James E. Cronin

THE "RANK AND FILE" AND THE SOCIAL HISTORY OF THE WORKING CLASS

Labor history continues to progress. It continues, too, to transform itself from a field staffed by enthusiasts and partisans into a discipline peopled with scholars sympathetic to their subject but also highly conscious of the methods, theories and interpretive frameworks within which they work.[1] The last few years in particular have witnessed a considerable intensification of scholarly controversy within labor history and a growing sophistication of debate. To some extent the discussion parallels the ongoing conversation among social historians about the boundaries of social history, the power of primarily social explanation and the role of language, culture and politics in social history.[2] Unfortunately, however, the stepping up of debate in labor history coincides with a dip – temporary, one hopes, but perhaps more long-term – in the fortunes of labor movements themselves. This conjuncture seems to have produced a sense of pessimism and malaise as the sentimental accompaniment to many of the debates within labor history.

Jonathan Zeitlin's essay on "rank and filism" illustrates each of these tendencies.[3] In its vigor, clarity and sophistication it is a most welcome addition to the debate within labor history; in its tone, however, it is an indication of the less than happy mood prevalent among labor activists and scholars of the labor movement. This combination creates a major intellectual problem for Zeitlin, for it means that his pessimistic tone tends to overwhelm his often quite sensible analysis, and imparts to his argument an overly negative attitude toward previous work on the history of labor, British labor in particular. In this essay I should like to assess Zeitlin's piece

[1] See Eric Hobsbawm, "Labour History and Ideology", in *Worlds of Labor* (New York, 1984).

[2] See Geoff Eley and Keith Nield, "Why Does Social History Ignore Politics?" *Social History*, 5 (May, 1980); Gareth Stedman Jones, *Languages of Class* (Cambridge, 1984); and J. Cronin, "Language, Politics and the Critique of Social History", *Journal of Social History*, XX, 1 (Autumn, 1986), pp. 177-184.

[3] Jonathan Zeitlin, " 'Rank and Filism' in British Labour History: A Critique", *International Review of Social History*, this volume, pp. 42-61. The arguments in Zeitlin's article were to some extent anticipated in arguments between Zeitlin and Alastair Reid on the one hand, and Richard Price, Richard Hyman, Keith Burgess and myself on the other at a meeting held in 1981 on the development of trade unionism. For the various positions, see the articles in Hans Mommsen and H.-G. Husung (eds), *The Development of Trade Unionism in Great Britain and Germany, 1880-1914* (London, 1985).

International Review of Social History, XXXIV (1989), pp. 78-88

and to dissent specifically from his evaluation of recent trends in labor history and from his implicit reading of the developing historiography of labor. I should then like to offer a few ideas of my own on where labor history has been headed of late, and of how we might best approach its future.

The starting point for Zeitlin's critique is his assertion that a number of recent and, in some cases, much-acclaimed studies of British labor have been marked by an undue focus on, and exaggeration of the role of, the rank and file of the trade unions. This tendency, tagged "rank and filism" by Zeitlin, allegedly has led historians into a number of interpretive mistakes. It has led some, like Richard Price, to overestimate the extent of control exercised by workers over the labor process and to overemphasize the importance of employers' efforts to break that control, and workers' efforts to protect it, in the general evolution of labor.[4] It has also biased labor historians, like James Hinton and, again, Price, against the leaders of the unions and against the state.[5] This has in turn caused these authors to interpret the strategies of leaders and the interventions of the state as efforts to coopt rank-and-file militancy and hence to reinforce managerial control of the labor process. "Rank and filism" has also, Zeitlin would argue, led historians like Hinton, Price, Bob Holton, Keith Burgess, Joe White, Richard Hyman and myself, to devote excessive attention to those episodes in the history of labor characterized by unusually prominent outbursts of rank-and-file activity, often in opposition to the wishes of the leadership.[6] These several mistakes are severe enough, according to Zeitlin, to call into question the entire "rank-and-filist" approach and the results of the many studies in which it is supposedly embodied. As he puts it, "the rank-and-filist paradigm is fundamentally unsatisfactory and should be abandoned outright".

This is a harsh judgement, so harsh as to require some examination of what lay behind it. It is certainly reasonable to criticize any or all of the works in question and to take issue with the interpretive framework within

[4] Richard Price, *Masters, Unions and Men* (Cambridge, 1980); "Rethinking Labour History: The Importance of Work", in J. Cronin and J. Schneer (eds), *Social Conflict and the Political Order in Modern Britain* (New Brunswick, 1982); and "The New Unionism and the Labour Process", in Mommsen and Husung, *Development of Trade Unionism*, pp. 133-149.

[5] James Hinton, *The First Shop Stewards' Movement* (London, 1983).

[6] Bob Holton, *British Syndicalism, 1900-1914* (London, 1976); Keith Burgess, *The Challenge of Labour* (London, 1980); Joseph White, *The Limits of Trade Union Militancy* (Westport, CT, 1978); Richard Hyman, *The Workers Union* (Oxford, 1971); and "Mass Organization and Militancy in Britain: Contrasts and Continuities", in Mommsen and Husung, *Development of Trade Unionism*, pp. 250-265; and J. Cronin, *Industrial Conflict in Modern Britain* (London, 1979).

which they have developed. But it should be recognized that such a blanket critique and rejection amounts to a summary dismissal of the research and writing of almost an entire generation of labor historians. Hence it represents a kind of historiographical nihilism that, in my opinion, cannot avoid being somewhat unfair and one-sided. The lack of fairness in Zeitlin's critique consists primarily in the fact that he reduces the richness and variety of the writing he criticises into a series of elaborations on, or illustrations of, a single flawed theory: "rank and filism". This method of reasoning allows Zeitlin to treat the results of "rank-and-filist" research as suspect and to pass lightly over, indeed virtually ignore, its empirical findings.

Zeitlin's lack of interest in the empirical results, and hence in the empirical supports of "rank and filism" is very disturbing, and it produces some odd statements. Early on in his paper, for example, Zeitlin writes about the concentration of "rank-and-filist" research on the period 1910-20, when a series of movements – "oppositional and often minoritarian", he disparagingly calls them – blossomed within and around the labor movement. These movements were openly critical of union officials and union structures, and they offered visions of alternative forms of working-class organization; their leaders and spokesmen often played prominent roles in the widespread strikes and protests of that era; and, as Zeitlin admits, they elicited "distressed reactions" from "politicians, civil servants and liberal intellectuals". To Zeitlin, however, the contemporary evidence – e.g., quotations from workers, activists, newspaper writers, officials and so on – testifying to the impact of these insurgent movements is not to be taken as proof of their reality. Rather, it is a measure of the confusion of the period, in which relatively minor movements were given unwarranted attention because their visibility happened to coincide with an intense competition between the "ideological discourses of revolutionaries and government".

This is a peculiar caution to direct at labor historians. Surely it is possible to be misled by the evidence, but surely the greater danger is to ignore or devalue it. Indeed, imagine the strictures to which a would-be "rank and filist" would be subjected if he or she were to disregard the contemporary documentation and to claim on some other basis that workers held oppositional ideas despite their repeatedly saying that they did not! Clearly there is room for a discussion of just how to approach particular kinds of evidence, but whatever the appropriate epistemological stance for this sort of work, it would seem highly questionable to preface a critique of an entire body of interpretation by preemptively dismissing the evidence in favor of it. Yet this is precisely what Zeitlin does as he makes his way toward criticising the assumptions which, he feels, underpin "rank and filism". The reason for this unusual procedure, it would seem, is that Zeitlin wants to get quickly to the heart of his critique, which is more theoretical – one might even say ideological – in character. Hence he does not linger over the findings of "rank-and-filist" research, but neatly reduces them, and it, to two basic assumptions.

What are these assumptions? Zeitlin claims that the research he labels "rank-and-filist" is based first of all upon the assumption that unions have an interest in, and a propensity toward, accommodation with management whereas ordinary workers do not. Unions are thus seen as institutions that constrain workers' inherent militancy and that help tie them to capitalism. The second assumption underlying "rank and filism" is that the working class is "endowed with a vast reservoir of latent power which is contained by the institutions which represent them". From this derive the critical judgements said to characterize "rank-and-filist" research, for without a theory ascribing to workers the potential for acting outside the framework of institutionalized collective bargaining, there would not be much point in criticising union leaders for defending those institutions and the boundaries and limitations they entail.

These assumptions are clearly unacceptable to Zeitlin, but of course, formulated in so stark a fashion they would probably be unacceptable to most historians of labor. The key question is therefore whether these assumptions are in fact held by any or all of the people whose work Zeitlin seeks to criticise. On this issue Zeitlin wavers quite a bit. He concedes, for instance, that "few historians would today put the point [about workers' interests in opposing capitalism] so baldly" and that "[m]any of the [. . .] difficulties" with this sort of analysis have been, or would be, "acknowledged in some form by the more sophisticated writers within this tradition itself". But if this is so, why should we accept Zeitlin's claim that their work is now, or at some earlier point was, based upon the implicit acceptance of these assumptions? Zeitlin does not bother to show these assumptions at work in the research and writing of his opponents. He simply argues that "rank and filists" must have held to them in order to "lend plausibility to a 'rank and filists' analysis" and because, as he puts it, "without the imputation to workers of such objective anti-capitalist interests, it is hard to see on what basis one might disregard the myriad empirical concerns which bind workers to their employers in a relationship of conflictual cooperation [. . .]".

Note again Zeitlin's unusual critical procedure. Having largely dismissed the evidence offered by "rank and filists" and thus bypassed their substantive findings, he claims to have divined the implicit assumptions behind their work. But in the next breath he admits that these assumptions are not held in anything like the form he expresses them by leading, or at any rate the more sophisticated, spokesmen of this tradition. He then proceeds to admit that his real reason for ascribing such beliefs to them is that he, Zeitlin, fails to understand how they could write what they write in the absence of such assumptions. But remember that Zeitlin has yet to review for his readers in any detail the actual writing and findings to which he objects. For some reason Zeitlin feels he can argue about the flawed assumptions behind "rank and filism" despite a nearly total disregard for what has actually been written.

Nor does Zeitlin use the remainder of his essay to provide readers with a clear sense of what the "rank and filists" say and what, in precise terms, is wrong with it. Indeed, having gotten to the essence of the "rank-and-filist" fallacy, Zeitlin decides that this error is too well-known to merit further discussion. He focuses instead upon a series of problems that he claims arise when applying a "rank-and-filist" perspective to the question of "the relationship between trade unions and job control". Among these are the problems of defining who it is that makes up the rank and file and officialdom in any particular situation; the difficulty of identifying these groups, however defined, with support for, or opposition to, militancy and radicalism; the problem of assessing whether institutional structures, such as conciliation or arbitration procedures or sliding scales, help or hinder workers' control over the labor process and, conversely, whether "autonomous regulation" by craft workers could by itself serve as a viable approach to job control over the long term; and, finally, the problem of explaining those many instances when union officials showed themselves responsive to pressures from below. These various problems are illustrated by snippets of information, drawn primarily from Zeitlin's own research into the history of engineering, that he claims are not easily assimilated into a "rank-and-filist" approach.

The problem with this style of critique, of course, is that there is no way of knowing whether these difficulties do in fact constitute real obstacles to "rank and filists" without examining more closely what they have written. That Zeitlin does not do, however. Still, it seems safe to say that, on the surface at least, these allegedly awkward facts do not, or would not, appear to pose any insoluble dilemmas for "rank and filists". It is no great discovery, after all, to find that the dividing line between officials and members was often blurred and shifting and that the precise interaction between them has varied with the internal structure of the union. Nor would it surprise most people, including the most resolute "rank and filist", to learn that "autonomous regulation" was not always effective or that bargaining procedures often enhanced workers' control at the workplace, at least for a time. Similarly, the fact that officials were sometimes more militant or more politically radical than their members is one of those facts that every student of labor history knows but one which, at the same time, hardly disproves the general point that leaders tend over time to become more cautious than those they lead.

As critique, therefore, Zeitlin's piece is extremely unsatisfying, largely because it fails to convey what it is that his opponents have done right and wrong. Instead, we get a claim about what writers in the "rank-and-filist" tradition really mean, qualified by disclaimers exempting most such writers from the charge of actually saying so, and followed by a brief rendition of certain empirical difficulties with which a "rank-and-filist" analysis would have to deal. Finally, Zeitlin ends his essay by offering his alternative to "rank and filism".[7] Rather than regard union leaders and members as divided into opposed camps with divergent interests, Zeitlin suggests that we view unions as institutions torn between conflicting tendencies and imperatives both internally and externally. Externally, unions have to decide whether to adopt a more oppositional or cooperative stance toward employers; internally, unions have to strike a balance between the need to centralize authority and policy-making and the need to mobilize their membership. These tendencies give rise, in Zeitlin's view, to recurring factional disputes within unions that are often fought out with rhetorical appeal to the rank and file and their interests. The adoption of such rhetoric is a tactical device, however, and not a reflection of what is going on within the union or on the shopfloor.

Two aspects of this alternative formulation are particularly striking to this reader. First, it is hard to imagine anyone disagreeing with these very general, and generally unobjectionable, propositions. Of course unions must compromise with employers and of course there is a potential contradiction between centralization and mobilization; and naturally there are factional fights that make use of democratic rhetoric without necessarily reflecting a broad rank-and-file movement. Anyone who has studied labor's past knows these things, and it is difficult to see how recognizing them can provide much of a basis for distinguishing between varying interpretations within labor history.

The second curious fact about Zeitlin's alternative is its narrow focus. It is put forward explicitly as a better way to understand internal conflict within unions. But is this really the point? Are those scholars whom Zeitlin criticises themselves primarily interested in explaining internal disputes

[7] Zeitlin's alternative is derived largely, as he himself says, from Charles Sabel, "The Internal Politics of Trade Unions", in Suzanne Berger (ed.), *Organizing Interests in Western Europe* (Cambridge, 1982). It should be noted, however, that the uses to which Sabel puts his argument are almost the exact opposite of those to which Zeitlin puts it. Sabel is arguing that corporatist models of political bargaining between strong and stable organizations of workers and employers are misleading and inapplicable precisely because they overestimate the capacity of union leaders to deliver the continued support of the rank-and-file for policies agreed at the top. This is clearly an argument that has at least as much in common with a "rank-and-filist" view of unions as with Zeitlin's more anodyne perspective.

within trade unions? My sense is that they are interested in the history of the working class much more broadly conceived, and that Zeitlin's narrowing of their concerns betokens a quite fundamental failure to come to grips with what his opponents are trying to say and do. Let me try very briefly to explain what, as I understand it, those portrayed by Zeitlin as "rank and filists" are attempting, and thus to suggest a different way of evaluating that project.

Probably the best way to grasp what "rank and filists" have been doing is to discuss their work in terms of the evolving historiography of labor. Surely Zeitlin is right to note that politically many of the "rank and filists" were involved in or sympathetic to the new left and other protest movements of the 1960s and early 1970s. "Rank and filists", of course, shared this orientation with much of their generation of labor and social historians. This orientation contained within it, however, only the vaguest outlines of a theory of labor's history. What it did imply was a confrontation with the existing literature on labor and the working class. Labor history in the 1950s and 1960s, it will be remembered, was still largely being written as the institutional history of trade unions or socialist groups or parties. This restricted vision, which largely excluded the activities, beliefs and daily lives of working people, was shared, moreover, by marxist and non-marxist historians alike. It was against this background that Edward Thompson and Eric Hobsbawm wrote; and it was largely the broadening of the boundaries of labor history implied by their work that so attracted younger historians. No doubt the intellectual force of their arguments also impressed itself upon the new generation, but the most prized aspect of Hobsbawm's and Thompson's contributions was the model it offered for others to emulate.

In this sense labor historians were merely part, albeit an important part, of the broader movement toward social history. With other social historians studying mobility and class structure, women, the family, the city, popular culture and so on, labor historians shared the desire to develop an approach, a methodology, to recover the lives of ordinary people and their beliefs and practices. All of these efforts therefore shared common, but rather vague and imprecise, political roots. To be sure, there were some social and labor historians who held to more explicit ideological positions that predisposed them to discovering among ordinary people a suppressed history of resistance and struggle. And there were others who, though unsure of what they would find in their research, were nevertheless ready to reject the comforting myths of 1950s-style industrial relations research, which tended to see labor conflict as aberrant, pathological and, in any event, transitional, and which saw collective bargaining as capable of managing and muting almost all potential conflict between labor and capital. For most of the younger generation of historians, however, an alternative interpretive framework remained inchoate, little more than an intuition. What they were quite clear on, though, was the inadequacy of older styles of research and analysis. The labor historians who began their research in the late 1960s and 1970s, many of whose efforts Zeitlin criticises, were thus united less by a common set of assumptions than by a shared *historiographic critique*.

This critique led them to espouse, in opposition to the extant historiography of labor, a broader investigation into the social history of the working class. For some, that meant looking at groups typically ignored by labor historians: the unskilled, the unorganized, immigrants, racial and ethnic minorities and working women; to others this goal dictated a focus upon the beliefs and culture – as opposed to the conditions and interests – of working people. To another group it meant turning away from the workplace and toward those aspects of life centered in the community. Still others decided to look in more detail at collective action – at riots, demonstrations and strikes – in the hope of getting a better sense of what motivated workers and what circumstances facilitated or hindered their efforts to act together. Yet another important cluster of researchers felt that, despite the orientation of previous labor history towards workers' economic interests and collective organization, very little was in fact known about workers' lives at work. This interest in work, prompted in the first instance by a reaction to the existing historiography, was further intensified by the impact both in Britain and the US of Harry Braverman's *Labor and Monopoly Capital* (1974).[8]

Zeitlin's criticisms are directed mainly at scholars working on the latter two topics, i.e., on industrial conflict and on the labor process. He errs, however, in concentrating on these two distinct, if overlapping, groups in isolation from those undertaking roughly parallel inquiries on other aspects of the social history of the working class. This has the effect of detaching these scholars and their studies from the broader historiographic movement of which they are a part. Such a procedure makes it possible to impute an ideological and theoretical unity to these scholars, as Zeitlin has done, but in so doing it simultaneously exaggerates the similarities in their outlooks and interpretations and misses the many connections between these scholars and the field as a whole.

At a minimum, therefore, a more balanced evaluation of the "rank-and-filist" tradition would not treat it as a self-contained field operating with a unified theory, but would assess its distinctive contribution to the overall project of writing the social history of the working class. The particulars of such an assessment are beyond the scope of this essay. Judging by the reception given to specific books and articles within the field, however, I would suggest that in broad terms it would be agreed that "rank and filists"

[8] Harry Braverman, *Labor and Monopoly Capital* (New York, 1974).

have enormously enriched our understanding of the daily life of workers, of the problems they face in the labor market and at work, of the strategies and attitudes of employers, and of the opportunities different working environments and economic conjunctures have presented for organization and collective action.

Such a positive assessment would presumably be reinforced by placing the results of various "rank-and-filist" studies alongside the equally important findings of social historians writing about working-class culture, about family life and gender relations among working people, and about migration, mobility and the shifting demographics of the class. Taken together, these diverse studies have advanced the project of creating a social history of the working class on many fronts. A great deal remains to be done, of course: we still know far too little, for example, about working-class women and their roles in the workplace, the family and the community; we know less about ethnic and racial divisions within the British working class than we should; and we know surprisingly little about the impact of the state upon working people's lives or, to turn the matter around, about when and where and on what terms working-class men and women confronted political authority. Perhaps even more important, labor historians' sense of the lives of those around the edges of the working class is quite undeveloped – it seems as if the preoccupation with the question of the labor aristocracy has obscured the more general problem of establishing the boundaries and charting the interactions between the working class and the lower-middle class, a problem that, over the course of the twentieth century, becomes transformed into the question of the relationship between manual and white-collar employees. And finally, it will be obvious to all those in the field that even the most nuanced social-historical investigations of the working class do not by themselves tells us much about working-class politics; and that the analysis of working-class politics requires both separate studies of political life as such and major theoretical efforts to link politics and social life.

It is likely, of course, that filling in the current gaps in our knowledge will require new research foci, and perhaps whole new frameworks, for labor historians. It is likely, too, that some of the lines of research that have been popular over the past decade or so might not in future yield a return commensurate with the effort entailed. It is possible, more specifically, that some of the styles of research and argumentation that Zeitlin criticises will fall in this category. On the other hand, it is extraordinarily difficult to predict what lines of investigation will bear fruit and which will not; and it would seem foolish, and possibly quite embarrassing, to foreclose the possibility of truly creative work being done on what would appear to be well-worn topics.

Locating "rank and filism" within a broader historiography thus leads to a more generous and, hopefully, more realistic appraisal. Reviewing it in conjunction with a wider range of research on the social history of the working class brings with it yet another benefit, for it gives us some clues about the current malaise among labor historians – the malaise to which Zeitlin's piece at least indirectly addresses itself. It is impossible to begin to review, even to list, the products of this research effort without feeling that labor history has in effect burst its previous boundaries, and without wondering how, if at all, it is to be put back together again into a coherent narrative whole. The problem of reintegrating the component parts of labor's history is made more difficult still by the declining popularity of some of the old, unifying metaphors about "the labor movement", with its "industrial" and "political wings" engaged in a long "forward march"; and from the reluctance of younger labor historians to make use of marxist models of class consciousness. This suggests to me that much of the current debate about labor history is really a complaint about its fragmentation and the absence of any model or metaphor with which to overcome it. It would seem in particular that such a concern lies behind the desire of some social historians to return to the study of politics via the analysis of language, for the study of language appears to many as a way to get beyond the empirical messiness produced by the cumulation of recent social-historical investigations of the working class.

But are the fragmentation, the untidiness and the lack of an overarching model really such a big problem? It seems to me there is a case for arguing that they are not. Fragmentation, I would propose, is in some sense a precondition for progress, a necessary corollary of the effort to bring women, the unskilled and ethnic minorities into the working class and of the attempt to look beyond unions and parties into other spheres of private and public, if not necessarily political, life. Calls for integrating frameworks and for synthesis, on the other hand, strike me as premature calls for intellectual closure, or, even worse, as evidence of a perhaps unconscious desire to return to the days when we could leave out women and minorities, when we could be confident of what workers thought and felt and needed and of the future progress of the "labor movement", and when we could employ marxist models without all the hedgings and qualifications that seem so necessary today. This is not to say that we do not need theory, nor that we cannot make excellent use of synthetic accounts, however provisional they might be. But theory and synthesis should be built upon a recognition of the achievements of recent research.[9] Whatever its deficiencies, that research

[9] Settling upon a useful theoretical approach to labor history is no easy task, but my instinct at this point would be to follow up the leads contained in work by various scholars writing about class formation. A good example is Ira Katznelson and Aristide Zolberg

represented in its time a major advance over the banalities of the so-called "Oxford school" of industrial relations, to which Zeitlin would apparently like us to return, and produced a wealth of empirical findings upon which to base further generalization and argument.[10] To move forward in this fashion, however, would require a more catholic appreciation that Zeitlin displays in his essay and a greater openness to the current variety, perhaps even confusion, of research styles and agendas within the broader effort to write the social history of the working class.

(eds), *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States* (Princeton, 1986). I have tried to make use of a similar approach in *Labour and Society in Britain, 1918-1979* (London, 1984).

[10] See Zeitlin's "From Labour History to the History of Industrial Relations", 2nd series, *Economic History Review*, XL, 2 (1987), pp. 159-84.