

SUGGESTIONS AND DEBATES

Neither Exceptional nor Peculiar

Towards the comparative study of labor in advanced society

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The practice of labor history is marked by a curious ambivalence toward the comparative approach. In method and style, labor historians tend toward the unique and the local, if not the microscopic; in interpretation and conceptualization, however, they routinely work with models that are highly general and at least implicitly comparative. Thus they write, more often than not, about a single occupation or workers in a particular town or, in a somewhat older style, about a particular industrial or political organization or, in an even older historiographical tradition that is still surprisingly vital among labor historians, about individual labor leaders. But when they get around to explaining what they have found in their detailed studies, they speak in broad terms about the general trajectory of labor in America, in Britain or wherever and, still more surprisingly, in terms that distinguish one or the other of these national experiences from the pattern presumed to obtain in industrial society in general.

There is thus a sharp disjuncture between the very broad interpretive framework and the rather narrow research strategy typically employed by labor historians. This is both troubling and encouraging. It suggests, on the one hand, that theory and research are even less well integrated in this field than in other areas of historical study. But, on the other hand, it also indicates the existence of a diffuse yet powerful impulse toward comparative analysis in labor history. If this impulse has yet to produce research equal to the task of genuine comparison, it does at least hold out the hope that those interested in the historical study of labor would be receptive to a serious effort at comparison.

Before discussing what such an effort might entail, it might be helpful to discuss in a bit more detail the contradictory relationship that seems to exist between labor history and the comparative method. The curious thing in this regard is that the form in which the concern for comparative analysis comes out is in the obsession with the uniqueness of each nation's labor history. The classic case, of course, is the United States, where labor

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historians remain fixated upon the problem of “American exceptionalism”.¹ The study of the working class of other countries is, however, by no means immune to this kind of implicitly comparative formulation. It might help to get a sense of the importance of such “exceptionalist” approaches to review briefly an example that is perhaps less well known than the US debate: that is the largely parallel discourse over the so-called “peculiarities of the English”.

The phrase “peculiarities of the English” comes from the title of Edward Thompson’s critique of the argument about the course of modern English history made by Perry Anderson and Tom Nairn in a series of provocative articles in *New Left Review* in 1964.² Anderson’s piece purported to offer a synoptic view of the overall development of English society and politics in the modern era, while Nairn’s two-part essay was focused more directly on the labor movement. But the starting-point for both Anderson and Nairn was their shared diagnosis of the tragic failure of the British working class and of British socialism.

What was the nature of that failure? Clearly it was not, as in America, the absence of class consciousness or of a distinctly working-class party; nor was it the absence of a socialist tradition within British political culture. It had to do instead with the paradoxical fact that British workers were extremely class conscious but that their class consciousness did not translate into a full-blooded commitment to socialist transformation. The political consciousness of British workers was distinguished by a vapid set of ideas that other scholars, before and after Anderson and Nairn, tended to group under the vague heading of “Labourism”. “Labourism”, in this

¹ For some of the more recent discussions of “American exceptionalism”, see Jerome Karel, “The Failure of American Socialism Reconsidered”, in R. Miliband and J. Saville (eds.), *Socialist Register 1979*, pp. 204–227; Mike Davis, “Why the U.S. Working Class is Different”, *New Left Review*, 123 (1980), pp. 3–46; Eric Foner, “Why Is There No Socialism in the United States?” *History Workshop*, 17 (1984), pp. 57–80; Sean Wilentz, “Against Exceptionalism: Class Consciousness and the American Labor Movement”, *International Labor and Working Class History*, 27 (1984), pp. 1–24; and the responses to Wilentz in the same issue by Nick Salvatore, pp. 25–30, and by Michael Hanagan, pp. 31–36. The notion of “exceptionalism” in labor history is, of course, but a part of a broader celebration of “American exceptionalism”. See, for example, Daniel Bell, “‘American Exceptionalism’ Revisited: The Role of Civil Society,” *The Public Interest*, 95 (Spring 1989), 38–56.

² Perry Anderson, “Origins of the Present Crisis”, *New Left Review*, (January–February 1964), pp. 26–53; Tom Nairn, “The Nature of the Labour Party”, Pt. 1, *NLR*, (September–October 1964), pp. 38–65; Pt. 2, *NLR*, (November–December 1964), pp. 33–62; and Edward Thompson, “The Peculiarities of the English”, in R. Miliband and J. Saville (eds.), *Socialist Register 1965*, pp. 311–362. On the impact of the debate within British social history, see Keith Nield, “A Symptomatic Dispute? Notes on the Relation between Marxian Theory and Historical Practice in Britain”, *Social Research*, 47 (1980). Its continuing relevance can be seen in Gareth Stedman Jones, *Languages of Class: Studies in English Working Class History, 1832–1982* (Cambridge University Press, 1983), which remains interpretively very much of a piece with the Anderson/Nairn position; and in Ellens Meikins Wood, *The Pristine Culture of Capitalism* (London: Verso, 1991), esp. pp. 11–19, which does not.

usage, was seen as a form of class consciousness lacking what Anderson referred to as “hegemonic thrust”; and it was said to have found its practical reflection in Britain’s supine and “corporatist” labor movement.

The causes for this failure, according to Anderson and Nairn, stretched far back into English history. Very briefly, they argued that British labor was deficient in political and ideological rigor primarily because Britain’s bourgeoisie itself lacked a properly developed class outlook. This was due to the fact that, in the era of bourgeois class formation, the English bourgeoisie faced an aristocratic enemy that was too powerful and too politically flexible to be beaten. Instead, the aristocracy cajoled, compromised with and in many ways absorbed the bourgeoisie, so that by the nineteenth century the British working class confronted a ruling class that was part aristocratic and part bourgeois, an élite whose pragmatic politics gave workers and their radical allies little to fight against – intellectually, at least – and who therefore retarded the ideological development of the forces opposed to capitalism.³ Indeed, a thorough-going and sophisticated critique of capitalism had to await the arrival in Britain of two German exiles, whose mature writings would not, in fact, be available in English translation until close to the turn of the century. By that time, the moment of most intense class conflict, associated with the Chartist movement of the 1830s and 1840s, had long since passed, and British radicals and socialists and ordinary workers had been forced to make do with an odd amalgam of romantic critique and left utilitarian proposals for reform – hardly the stuff, in the perspective of Anderson and Nairn, out of which revolutionary commitment is formed.

In response to Anderson’s and Nairn’s argument, Edward Thompson wrote a long and brilliant essay in the *Socialist Register* 1965. His detailed counter-arguments need not concern us here, however interesting and well-put they were. Suffice it to say that Thompson outlined a far subtler and more informed account than Anderson and Nairn, and that his rival account would undoubtedly accord much better with the views of most professional historians.⁴ But, in an odd sense, Thompson failed not only to win over his opponents, but also to convince many of the younger generation of historians who have subsequently gone on to write the his-

³ This focus upon the persistence and strength of aristocratic forces in explaining the weakness of British socialism ironically reverses the arguments put forward about the peculiarities of the American and German labor movements. Thus it has often been argued that in America it was the absence of feudalism and an inherited class structure that allowed individualist and capitalist values to sink such deep roots in American political culture and to prevent the growth of class politics. In Germany, of course, the argument has been that aristocratic power, manifest in particular in the so-called “re-feudalization” of the Reich in the late nineteenth century, hindered the development of German democracy, pushed the German labor movement into a permanent quasi-revolutionary stance and thus prevented the emergence of a strong and consistent social-democratic reformism.

⁴ See, for example, Ross McKibbin, “Why Was There No Marxism in Britain?” in *Ideologies of Class: Social Relations in Britain, 1880–1950* (Oxford: Clarendon, 1990), pp. 1–41.

tory of working people in Britain. It would seem, in other words, that Anderson and Nairn had hit upon an important problem and that, whatever the merits of their particular solution to it, the problem, once posed, would not go away. As much as Thompson might argue that the Anderson/Nairn analysis depended upon a grossly ahistorical contrast between British reality and highly romantic renderings of the history of the French revolution, the Paris *commune* and the Bolshevik revolution, the comparative question has remained compelling, as has their putative solution to it. There was something unique and different about British labor, or so it seemed, and it was important to discover what it was and why it was.

Scholars writing about other labor movements may not be quite so pre-occupied analytically with the problem of “peculiarities”, but they are nevertheless seriously concerned with it. Students of the French labor movement, for example, have been struck by its apparently unique features – the prominence throughout the nineteenth century of small workshop production and the mentality that accompanied it, the strength of syndicalism early in the twentieth century and of the communists from 1945 until the early 1980s, and the consistently low rates of unionization, at least by international standards. Likewise, historians of German labor have been regularly concerned with explaining the strange process of “negative integration” that supposedly marked the rise of labor in imperial Germany and that prevented social democracy from playing the reformist role it might otherwise have performed. Paradoxically, it is also the case that many scholars and activists have often considered the German Social Democratic party as the model of what a powerful socialist movement should look like; hence, the argument about German peculiarities is regularly turned around.

The concern for the “peculiarities” of the English, American, French, German, or other labor movements is, however, an odd form for a comparative perspective to take.⁵ It is a sort of incomplete and semi-conscious comparative analysis. On the other hand, it is not all that surprising that such a hybrid would emerge from the study of labor. Labor history is, as Eric Hobsbawm has reminded us, a highly ideological field, and students of labor seem to be drawn more powerfully than others to overarching theories about the direction of social and economic change, in particular to Marxism and to modernization theory.⁶ Both of these theoretical orientations encourage an essentially teleological approach to the evolution of labor movements that assumes a common set of social and economic

⁵ It does not, for example, fall neatly into any of the categories of useful comparative analysis that Charles Tilly describes in *Big Structures, Large Processes, Huge Comparisons* (New York: Russel Sage Foundation, 1984). Nor does it conform to any of the three styles of historical and comparative sociology that Theda Skocpol describes in her essays in *Vision and Method in Historical Sociology* (Cambridge University Press, 1984).

⁶ Eric Hobsbawm, *Workers: Worlds of Labor* (New York: Pantheon, 1984), pp. 1–14.

processes interacting to produce broadly similar outcomes across nations.

Inevitably, though, such universalizing assumptions run up against the awkward realities of working-class history. The result is a recognition that, whatever the general pattern, the particular case under review does not fit. It is a testimony to the candor and professional rigor with which labor historians today conduct their inquiries that these anomalies are acknowledged and, to the extent possible, explained away. Still, the recognition has typically produced not a reworking of the interpretive framework, but rather a search for that factor or set of factors which makes the experience of this or that nation different from the presumed norm of other industrial societies. This is not a very sound basis for conducting comparative analysis, for it gives rise to a style of analysis in which it is taken for granted that most of the factors that condition the development of the labor movement do not vary significantly and that the one or two special, or peculiar, factors that do happen to vary in an obvious fashion will account for the varied outcomes of that development. Work in this vein is prone to errors in two directions. It errs, first, in homogenizing, or neglecting entirely, certain aspects of the process of class formation across and within societies; and second, it tends to attribute far greater significance than is warranted to variations in the narrow range of factors deemed critical and, related to that, to magnify the actual differences between cases on that particular dimension.

These weaknesses are evident to some degree in even the best work in comparative labor history. Take, for example, the recent collection on class formation put together by Katznelson and Zolberg. Their book represents a major advance in the effort to develop a framework for comparative analysis, but in the end opts for the familiar focus upon one variable – in this instance politics – to explain the diverse trajectories of national labor movements.⁷ Thus, Katznelson begins by arguing that a proper conceptualization of class formation must encompass the formation of classes at work and in the community – in the spheres of production, consumption and social reproduction, to use another parlance – and also in politics. He ends up, however, by stressing politics, for it is at the level of politics, he claims, that genuinely significant variation occurs and that variations of this sort are linked most closely and directly to the most critical outcome of class formation: the political strength of the labor movement. Katznelson argues even more explicitly that “the broad outlines of [. . . social and economic] changes were shared across political boundaries” in the

⁷ Ira Katznelson and Aristide Zolberg (eds.), *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States* (Princeton University Press, 1986). For earlier variations on this argument, see Adam Przeworski, *Capitalism and Social Democracy* (Cambridge University Press, 1985); and Katznelson, “Working-Class Formation and the State: Nineteenth-Century England in American Perspective”, in Peter Evans, Dietrich Rueschmeyer and Theda Skocpol (eds.), *Bringing the State Back In* (Cambridge University Press, 1985), pp. 257–284.

nineteenth century and that such factors could therefore best be used to explain “the shared features of working class formation”. To get at the differences in class formation, however, it is necessary to look at “extra-economic factors”, especially “the organization of the state and its public policies”.⁸

Put schematically, then, the argument offered by Katznelson and Zolberg is that the social and economic factors shaping class formation did not vary greatly across nations and hence do not require much attention. Cultural factors, what they refer to as “dispositions”, varied a bit more, but not critically. The essential variation was thus in political structure. By this they would appear to mean those things discussed by other writers, such as the degree of repression and the openness of the system, plus the policies toward labor and social issues pursued by the regime and, most important, the pace and pattern of its progress toward democracy.⁹ The timing of democratization matters enormously in their account, because certain structural features of the political system are fixed in a more or less permanent fashion at the moment when workers first enter the polity on a large scale. In the United States, it is argued, workers were enfranchised quite early on, well before the process of industrialization really took hold. Hence, conflicts generated at work or over the very shape of the workplace remained independent from political struggles. Workers thus participated in politics as residents of the local community – as individualized citizens of a mass democracy or as members of particular ethnic groups – but not as a class defined in economic and social terms. In England, by contrast, it is argued that the *de facto* exclusion of large numbers of workers from electoral participation during the early and middle stages of industrialization and class formation allowed class loyalties to develop prior to enfranchisement and to be reinforced by a common lack of political rights. This ensured that the granting of effective democracy, which occurred in several steps from 1867 to 1918, was accompanied by efforts to project a working-class presence into politics and, with the founding of the Labour party, by its steady growth as a mass party of the working class.

Both this specific analysis, and the broader rethinking of the process of class formation of which it is a part, constitute a notable advance in the comparative study of labor. Nevertheless, it remains largely trapped within the logic of the “peculiarities” or “exceptionalism” approach, for it continually reverts to an emphasis upon one factor in the process of class formation at which the supposedly critical variation occurs. The focus on politics, though useful in itself and a needed corrective to earlier work,

⁸ Katznelson, “Working-Class Formation: Constructing Cases and Comparisons”, in Katznelson and Zolberg, *Working-Class Formation*, pp. 22–23.

⁹ For examples, see Seymour Martin Lipset, “Radicalism or Reformism: Sources of Working-Class Politics”, *American Political Science Review*, 77 (1983), pp. 1–18; and Dick Geary, *European Labor Protest, 1848–1939* (London: Croom Helm, 1981).

tends to neglect the economic and the social-structural dimensions of class. More precisely, such work proceeds on the assumption that the social and economic histories of various countries can be assimilated to a common pattern. This is an extremely difficult proposition to defend – theoretically, empirically, or methodologically. Theoretically, the argument for the convergence of economic and social structures over time and space, and hence for the common pattern of social change, has been abandoned by all but the most committed, and least historically minded, advocates of modernization theory. Empirically, research in the history of European nations experiencing industrial transformation has turned up so many alternative paths and so many anomalies as to lead scholars to question or severely qualify the very notion of an “industrial revolution”. Methodologically, the argument for the centrality of politics, or of variation in political structure, in determining the outcome of the process of class formation simply cannot be demonstrated convincingly without equal attention to the range and incidence of variation in social and economic structure.

If the most sophisticated approaches to comparative labor history have yet to provide a model for how actually to conduct effective comparison, it might seem appropriate to turn for guidance to work that is not explicitly or self-consciously comparative. What, more precisely, do the better national histories of labor suggest as a model? While it is probably impossible to agree on what constitutes the best recent work in labor history, it is surely not controversial to say that the most compelling rendition of working-class development remains that of Edward Thompson. By extension, we might further suggest that if the practice of national labor history contains a model that can be more broadly applied, it ought to be manifest most clearly in Thompson’s *The Making of the English Working Class*, and to be displayed in subsequent work that seeks to apply Thompson’s approach to other times and places. Thompson has, after all, provided the inspiration for a generation of researchers who have tried to fashion synthetic and interpretive accounts of labor that follow his lead in laying great stress upon the role of popular culture in shaping the distinctive features of the labor movement.

Not only is Thompson’s the most distinguished single book in modern labor history, but works inspired by and closely modelled on it have been uniquely influential in shaping historians’ understanding of particular national experiences. William Sewell’s book on France, Gareth Stedman Jones’ work on the later stage of British labor history, Herbert Gutman’s essays on American working-class history and Sean Wilentz’s more focused book on antebellum New York – to take four widely, and justly, praised examples – have all deployed an interpretive strategy akin to Thompson’s. Each of these scholars displays the command of sources and the feel for context and meaning that distinguished Thompson’s work, and each has fashioned a rich and nuanced account of working-class life and politics in Britain, France, or the United States. It would be to some extent

unfair, therefore, to lump them together or to reduce these efforts to a few simple formulae. Nevertheless, they share enough by way of method and logic to permit a rough and schematic summary.

Viewing them in common, one could say to start that Thompson sees the distinctiveness of the English labor movement in the unique tradition of plebeian radicalism with which English artisans and laborers entered into the process of industrialization and in the way that tradition shaped the consciousness of workers as it, and they, emerged at the other end of the process. Stedman Jones also focuses on workers' prior cultural inheritance but, by contrast, sees later generations of British workers as much less well-served by their heritage.¹⁰ The language and political perspective of Chartism, for example, were so thoroughly rooted in eighteenth-century ways of thinking that they could not be remade to cope with the give and take of politics in the 1840s or with the wave of capitalist expansion that began around that time. The radicalism – secular and republican but not especially social – that survived into the mid-Victorian era, moreover, was unable to stand up to the attractions of mass consumption and mass entertainment that became gradually more available after 1880 and that, by the Great War, had produced a more complacent, self-enclosed and passive working-class culture. The Labour party was formed at the turn of the century largely to protect that emerging, defensive culture but, Stedman Jones argues, the party had no distinctive vision of its own; hence, its triumph in 1945 produced nothing more than a series of statist measures inspired by a philanthropic brand of reforming liberalism.

The literature inspired by Thompson, but focused upon other national experiences, contains strong echoes of the original formulation. William Sewell, writing about roughly the same period as Thompson, claims that a "corporate" idiom based on notions of occupational and local solidarity dominated the mental world of French artisans and hence decisively influenced their experience of economic transformation and their, and later generations', response to it.¹¹ In Gutman's analysis, American working-

¹⁰ Stedman Jones contrasts his account and its focus upon language with that of Thompson, Hobsbawm and others influenced by Marxism. These claims to epistemological novelty notwithstanding, his approach remains remarkably similar in method to Thompson's. For one of several assessments of Stedman Jones' book, see my review essay "Language, Politics and the Critique of Social History", *Journal of Social History*, 20/1 (1986), pp. 177–184. The distance that lies between Thompson's work, with its emphasis on culture and political rhetoric, and that of more recent historians concerned with language, is also exaggerated by Joan Scott in her essay on Thompson in "Women and the Making of the English Working Class", in *Gender and the Politics of History* (New York: Columbia University Press, 1988), pp. 68–90, as is the distance separating Scott from Stedman Jones as claimed in her essay "On Language, Gender and Working-Class History", pp. 53–67. For a more general, critical, discussion of the linguistic turn in labor history, see Bryan Palmer, *Descent into Discourse: The Reification of Language and the Writing of Social History* (Philadelphia: Temple University Press, 1990).

¹¹ William Sewell, *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge University Press, 1980). For later adaptations of the language

class consciousness was shaped by the recurring confrontation between the nation's extremely rationalized and highly modern industrial culture and the basically peasant cultures that successive waves of immigrants brought with them to the United States, between, as he put it, "diverse preindustrial cultures and a changing and increasingly bureaucratized industrial society".¹² Inevitably, this diversity of ingredients, and the fact that they were added to the evolving consciousness of American workers at different points in time, prevented the emergence of a coherent popular culture or popular politics analogous to those visible at the national level in France or Britain.¹³ Provisional local syntheses were feasible, however, as in antebellum New York. Because a distinctly industrial culture had yet to take root in pre-Civil War New York, for example, the city's economic base remained artisanal, and popular attitudes radical and republican; and it was these notions, according to Sean Wilentz, that workers invested with newer, more social, implications in order to make sense of the capitalist society emerging around them.¹⁴

The internal logic informing such works is difficult to assail on its own terms. Clearly, it matters a great deal what beliefs and attitudes working people hold when they confront economic and social change; and surely these will have a profound impact upon the popular culture, the language and the politics that result from that engagement. And, of course, the richness, variety and viability of those beliefs and attitudes, so often lumped together indiscriminately and dismissively labelled "pre-industrial", have not been sufficiently appreciated by previous scholars.

of reform, see Judith Stone, *The Search for Social Peace: Reform Legislation in France, 1890-1914* (Albany: State University of New York Press, 1985).

¹² Herbert Gutman, *Work, Culture and Society in Industrializing America* (New York: Vintage, 1977), p. 67 and *passim*. The consequence of this interaction, not surprisingly, was not an enduring cultural predisposition but a series of recurring patterns that appeared discontinuous but shared a common underlying consistency. On the precise political languages and cultures that went into, and emerged from, these confrontations Gutman was very eclectic. Toward the end of his life he had some particularly interesting things to say about the relationship between republicanism and socialism. See the transcript of his interview with Mike Merrill reprinted in *Power and Culture*, ed. by Ira Berlin (New York: Pantheon, 1987), pp. 332-339.

¹³ Though the extent to which working-class culture was genuinely national in either Britain or France remains quite debatable. On the yearning for national synthesis in American labor history, but the frank recognition of its impossibility, see the essays collected in J. Carroll Moody and Alice Kessler-Harris (eds.), *Perspective on American Labor History: The Problem of Synthesis* (DeKalb, Ill.: Northern Illinois University Press, 1989). It seemed for a time as though a labor history written around the workplace and the question of control might provide the vehicle for a genuinely synthetic account, but that seems less attractive at a time when historians are made daily more aware of the gendered and racial character of the labor market. For the best effort in this direction, see David Montgomery, *The Fall of the House of Labor* (Cambridge University Press, 1987). On the centrality of race to definitions of working-class identity in the United States, see David Roediger, *The Wages of Whiteness: Race and the American Working Class* (London: Verso, 1991).

¹⁴ Sean Wilentz, *Chants Democratic: New York City and the Rise of the American Working Class, 1790-1850* (New York: Oxford University Press, 1984).

The question, however, is whether the logic embedded in such studies is adequate to the interpretive task for which it is used. If such studies aimed merely to point out an aspect of labor history that had previously been missed or to enrich the understanding of popular culture, it might be. But these impressive works would seem to aim at rather more than this. Their objective is to analyze what is uniquely decisive in giving shape to the English, the French, or the American labor movements as a whole, or in particularly important regions and at what are seen as decisive, and thus defining, moments. This is, however, an implicitly comparative question not unlike the question that has regularly been put by those seeking to explain "American exceptionalism", "the peculiarities of the English", or the unique character of the French or the Germans. For this grander objective the logic they typically employ would seem to be less helpful.

The most serious problem has to do with the concepts of culture and language so prominently deployed in these analyses. In Thompson the recourse to culture was in the first instance ideological, a way of asserting the autonomy and worthiness of the beliefs of ordinary people. That assertion was aimed at scholars of British historical development, of course, but was intended as well to reach those of more activist orientation whose vision of the capacity of workers was more constricted than Thompson's and whose political imagination was confined to the forms of collective action and the programmatic solutions evolved in the era of the Second and Third Internationals.¹⁵ For these purposes a vague, populist definition would do; and such an amorphous notion, when combined with the democratic impulses of a new generation of labor and social historians, served to open up to historical inquiry large fields of working-class experience that had previously been closed off and rendered invisible. But subsequent researchers have felt the need for more precise and sophisticated definitions, and for these scholars the inadequacies of existing notions of culture, whether those embedded in Marxist arguments about consciousness or in anthropological visions of social totality, proved decisive. Increasingly, they turned to the study of language, which in practice usually meant political rhetoric. But if Thompson's definition was overly broad and thus failed sufficiently to separate culture and thought from society, later formulations threatened to divorce culture, language and rhetoric from any social referent.¹⁶ Neither conception can stand up to critical scrutiny, for neither allows for a clear judgment about the distinctive contribution of

¹⁵ On the limited imagination displayed in socialist practice in the twentieth century, especially since the 1920s, see Geoff Eley, "Reviewing the Socialist Tradition", in Christiane Lemke and Gary Marks (eds.), *The Crisis of Socialism in Europe* (Durham: Duke University Press, 1992), pp. 21–60.

¹⁶ The problem, then, is drawing the appropriate boundaries around the concept of culture. Recent writing typically draws the boundary too narrowly and also too firmly; but others err in the opposite direction. See, for an example of the latter, Jonathan Prude's otherwise quite useful essay, "Directions of Labor History", *American Quarterly*, 42/1 (March 1990), pp. 136–144.

culture or language to working-class development. Language and culture, in consequence, often appear in these accounts to play the role that other factors – like the state, for example, in Katznelson and Zolberg's view – play in other efforts to account for the distinctiveness of each nation's labor history. But the focus remains on accounting for peculiarities and the method is again "operationalized" as a singling out of the one or another factor that, taken more or less in isolation, explains key national variations in the trajectory of labor's political development.

More practically, the focus upon culture had led to a style of analysis that is more descriptive than analytical. It has produced rich and fascinating readings of texts and rhetorics, and occasionally also of popular rituals, but these are seldom matched by comparably rich or sophisticated discussions of social and economic structure. The result is that several of the best works in this tradition are marked by a considerable imbalance between their treatment of culture and of the more material aspects of social relations. That, in turn, tends to undermine faith in the argument about the importance of culture vis-à-vis other factors in explaining the development of labor movements between and within countries. Together, these difficulties suggest that the focus upon cultural variation is less useful in analyzing labor's past than the current fashion for such work might lead one to believe.

Neither the most thoughtful and recent comparative work nor the most effective synthetic, largely cultural, accounts would seem therefore to offer a useful paradigm for comparative labor history. Recent efforts have undoubtedly succeeded in drawing attention to what are likely to be critical factors in any genuinely comparative account and they have yielded many specific insights. On balance, however, they have not managed to break fully and decisively with the logic of "exceptionalism" and for that reason do not allow for significant variation at all levels of the process of class formation. Most important, they do not allow for variation in the social and economic structures within which class relations take shape. This failure leaves us with two options: one is to abandon the hope of doing serious comparative analyses of labor; the other is to begin constructing a sounder basis for comparison. It is not unreasonable to argue against comparison altogether. Comparison, after all, presupposes some degree of comparability and thus of similarity, and it may well be a mistake to assume that the experience of workers at different times and places is similar at any level or that they face similar constraints and opportunities. Similarities might, of course, emerge empirically, but the assumption of similarity may preempt the discovery of crucial differences. It would seem safe, however, to assume that, at least for readers of this journal, the case for comparison has already been granted, and for that reason it would appear more helpful and appropriate to proceed to suggest a better way actually to do comparative labor history.

The question for those who would like to see the development of an intellectually compelling approach to comparative labor history is just how

to free ourselves from the related tendencies toward thinking in terms of exceptions from the common pattern and toward research focused primarily upon one or another factor in the process of class formation. Several things would seem to be necessary for this to happen. The first is a frank and thorough rejection of the assumptions underpinning the exceptionalist paradigm. As it happens, several of the “pernicious postulates” identified and criticized by Charles Tilly in his recent book are, in slightly modified form, precisely the assumptions that have for years been misleading labor historians. Thinking through his critique should help to provide at least a start toward accomplishing this first task and clear the way for the second, which is to begin to imagine both a wider range of outcomes to the evolution of labor history and a greater variety of factors or combinations of factors as causes of that diversity. Indeed, many of the arguments that, taken in isolation, have proved inadequate conceptualizations, might easily be transformed into quite useful hypotheses about the process of class formation. Devising research strategies to explore such ideas and to test such hypotheses is the third, and perhaps most difficult, task. It will require in the first instance figuring out how to categorize and measure the variety of economic and social structures as well as the political and cultural factors likely to be important. Since this effort will undoubtedly generate more information on a wider range of questions than has been characteristic of previous work in labor history, however, it will also require some method or set of principles through which to reduce the data to manageable proportions.

Let us, as briefly as possible, give some indications as to how these two problems might be solved and how, presumably, comparative labor history might make a bit more progress than it so far has. Probably the most daunting problem is how to gather and organize information on variations in social and economic structure. It is clear that a good deal of preliminary work must be done before we can confidently utilize existing sources of data – e.g., the census – to construct a reliable comparative mapping of social structures.¹⁷ Even the more limited study of comparative social mobility appears beset by grave data problems.¹⁸ Nevertheless, there are indirect approaches to the problem which could conceivably yield a much closer approximation to social structure than has thus far been possible. One route would be through the study of industrial structure, the second via the analysis of the labor market and its demography.¹⁹

There are a number of advantages to be had in exploring these avenues.

¹⁷ See Margo Conk, “Labor Statistics in the American and English Censuses: Making Some Invidious Comparisons”, *Journal of Social History*, 16 (1983), pp. 83–102.

¹⁸ See, for example, Hartmut Kaelble, *Social Mobility in the 19th and 20th Centuries: Europe and America in Comparative Perspective* (New York: St. Martin’s, 1985).

¹⁹ Some of the suggestions offered below echo those made previously by David Brody in “Labor History, Industrial Relations, and the Crisis of American Labor”, *Industrial and Labor Relations Review*, 43/1 (October 1989), pp. 7–18.

First and most obvious, such approaches can help us to overcome the tendency for the analysis of social structure to get bogged down in the problem of how to aggregate vast numbers of individuals or households into meaningful groupings. There are far fewer firms than people, and labor markets can be described at modest levels of aggregation and disaggregation. Even if one believes – as the fashion seems to dictate these days – that the way forward in the study of class politics and collective action leads through methodological individualism, it should still be possible to advance the study of social structure by other means.²⁰ The second and more practical advantage is that, as a result of recent research, we now know much more about both of these aspects of social and economic history. On industrial structure, for example, there is much to be learned from the so-called “new business history” associated with the work of Alfred Chandler and from those less historical social scientists who analyze the different technologies and management systems adopted by employers in different firms and sectors. The debate about the transition from Fordist to post-Fordist technologies – a rather grandiose variation on this approach – has also produced useful empirical work on the changing structure of industry.²¹ We are even better served by the array of research relevant to understanding the structure of labor markets. One key strand has been generated by economists seeking to understand inequality by studying the segmentation of labor markets.²² A second has been produced by scholars concerned with the sexual division of labor.²³ A third, and related, body of research has focused upon proto-industrialization, family strategy and the demographic origins of the proletariat.²⁴ The extensive

²⁰ The argument in favor of methodological individualism has been put most forcefully by Jon Elster. See, for example, his “Marxism, Functionalism and Game Theory: The Case for Methodological Individualism”, *Theory and Society*, 11 (1982), pp. 453–482; and *Making Sense of Marx* (Cambridge University Press, 1985); and also Przeworski, *Capitalism and Social Democracy*.

²¹ See, for example, Michael Piore and Charles Sabel, *The Second Industrial Divide* (New York: Basic, 1984).

²² David Gordon, Richard Edwards and Michael Reich, *Segmented Work, Divided Workers: The Historical Transformation of Labor in the United States* (Cambridge University Press, 1982). Cf. also Mike Davis, *Prisoners of the American Dream: Politics and Economy in the History of the U.S. Working Class* (London: Verso, 1986).

²³ See, among others, Sylvia Walby, *Patriarchy at Work* (Cambridge: Polity, 1986); Wally Secombe, “Patriarchy Stabilized: The Construction of the Male Breadwinner Wage Norm in 19th-century Britain”, *Social History*, 11 (Spring 1986), pp. 53–76; Sonya Rose, “Gender Antagonism and Class Conflict: Exclusionary Strategies of Male Trade Unionists in 19th-Century Britain”, *Social History*, 13 (May 1988), pp. 191–208; and Mike Savage, “Trade Unions, Sex Segregation and the State: Women’s Employment in the ‘New Industries’ in Inter-War Britain”, *Social History*, 13 (May 1988), pp. 209–230.

²⁴ Jane Humphries, “Enclosures, Common Rights and Women: The Proletarianization of Families in the late 18th and early 19th Centuries”, *Journal of Economic History*, 50 (March 1990), pp. 17–42; David Levine, “Recombinant Family Formation Strategies”, *Journal of Historical Sociology*, 2 (1989), pp. 89–115; W. Secombe, “The West European Marriage Pattern in Historical Perspective: A Response to David Levine”, *Journal of Historical Soci-*

literature on immigration and urbanization likewise provides considerable material on the evolving social structure of the working class.²⁵ Even if it is not possible, as surely it will not be, to accept the interpretations elaborated within these fields of inquiry, their cumulative findings can be mined to produce a wealth of new data which historians of labor can use to construct far subtler and more detailed class maps than has hitherto been possible.²⁶

The problem of making sense of these additional sources of information for comparative labor history is at the same time easier and yet riskier than actually getting hold of the data. It is easier because what is mainly involved is making some choices about what sorts of meaning one wants to extract from the data. It is riskier, of course, because one's choice might turn out to have been wrong and to lead in the wrong direction. Still, if one is to make any sense at all of the material, it is impossible to avoid making such a choice. Ultimately, the choice will come down to deciding upon an interpretive framework and that, it seems to me, will require that we fashion a plausible, working hypothesis about how the various levels of class formation, or the factors conditioning the evolution of labor, interact and combine. The sense of the matter to emerge from this review is that the most useful interpretive framework will have at its core a concern with the problem of organization, resources and "class capacity".²⁷ A great deal, perhaps the bulk, of writing on labor history has concerned itself either with the structuring of interests or the production of consciousness. Labor history would surely be very impoverished without some notion of interest and some attention to consciousness, but both of these concerns have been so mixed up with political and ideological debates that at present they tend more to obscure than to illuminate the actual history of labor. A concern with organization and the mobilization of resources can be compatible, of course, with quite strong views about the rights and wrongs of labor, but it is not so easily overwhelmed by them.

What would a concern for organization and resource mobilization mean? In a narrow sense, it would mean looking at the level and character of working-class organization as an important fact in and of itself.²⁸ This

ology, 3 (1990), pp. 50-74; and Charles Tilly, "Demographic Origins of the European Proletariat", in D. Levine (ed.), *Proletarianization and Family Life* (New York: Academic Press, 1984); and Tilly, "Flows of Capital and Forms of Industry in Europe", *Theory and Society*, 12/2 (March 1983), pp. 123-142.

²⁵ Stephen Castles and Godula Kosack, *Immigrant Workers and Class Structure in Western Europe*, 2nd edn (Oxford University Press, 1985); Paul Hohenberg and Lynn Lees, *The Making of Urban Europe, 1000-1950* (Cambridge, Mass.: Harvard University Press, 1985).

²⁶ One useful, if not fully convincing, effort in this direction is Charles Sabel's *Work and Politics: The Division of Labor in Industry* (Cambridge University Press, 1982). Another helpful, if again provisional, approach is Erik Wright, *Classes* (London: Verso, 1985).

²⁷ Andrew Levine and Erik Olin Wright, "Rationality and Class Struggle", *New Left Review*, 123 (September/October 1980), pp. 47-68.

²⁸ It is interesting that those scholars who pay closest attention to variations in the strength and structure of labor organization have done so in order to assess the impact of labor

importance is premised on the assumption that it is organizational networks which mediate between the social and economic realm and the realm of politics. More broadly, however, thinking in terms of organization and resources means asking different sorts of questions about the impact of society and economy upon politics. It means asking how social and economic structures affect the capacity of working people to organize and act collectively, rather than asking, as has so often been done in the past, how social and economic structure produced or altered consciousness. More broadly still, thinking about organization necessarily involves bringing more decisively into labor history those actors who oppose or ally with workers: principally employers and the state, but occasionally other organized groups representing peasants, farmers, or the middle classes.

It does not mean attempting to write "total history" or the history of society as a whole, or histories that vainly seek to encompass the separate but related histories of workers and, say, the bourgeoisie.²⁹ Indeed, the focus on organization provides a method for slicing into those other histories in ways that do not require us to attempt such impossible feats. Nor does the concern for organization mean returning to an older style of institutional labor history in which the history of the workers could be written as the history of the organizations seeking to represent them. Nor should it entail writing labor's history in the rather sterile terms employed by the industrial relations expert.³⁰ A history of labor sensitive to questions of organization and organizational resources need not glorify organizations or their leaders, or ignore the problematic relationship that seems always to obtain between leaders and the rank-and-file.³¹ And it does not mean ignoring the history of the unorganized – of women, the less skilled or

organization on politics, particularly social democratic politics, and on the creation of the welfare state. See the literature cited in Michael Shalev, "The Social Democratic Model and Beyond: Two 'Generations' of Comparative Research on the Welfare State", *Comparative Social Research*, 6 (1983), pp. 315–351; and also Gösta Esping-Andersen, *Politics against Markets* (Princeton University Press, 1985); J. R. Hollingsworth and R. Hanneman, "Working-Class Power and the Political Economy of Western Capitalist Societies", *Comparative Social Research*, 5 (1982), pp. 61–80; John Stephens, *The Transition from Capitalism to Socialism* (London: Macmillan, 1979); and Gary Marks, *Unions in Politics: Britain, Germany and the United States in the Nineteenth and Twentieth Centuries* (Princeton University Press, 1989). For an approach critical of this literature, see Peter Baldwin, *The Politics of Social Solidarity: Class Bases of the Welfare State, 1875–1975* (Cambridge University Press, 1990).

²⁹ Though the effort is more feasible when confined to a particular region or locality. See, for example, Theodore Koditschek, *Class Formation and Urban-Industrial Society: Bradford, 1750–1850* (Cambridge University Press, 1990).

³⁰ As would seem to be the point of Jonathan Zeitlin, "From Labour History to the History of Industrial Relations", *Economic History Review*, 40 (1987), pp. 159–180.

³¹ Charles Sabel, "The Internal Politics of Trade Unions", in Suzanne Berger (ed.), *Organizing Interests in Western Europe* (Cambridge University Press, 1982); Mark Leier, "Which Side Are They On? Some Suggestions for the Labour Bureaucracy Debate", *International Review of Social History*, 36 (1991), pp. 412–427.

migrant workers – for being concerned with organization means asking why some groups have been poorly organized as well as asking why and when other groups were able to organize. In short, research informed by an organizational perspective can make sense of a broader range of phenomena because it offers a common set of questions by which to relate them to one another.

It would be rather presumptuous to pretend that these suggestions add up to a formula guaranteed to produce successful comparative studies of labor. A great deal will depend upon the tractability of the data that can be put together and upon the answers that questions about organization can elicit from the data. Much will depend, too, upon who, if anyone, decides to undertake such work. Unfortunately, there is not a great deal of intellectual or material support for comparative social or labor history among historians just now.³² There is, instead, considerable demoralization among Western labor historians, partly due to the crisis that has overtaken labor itself and partly due to the changing fashions of the profession.³³ It may well fall to historians of labor outside Europe and North America, therefore, to develop more compelling approaches to the comparative study of labor. Such scholars begin, it strikes me, with two obvious advantages: the first is their ability to absorb and transcend the findings and frameworks of research centred on Europe and the United States; the second is their evidently greater willingness to make use of data and concepts on offer from social scientists doing related research.³⁴ In fact, the other possible locus of innovation in comparative labor history is among the several clusters of historically minded economists, sociologists and political scientists who are working within their disciplines to reassess labor's role in economy, society and politics; and it is quite possible, likely even, that they will bring to the task different questions and research tools than we, as historians, would imagine or propose. Still, it would seem

³² It must be conceded, however, that even when support has been available, the task of producing genuinely comparative work has proved extremely difficult. See, for example, the three recent sets of conference papers published on comparative labor history: W. J. Mommsen and H.-G. Husung (eds.), *The Development of Trade Unionism in Great Britain and Germany, 1880–1914* (London: Allen & Unwin, 1985); and Leopold Haimson and Charles Tilly (eds.), *Strikes, Wars and Revolutions in an International Perspective* (Cambridge University Press, 1989); and Leopold Haimson and Giulio Sapelli (eds.), *Strikes, Social Conflict and the First World War: An International Perspective* (Milan: Fondazione Giangiacomo Feltrinelli, 1992). In the three collections, the individual papers are typically of high quality but seldom venture beyond national boundaries. There are currently two projects underway on comparative labor history – one sponsored by the Fernand Braudel Center in Binghamton and the other by the International Institute of Social History in Amsterdam – but the results have yet to appear.

³³ Eric Arnesen, "Crusades against Crisis", *International Review of Social History*, 35 (1991), pp. 106–127.

³⁴ Charles Bergquist, *Labor in Latin America* (Stanford University Press, 1986); and "Latin American Labour History in Comparative Perspective: Notes on the Insidiousness of Cultural Imperialism", *Labour/Le Travail*, 25 (Spring 1990), pp. 189–198.

from this review that there is likely to be considerable interest and sympathy among labor historians toward comparative approaches and that there are at least some means at hand by which historians and scholars working in other disciplines can produce better comparative work than has so far been done. Overall, then, the prospects are not bad.