions can the Philippines put again its act together, as it seemed it might be able to do in the early 1950s before the tragic death of charismatic President Magsaysay? Or, is the ungluing of neighboring, multiethnic Indonesia an alternative scenario for the Philippines? My initial comparative question of occupational stratification that led me to do field research was relatively easy to answer in a dissertation after solving some technical methodological questions, but the uncovered comparative problems of development retain their challenge.

The second project is one I began at mid-career. While in Paris on a sabbatical, I chanced to see a poster proclaiming a coming festival of "minority nations". I attended the festival, consisting of different singing troupes from Corsica, Brittany, the Basque region, Catalonia, Quebec and so on. What particularly caught my attention was the recurrent theme of being "colonized" victims, deprived of expressing themselves in their own language and culture by that of the dominant state. Having (after my Philippines project) worked on another part of the Third World, late colonial Africa, to hear the discourse of the "colonial situation" in reference to long-established Western democracies was challenging. It led to my doing fairly extensive study of settings like Quebec, Wales and Scotland, utilizing both historical data, field interviews, and (in the case of Quebec) participant-observation. I became convinced that the "colonial situation" (having intersubjective as well as objective dimensions) and movements of independence, in Africa and in Western societies had significant structural features in common, and perhaps dynamic elements in common. The uncoupling of overseas "colonies" from "empires" seemed to have a cognate in the autonomous movements (that became labeled "ethnonationalism") seeking to uncouple "nation" from "state". This was taking place even as leading political sociologists (such as Skocpol and Tilly) were giving primary attention to "bringing back the state" to the forefront of comparative and historical analysis. With a group of social scientists intrigued by the "anomaly" of Western countries being seats of regional autonomy movements against the central state, we eventually brought out a volume seeking a comparative understanding of the phenomenon that had little place in the accepted sociological wisdom of modernity.

This project is not ended. It got a new impulse in 1989-1990 with the implosion of the Soviet Empire, with a plethora of nationalist movements springing up from the Baltics to the Balkans and points east as unintended consequences of *perestroika.* I have come to consider the nationalist movements of Eastern Europe, those of Western societies, and those of Africa as one large interrelated set, one large "laboratory" for comparative and historical analysis. The ones in Western societies have political formations in the vanguard of autonomy which are essentially social democratic in their orientation and may or may not achieve in the next half-century their goal by peaceful means. Certainly this is an opportune time for comparative, collaborative studies with counterpart colleagues East and West, North and South. As a frame for this on-going research project, I find heuristic the set of questions "What is our national identity?"; "Who is ‘the other’?"; and "Under what conditions will ‘the other’ be accepted and accept to become an ‘insider’?" This, in my way of thinking, is a key problematic of national development in the post cold-war era. I cannot think of any country where the set of questions does not apply, whether Great Britain, Germany, France, Russia... or the United States. Obviously, there is great variability in the details of the questions, but sorting that out is certainly part of the challenge of cross-national analysis.

Now to the third project, one linking to my earlier concerns but which has come to the foreground of my work at century's end. Ultimately, a comparative-historical orientation returns us to home, that is, the understanding in time and space of "others" sheds light on our own situation, just as at the departure our understanding, implicitly or explicitly, of our situation frames our comparative inquiry. Broadly speaking, then, I see one justification of comparative historical analysis as providing critical feedback on our existential condition. In trying to make sense of national development and its obverse, the uncoupling process of nation-states (including here the separation of colonies from their *métropole*) at different stages, I take as a presupposition that there is nothing inevitable about the uncoupling, although after a certain threshold is reached, the process may be irreversible. I now come to my third problem set. What, given the extent of ethnic, racial and regional diversity in the United States accounts for the absence of separatist movements, even as hostility to the central government has grown and become fashionable? What accounts for an underlying national unity and an overriding nationalism, even if the very word "nationalism" is a term of opprobrium? More critically, is this national unity transitory in the face of growing distrust and mistrust between races as well as toward government? Is there, in sum, an uncoupling or a readjustment of the American nation-state and key features of national identity?

My ingress into American national identity is the religious factor in national development, more specifically what I am calling "American religious exceptionalism". Standing on the shoulders of Weber and Lipset, my current major CHS project is an historical excursion that begins with Sombart's heuristic question at the time of the St. Louis World's Fair, "Why no socialism in the United States?" What Sombart left out, perhaps because he was even less "religiously attuned" than Weber, was the significance of the religious factor among urban native and immigrant American workers. To document this involves establishing levels of religious participation in European countries as well as the United States. That is just a starting point. What I seek to do is an historical interpretive study (in the mold of Bellah's *The Broken Covenant*) with a dual focus: first, linkages between certain institutional features of American society, including its foreign policy and the basic Puritan-Calvinist paradigm, and second, how the religious vitality of the USA (as measured by various indicators), an anomaly by accepted standards of modernity, is evidenced in all the major faith communities. But why in the United States, the epitome of advanced capitalism, and not in other countries? And will the anomaly disappear in the next century as a result of demographic changes, globalization, or other situational factors? I doubt that this problematic can be resolved, but "my nose" tells me that the fate of American national identity is tied to the evolving interaction of capitalism American-style with its religious culture.

To condense my observations, the significant problems that CHS uncovers are seldom resolved definitively, but may well resurface, even in the same setting. Keeping abreast of these, by means of a variety of empirical research projects, is one way for sociology to both mature as a discipline and to stay youthful in its inspiration.

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STUDYING PUBLICS IN HISTORY

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A potentially exciting new direction for comparative-historical sociology is the study of publics in history. For many years now, the upsurge of interest in ideas from the Habermasian tradition of critical theory (which helped to reintroduce the concepts of civil society and the public sphere into sociological inquiry) has, with but occasional exceptions, failed to leave much of a mark upon long-established currents in comparative-historical research. In part, this is due to the conceptual schemas that comparative-historical sociologists themselves have relied upon for guidance, but in part also to the inability of analysts interested in civil society and publicity to move beyond the normative level by incorporating research techniques and insights from empirical sociology. In our view, a renovated approach to the study of publics in history might not only fill an important void in the literatures on civil society and the public sphere, but also introduce in a more compelling way the promising insights of this largely normative tradition into empirically oriented social and historical inquiry. Our agenda for research into publics in history comprises three dimensions.

The first of these dimensions is that of *institutions and their interstices*. Since its resurgence in the 1960s, comparative-historical sociology has been largely oriented around two "master concepts": the administrative-bureaucratic state and capitalist social relations. Here, we begin by delineating a third analytically distinct institutional domain --*civil society*-- which resides, metaphorically speaking, "in between" states and economies and which is organized around principles of solidarity and associationalism. This concept plays several potentially useful roles in comparative-historical research, which complement and in many respects parallel those of the aforementioned two master concepts. For example, it allows researchers to explore historically the variable autonomy of actors within civil society vis-a-vis those in states and economies, the complex, reciprocal determinations among these three institutional complexes; and it makes possible cross-national comparisons even in the present day, focusing not only upon the self-defense of civil society vis-a-vis states and economies, but also upon the internal democratization of civil society itself. This threefold schema is only part of the story, however. For "societies have never been sufficiently institutionalized to prevent interstitial emergence." States, economies, and civil societies are relatively bounded and stable complexes of institutions, but *publicity* is emergent. Through networks of publicity, the communicative impulses of (certain tendencies within) civil society impact upon the state and economy, as well as reflexively back upon civil society itself. Through what we term political, economic, and civil publics, social actors seek to influence (and even to transform) these three established complexes of institutions, respectively.

The second dimension of our research agenda, which cross-cuts the first, has to do with *agency and the relational contexts of action*. Institutions can be conceptualized as bounded sets of practices channeled through overlapping (and partially autonomous) matrices of social, cultural, and social-psychological ties and reproduced agentically through ongoing iterational processes. Networks of publicity, too, can be analyzed in terms of social structure, culture, and social psychology and in terms of both their internal micro-dynamics of interaction and the external macro-dynamics of their engagement with established institutions. Consider, for example, the *social-structural* insight that publicity "strengthens" through increases in the intensity of association, public debate, and decision-making. Defining density as "the proportion of possible lines that are actually present" between nodes of a social network, one can inquire more specifically into the relationship between thickness of weave in networks of publicity and not only their internal vitality, but also their capacity to influence political or economic decision-making or else to impact back upon civil society itself. Social-structural approaches can also help in analyzing the "cohesive subgroups" that typically emerge within and among networks of publicity. And they can open out onto larger themes of agency and dynamic processes by showing how enhanced capacities for self-organization or influence on the part of public actors flow disproportionately to those with social networks optimized for structural holes. The *cultural* and *social-psychological* dimensions of publicity can be studied in a parallel fashion, drawing upon new approaches in cultural analysis and in the sociology of emotions and highlighting symbolic and psychical/emotional networks, respectively -- as well as actors' different modalities of engagement with those relational contexts. Symbolic and psychical matrices constrain and enable action within and across publics no less than do social-structural configurations.

The third dimension of our research program has to do with *the specification of causal mechanisms*. While the two frameworks set forth above introduce a range of systematically interconnected concepts and distinctions regarding publicity, they do not take us as far as we need to go, for they fail to raise the issue of productive or generative causation, the question as to how particular outcomes or effects are actually brought about in complex historical sequences involving publics. Such a question eludes as well the two currently dominant strategies in comparative-historical inquiry, interpretation and explanation, for these revolve instead around either historicist story-telling or the search for concomitant occurrences (or necessary and sufficient conditions). If historical processes in which publics play an important role are to be analyzed in generalizable fashion, then a relevant set of "*recurrent causal sequences of general scope*" -- causal mechanisms involving publicity-- must be delineated. Here, the concepts and distinctions presented above can be of at least partial value, for they can open up new analytic terrains upon which to hunt for and to specify such generalizable causal processes. Empirical studies, for example, can inquire into the divergent causal mechanisms whereby political, economic, and civil publics manage to expand democratic decision-making within their respective targeted institutions. Others can inquire into bridging mechanisms within and across publics in the social-structural context of action, or into linguistic exclusion mechanisms that serve to divide public actors within the cultural context of action, or into trust-building mechanisms that establish psychical or emotional ties of solidarity within the social-psychological context of action. And such causal mechanisms can be invoked singly or in concatenation to explain historical outcomes, whether in single case studies or in comparisons among multiple cases.

Much work remains to be done, of course, in elaborating an adequate research agenda for the study of publics in history. But the payoffs are considerable if comparative-historical inquiry is to move in systematic fashion beyond the study of its two master concepts into a still more multifaceted agenda that includes both civil society and the complex dynamics of networks of publicity.