

“WHAT’S IN A NAME?”  
WORKPLACE HISTORY AND “RANK AND FILISM”

Historical truth, like other kinds of intellectual understanding, proceeds through argument and discussion. Thus, we should be grateful to Jonathan Zeitlin for his rigorous responses to a body of scholarship which he categorizes as “rank-and-filist”. Although much of Zeitlin’s argument is unexceptional, its proposal to consign as erroneous and irrelevant the scholarship that fits his “rank-and-filist” paradigm requires some examination.<sup>1</sup> Most troubling are the procedures he uses to make his case; the prescriptive advice he offers though not without merit is also highly problematic. I shall discuss these objections in a moment. But first we must define the problem as Zeitlin sees it.

Zeitlin begins by describing rank and filist history as history that seeks the authentic experience of the worker, is critical of labour’s institutions, and is infused with the new left spirit of 1968. Painting in very broad strokes Zeitlin then goes on to attribute a variety of characteristics to such historians: they are all leftists drawn to turbulent periods of British history, they focus upon oppositional currents within the labour movement, and they *implicitly* (my italics) subscribe to certain problematic assumptions about trade union collaborationism and the constraints it imposes upon latent worker power. The clear impression that Zeitlin conveys is that “rank and filism” comprises a fully fledged “theory” of labour history and for the next twenty pages or so he proceeds to argue as if this was the case. Yet just as he is about to briefly outline his own prescriptive suggestions, a sudden narrowing occurs. Having undermined to his satisfaction “the plausibility of a ‘rank-and-filist’ analysis” of British industrial relations “since the *late* nineteenth century”,<sup>2</sup> the task suddenly becomes how to “understand internal conflict within trade unions”. This is a very different proposition indeed, one that is much narrower than the previous critique had implied and its sudden eruption suggests some confusion on Zeitlin’s part as to the nature of the problem. In any case, if “rank and filism” applies only to the internal history of trade unionism as opposed to a theory of the rise of labour, then it

<sup>1</sup> Zeitlin, “‘Rank and Filism’ in British Labour History: A Critique”, p. 23: “the ‘rank-and-filist’ paradigm is fundamentally unsatisfactory and should be abandoned outright”.

<sup>2</sup> *Ibid.*, p. 23. My italics again, because this limitation was not mentioned before nor is it characteristic of much “rank-and-file” scholarship.

is a little redundant to sweepingly condemn its insidious influence on labour history as a whole.

Indeed, at this point, it is necessary to point out that the attack on "rank and filism" is to some extent misdirected. The main scholarly theme of the historians he cites – Holton, Burgess, Hinton, White, Cronin, Hyman and myself – has been a little more profound than the adulation of a militantly pure rank and file. If one may fairly attempt to summarize their common interest it has been the effort to write non-institutional histories of industrial relations which focus on economic and social processes. In particular, if there has been an organizing principle that offered itself for serious critical examination it has been the focus on the workplace and, more controversially, upon work control. I have always understood my own work to be principally concerned with the role that the struggle for authority at the workplace played in the dynamic of labour history.<sup>3</sup> On this issue, as on the argument that workshop organization is a central feature of British labour which deserves historical examination, Jonathan Zeitlin is either silent or, indeed, tends to endorsement.<sup>4</sup> It is, I think, true that this particular focus has been the most suggestive in terms of subsequent research, and much fine research (including some from Zeitlin himself) has followed from the earlier, and cruder, formulations. Zeitlin's category of "rank-and-filist", therefore, fails to accurately capture the central concern and historiographical innovation of the scholarly tradition he finds objectionable.

The use of the term "rank and file" has been common enough amongst labour historians, but why, then, is it now the subject of such stricture? I suspect that this has to do with the two aspects of its current usage. First, and conceptually most important, one of the purposes of this kind of history was to ask (à la Thompson) what role the actions of ordinary workers played in the historical process? In other words, how could we factor in as historical agents workers who were not leaders, union officials, or remarkable in any other way? This question needs to be confronted in any critique of this kind of workplace history because it was integral to the whole project. It was this objective as much as anything else that dictated a non-in-

<sup>3</sup> This theme is quite consciously the central conceptual thread of my *Masters, Unions and Men: Work Control in Building and the Rise of Labour 1830-1914* (Cambridge, 1980).

<sup>4</sup> See Jonathan Zeitlin, "The Emergence of Shop Steward Organization and Job Control in the British Car Industry: A Review Essay", *History Workshop Journal*, no 10 (Autumn, 1980), p. 121 where he adopts the "rank-and-filist" position that the peculiarity of the British case is the generalization of informal organization and job control beyond the craft sector. Unfortunately, he spoils this argument by claiming (pp. 129-31) that the non-skilled workers "learnt" these strategies from the skilled. See also his "Engineers and Compositors: A Comparison", in Royden Harrison and Jonathan Zeitlin (eds), *Divisions of Labour. Skilled Workers and Technological Change in Nineteenth Century Britain* (Brighton, 1985).

stitutional approach. More precisely, it dictated an approach that tried to look at how the history of labour institutions was conditioned and determined by the actions of those who were not part of their organizational hierarchy. One of my purposes in *Masters, Unions and Men*, for example, was to suggest that trade union growth was the product of workplace social relations between workers and employers rather than the product of an inexorable process of associational maturity amongst workers. This aspect of the scholarship Zeitlin either fails to recognize or considers unworthy of attention.

The second aspect of the term followed naturally from the first, because it was employed (by me at least) as a way of describing and understanding the tensions that pervaded industrial relations in the later nineteenth century.<sup>5</sup> It is this aspect that Zeitlin mistakenly takes as the whole meaning. I will return to this matter at a later stage of this paper. For the moment, I think it important to explore further the serious absences and gaps in Zeitlin's treatment of "rank and filism" because they are integral to the credibility of his argument.

In spite of the fact that Zeitlin has written from a workplace perspective – and, indeed, written about work control – his critique fails to confront the conceptual significance that the workplace focus occupies in the body of scholarship he attacks. More than simply a matter of breaking away from the traditional focus on organizations and institutions, the workplace was seen as a useful point of entry to study power and authority relations within the working class – as a way of approaching the relationship between agency and structure. It was for *this* reason, for example, that this kind of history has been interested in the theory of labour processes. Workplace history hoped to capture at an intimate level one of the most important social relationships in society – that between worker and employer – and allow us to admit both agents into the historical process rather than seeing one or the other as passive or one-dimensional. Zeitlin has rightly been concerned to enter employers into the picture – mainly through the perspective of the employers association – but those he critiques also believe in looking seriously at what employers said and did as part of understanding labour history.<sup>6</sup> The workplace is an excellent place to study this aspect of social relations because it is where the labour process is actualized and where the theory and practice of industrial relations strategies meet, founder, are successful or modified. But beyond that, if the workplace was seen as a

<sup>5</sup> I should point out that it was employed at a rather late stage of *Masters, Unions and Men. Work Control in Building and the Rise of Labour 1830-1914* (Cambridge, 1980), the first substantive index reference to rank and file being page 210.

<sup>6</sup> "The Labour Strategies of British Engineering Employers 1890-1914", in W. Mommson and H. Gerhard-Husung (eds), *The Development of Trade Unionism in Britain and Germany 1880-1914* (London, 1985).

place where (amongst other things) power struggles occurred over the respective spheres of authority, then it was unavoidable that we should ask how all this affected the development of labour organization, industrial relations, and even class relations in all their various forms.

Naturally, no one would pretend that the answers to these questions were satisfactory or complete; some would even disagree with the original question.<sup>7</sup> That is not at issue here. The point is that there was a much more ambitious and complex epistemology behind the focus on the workplace, and a more sophisticated scholarly agenda than Zeitlin either allows or confronts, and his objections tend to be weakened by this absence. Indeed, at several crucial points, Zeitlin's procedure tends towards caricature.

The first instance of this occurs at the very beginning of his piece where he claims that in spite of their differences there is a main intellectual linkage between "rank and filists" and the "Oxford school" of industrial relations. In fact, the main thrust of historians of the workplace was to reject the underlying assumptions of the "Oxford school" whilst recognizing that they had identified a feature of industrial relations (i.e. what they called the informal system of industrial relations at the workplace) that historians had previously ignored but which was of considerable historical and contemporary significance. The industrial sociologists who bore the closest affinity to "rank and filists" were, perhaps, people like Alvin Gouldner, Tom Lupton, Huw Beynon, Seymour Melman, Richard Hyman and others whose studies of local industrial relations illuminated the historical evidence in a way that the "Oxford school" (with its presentism and its emphasis on the dysfunctionality of industrial conflict) was unable to do. Thus, the differences between the workplace historians and the "Oxford school" were far more profound than the simple "opposed political valuations" that Zeitlin imputes as the key difference.<sup>8</sup> The "Oxford school" was interested in resolving industrial conflict as part of their public policy role; workplace historians were interested in explaining it historically and exploring its dynamic role in the history of labour.

Caricature is also evident in the very notion of a school of "rank and filists". Beyond the key scholarly theme of an interest in the non-institutional processes of labour's history there are significant differences of focus, assumption and bias.<sup>9</sup> Keith Burgess, for example, tends to empha-

<sup>7</sup> Patrick Joyce, *Work, Society and Politics. The Culture of the Factory in Later Victorian England* (Brighton, 1980) and his "Introduction" to Joyce (ed.), *The Historical Meanings of Work* (Cambridge, 1987). See also, Richard Price, "Re-thinking Labour History: The Importance of Work", in James Cronin and Jonathan Schneer (eds), *Social Conflict and the Political Order in Modern Britain* (London, 1982).

<sup>8</sup> Zeitlin, "Rank and Filism", p. 2. See also James Cronin, *Industrial Conflict in Modern Britain* (London, 1979), pp. 24, 56.

<sup>9</sup> For an example of radical differences see James Hinton's review of Richard Price,

size more the determining role of technological change in the labour process.<sup>10</sup> James Hinton could justly protest about being termed a “rank and filist”, since he seems to be most interested in those segments of the working class seeking a socialist transformation of society.<sup>11</sup> Perhaps the main theme of James Cronin’s work has been the attempt to explain large sociological phenomenon by means of close social and economic analysis.<sup>12</sup> Richard Hyman has been concerned to develop a theory of industrial relations that goes beyond the wooden, institutional categories of the pluralists.<sup>13</sup> It is true, of course, that all of these historians saw themselves as expanding the vision of prior traditions of scholarship by trying to include previously excluded groups into the *explanation* of historical change. This is the real scholarly basis upon which the group could be critiqued, but Zeitlin fails to confront the legitimacy or otherwise of that intellectual endeavour.

A further caricature appears in the way Zeitlin implicitly subscribes underlying assumptions to “rank-and-filist” history: that trade unions are agents of capitalism, and that working class power is circumscribed by the institutions that represent them. Zeitlin claims that these assumptions transcend the difference between the various proponents of this approach, although he is a bit cagey about who he believes actually believes those assumptions.<sup>14</sup> The most difficult thing about this caricature is that it is like red baiting; however much one protests, the mud will stick. The most one can say about this accusation is that it unfairly reduces complex positions and propositions to a simplistic level. Anyone who reads the body of work that Zeitlin is critiquing will usually find a rather different, and more intelligent, picture than the one he paints.<sup>15</sup>

*Labour in British Society* (London, 1986), *Bulletin of the Society for the Study of Labour History*, vol. 51, 3 (1986), pp. 36-40.

<sup>10</sup> This was especially true in his earlier work, as it was in mine. See Burgess, “Technological change and the 1852 lockout in the British engineering industry”, *International Review of Social History*, XIV (1969); *The Origins of British Industrial Relations* (London, 1975); Richard Price, “The Other Face of Respectability: Violence in the Manchester Brickmaking Trade 1859-1870”, *Past and Present*, no 66 (February, 1975).

<sup>11</sup> *The First Shop Stewards’ Movement* (London, 1974); *Labour and Socialism* (Brighton, 1983); “Coventry Communism: A Study of Factory Politics in the Second World War”, *History Workshop Journal*, no 10 (Autumn 1980), “Self-Help and Socialism: the Squatters’ Movement of 1946”, *History Workshop Journal*, no 25 (Spring, 1988).

<sup>12</sup> Best illustrated by his books *Industrial Conflict in Modern Britain* and *Labour and Society in Britain 1918-1979* (London, 1984).

<sup>13</sup> See in particular *Industrial Relations: A Marxist Introduction* (London, 1975).

<sup>14</sup> See Zeitlin, “‘Rank and Filism’”, p. 8: “most proponents implicitly subscribe” which strikes me as rather heavily qualified upon which to build an effective indictment.

<sup>15</sup> See *Masters, Unions and Men*, pp. 16-17 for an explicit rejection of the charge that trade unionism can be seen as “comfortably settling down to impose employer’s discipline in return for the right to bargain over economic conditions”. The “discipline” that was imposed was that of the industrial relations system in which the trade unions were partners.

To take a specific example. Zeitlin abruptly summarizes the discussion of conciliation and arbitration by Burgess and Price as representing "the nadir of union accommodation to the rules of the capitalist game" whereas in reality there were substantial economic and work control gains secured by such systems. The fact of gains was not denied and Zeitlin fails to engage with the main purpose of the discussion of this question – at least in *Masters, Unions and Men* – which fell into two broad categories. First, the purpose was to show that contrary to the usual historiographical assumptions these systems in building and other industries flowed from union defeats, not victories; that they originated from management and not the unions; and they could be seen as a strategy to bring order to industrial relations. There is evidence to suggest that crises of industrial discipline, sometimes in conjunction with crises of competition and the need to cut labour costs, were the context for the emergence of boards in industries other than building. Thus, the north of England iron trade board, Mundella's hosiery board, the Nottingham lacemakers' board, the pottery board of 1836, the sliding scales in South Wales and Northumberland, the boot and shoe board of 1895, all came out of bitter conflicts which arose from the expansion of worker control of the production process, were frequently imposed upon their defeat and were all pragmatic answers to the problems of industrial discipline.<sup>16</sup>

My second purpose was to show that the origins and results of such systems were not purely economic, but had to do with power and how power was exercised in industrial relations. Thus the significance of clauses that specified the authority and power of employers in the early Boards. This was the point, for example, of arguing that Boards were established precisely where the power of labour over the productive process had been greatest and, from the employers point of view, needed to be pushed back. This was also the point of linking the establishment of the Boards with the formalization of industrial relations because it was through formalization that a new ordering of power relations could be secured.<sup>17</sup> In all of this, the trade union as institution played a quite minor role in large part because

<sup>16</sup> See N.P. Howard, "The Strikes and Lockouts in the Iron Industry and the Formation of the Ironworkers' Unions, 1862-1869", *International Review of Social History*, XVIII (1973); F.A. Wells, *The British Hosiery Trade* (London, 1935), Ch. IX; Norman Cuthbert, *The Lace Makers' Society* (Nottingham, 1960), p. 43; Joseph D. Weekes, *Report on the Practical Operation of Arbitration and Conciliation in the Settlement of Differences Between Employers and Employees in England* (Harrisburg, PA, 1879), pp. 3, 15; J.H. Morris and L.J. Williams, "The South Wales Sliding Scale, 1876-1879", *The Manchester School*, XXVIII (1960), pp. 162-4; Alan Fox, *A History of the National Union of Boot and Shoe Operatives 1874-1957* (Oxford, 1958), Section IV; S. & B. Webb, *Industrial Democracy* (London, 1913), pp. 185-192.

<sup>17</sup> *Masters, Unions and Men*, pp. 116-121.

Boards were often established where unions possessed a weak presence. Thus, they could hardly represent the nadir of union accommodation. Indeed, it was an important part of the argument that this formalization was one of the sources of trade union growth and development.

The examples Zeitlin cites as counting against my argument (cotton and iron) refer mainly to the *results* that such systems produced, not to their origins. And, of course, once industrial relations were formalized a new dynamic kicked in which introduced a more bureaucratically defined description of power and obligations.<sup>18</sup> Once industrial relations were formalized new responsibilities devolved onto the various partners which for unions meant the demise of direct local control over negotiations and the acceptance of bargains that their members sometimes disliked. There was nothing “good” or “bad” about this; it was a fact of intra-union life which was conditioned by the structural pressures and priorities of the institution itself. I see nothing in Zeitlin’s account to undermine the argument that these procedures promised a stabilisation of industrial relations that was a main attraction to employers and unions and, at the same time, a major cause of tension within unions. Thus, three notorious “rank and filists”, Clegg, Fox and Thompson have written that one consequence of industrial conflict in industries with extensive systems of collective bargaining

was a growing distrust of collective bargaining among the rank and file. For the union leaders, as joint authors of the new procedures, collective bargaining had come to stay. Despite its shortcomings, they saw in it the guarantee of union stability, one source of their own power, and the best means available for winning benefits for their members. The rank and file, anxious to protect their privileges and customs from encroachment by the employers [. . .] did not see collective bargaining in the same light. It might have done much for their leaders, but what was it doing for them? By 1910 a rift between leaders and local militants was beginning to widen in a number of unions.<sup>19</sup>

The same habit of caricature is revealed in the various hints that Zeitlin drops about the political assumptions and purposes that he attributes to “rank and filism”. Indeed, much of Zeitlin’s case flows from his ascription of a political teleology to the “rank and filists” which sees the abolition of capitalism as the only true interest of the working class, and his assumption that their analysis follows from a series of *a priori* political premisses rather than reflecting a viable reading of the evidence.<sup>20</sup> I find this highly ironic because one of the strengths of workplace history was precisely its effort to

<sup>18</sup> *Ibid.*, p. 190.

<sup>19</sup> H. A. Clegg, Alan Fox and A. F. Thompson, *A History of British Trade Unions Since 1889*, Volume I, 1889-1910 (Oxford, The Clarendon Press, 1964), pp. 472-473.

<sup>20</sup> Zeitlin, “‘Rank and Filism’”, pp. 9, 24.

closely examine the reality of working class behaviour in order to break away from a teleology that imposed a pre-determined view of how the working class should proceed. Indeed, some of the history that Zeitlin condemns was explicitly written as an attempt to develop an interpretation of labour's history that avoided the equally sterile traps of mindless empiricism on the one hand and the judgment of labour history by ideal standards of evolution on the other.<sup>21</sup>

Caricature makes refutation easy; but it is irresistible when combined with partiality. I have already suggested how this has shaped his very definition of the problem, but it also leads Zeitlin to miss main themes of the books he critiques. I notice this particularly (of course) in my own work. The general "theory" of labour history that has been offered in *Labour in British Society* about how the historical process that made workplace organization and action such an important part of British labour's presence in society is simply subsumed under the "rank-and-filist" paradigm. Nor do we encounter any reflections on the argument that tried to address the way that tradition entered into labour's political and social relations with the State in the twentieth century. And this is aside from the reinterpretation of very specific themes that are *also* part of this scholarship – such as the dynamic of labour law, or the process by which labourism emerged as the ideology of the labour movement. Similarly, the major themes of *Masters, Unions and Men* are virtually ignored. Those themes were the shift from an informal to a formal system of industrial relations, a consideration of how that transition occurred and of its implications for employer-worker and worker-union relations. It would be impossible to gather from Zeitlin's piece that this was, in fact, the central story of the book. It might be thought that such partial treatments of arguments hardly warrant the stern recommendation that this scholarship is unworthy of further consideration.

But aside from these general objections to the procedures Zeitlin uses to set up his targets, how effective are the specific indictments that he issues? Four points stand out: the difficulties of drawing a line between officials and rank and file; the fact that leaderships are often more militant than their members; the importance of formal organization to job control; and the fact that union policies are susceptible to pressure from below.<sup>22</sup>

The absence of a categorical line between officials and the rank and file that applies to every situation over time is hardly surprising. Equally, the fact that the different histories and governing structures of unions will produce different relationships within the union hierarchy is unexceptional. Zeitlin is quite right to point to the complexity of relationships within unions and I would be quite prepared to admit that the term "rank and file"

<sup>21</sup> E.g. *Labour in British Society*, pp. 1-12.

<sup>22</sup> Zeitlin, " 'Rank and filism' ", p. 23.



does not entirely capture that complexity. Tension within unions can take a variety of forms: between various bodies in the official hierarchy as well as between recognised functionaries and those who are merely members.

Zeitlin prefers to conceptualize this tension as a function of different interest groups, as *factional struggles between rival groups of leaders*.<sup>23</sup> And it is clearly true that the struggle for power is an important feature of trade union organization, as it is of any bureaucratic organization. It would be a real gain for the internal history of trade unionism to be written from this perspective. But two cautions need to be entered.

First, although interest group politics may satisfactorily explain leadership struggles, it does not necessarily extend to understanding the actions of the wider membership outside of the narrow circle of viable contenders for power. Opposition or debate below the level of organized leadership groups may not be neatly packaged into discrete interest groups – at least the historical evidence does not suggest such an organizational coherence. It is unlikely that the interest group formulation is appropriate to explain local workplace conflicts such as those in London building in the early 1890s or 1913-14 that reached beyond the workplace to impact significantly on union and industrial relations structures.<sup>24</sup> To explain the behaviour of the ordinary mason, bricklayer, and others (which is what this body of scholarship is concerned to do) the term rank and file still seems to me to be superior to “interest group”.

In the second place, Zeitlin’s brief sketch of interest groups competing for power suggests a rather idealist view of union power struggles. Unions are not pure democracies and (arguably) became less so in the period 1850-1914 with certain consequences for their internal relations. The use of the “interest group” concept would have to come to grips at some point with the distribution of *power* within unions. At no point does Zeitlin exhibit an interest in this fundamental question without which internal union politics cannot be understood. Leadership authority is not something that flows from the unimpeded democratic mandate of the membership and it was a prime virtue of the historians Zeitlin critiques that they sought to understand how authority was constructed and maintained. Studies of the internal politics of unions remain rare, but to illustrate the point let us take the case of the National Union of Seamen whose internal politics have recently been analysed by Dr Laura Tabili as part of a wider study of racial politics in inter-war Britain.<sup>25</sup>

<sup>23</sup> *Ibid.*, pp 24-25.

<sup>24</sup> *Masters, Unions and Men*, pp. 181-83, 258.

<sup>25</sup> The following section is taken entirely from Laura Tabili, *Black Workers in Imperial Britain 1914-1945* (Ph.D. Dissertation, Rutgers University, 1985), pp. 313-401.

Tabili shows how racial division within the union were consciously and actively used by the leadership as a tactical strategy in its interlocutor role between owners and rank and file. Neither the leadership nor the rank and file were *necessarily* racist. Race was a tactical issue in the leadership's continual need to maintain its credibility with owners and with its membership. Indeed, union officialdom had learnt the advisability of racist policies as the price of accommodation with the State in the First World War.

Initially, the union's inclination had been to include foreign workers within its ranks, but it was soon realised that the language of imperial prejudice was an effective medium of communication with shipowners and the political world – especially as nationality became an important issue in politics just before 1914. The bargain struck with the employers and the State during the War brought the union recognition and a tenuous security at the price of creating (and later extending and refining) a segmented labour market which excluded non-white seamen from the conditions attained for whites. This hardly benefited the rank-and-file workers as a whole, though it obviously benefited and encouraged sectionalist groupings. More important, however, was the key role the racial division of labour came to play in the power politics of shipowners and union leadership.

On the one hand, employers could use the threat of cheap non-white labour to keep union demands moderate. On the other hand, union officials could point to the threat from non-white labour (continuously employed in large numbers) as both the cause of British seamen's problems and as the justification for accepting the moderate bargains struck by the union and its support of employer discipline.<sup>26</sup>

The issue of race, therefore, may not be seen simply as a function of visceral prejudice faithfully reflected by the union leadership. As Dr Tabili conclusively demonstrates, race was inseparable from intra-union power relations – indeed it was this that defined its very meaning – and was consciously used by the high officials of the National Union of Seamen to justify the bargains reached with employers – which were often highly controversial with the membership – and to maintain its position *as* a leadership.

Such realities of union power, it seems to me, are evaded by the "interest group" analysis of union politics which cannot adequately account for the operation and distribution of power within unions. Thus, at some point in the analysis of intra-union politics we will encounter the ordinary union

<sup>26</sup> In a remarkable editorial in 1931, for example, the union journal threatened to deny employment (through its National Maritime Board agreement with employers which supposedly gave it a closed shop on British ships) to men who "misbehaved themselves" with employers. Tabili, *Black Workers*, pp. 361-62.

members who at certain times will resist the exercise of that power. He or she will not always do so at the bidding of an alternative leadership interest group (though sometimes they will), but (and this is especially true in the period in question) will often act “spontaneously” or with a local leadership that has no hope or desire to form a sociologically identifiable alternative interest group. The local leadership may be of various kinds and may be responding (as in the case of District Committees) to the pressures of their constituents, or may be responding (as in the case of the shop stewards organization) to immediate pressures that demand immediate actions. This approximates to the situation that existed in building and is outlined by Zeitlin in engineering in the late nineteenth and early twentieth centuries. To describe those historical actors as “rank and file” does not seem to me do injustice to the historical evidence. Thus, even if no clear line can always be drawn between the officials and rank and file, the latter term can still be helpful in explaining the actions of the ordinary union member.

Ultimately, however, it is possible to reconcile “rank and filism” with Zeitlin’s preferences. Thus, we can agree that once we move the analysis into the recognised organizational components of the structure of the union, interest group politics becomes a useful way of understanding the bureaucratic infighting. But to reduce all internal union conflict to interest groups would mean a narrowing of focus away from the actions of the ordinary union member and towards the actions of those who were linked in some way into the internal organizational hierarchy.

There is no need to dwell on Zeitlin’s second objection that leaders are often more militant than their members. One can agree, of course, that this is sometimes the case. But it depends upon one’s definition of “militant”. Zeitlin assumes that “rank and filists” use the term with heavy political connotations. In fact, militancy meant a vigilant defence of work control and other workplace interests which frequently did not converge with the institutional priorities of the industrial relations system. In that respect the evidence of the “rank and filists” is not contradicted by Zeitlin. Naturally, a union leadership will have different priorities to a work group and those divergences will sometimes cause tension. The leadership may be more militant on procedural rules, the workgroup more militant on walking time or some other local issue.

Zeitlin’s points about the support of job control by formal organization and union coordination are more ambiguous than he believes. As has already been suggested, when discussing formal institutions a distinction must be drawn between origins and results. I have already argued that the origins of conciliation structures in building do not support Zeitlin’s case; and I think the same applies to the *origins* of similar structures elsewhere. But it is obviously true that once in place such structures could serve to conserve existing gains. My argument in *Masters, Unions and Men* was that

official union negotiation tended to be less militant on job control issues and that this caused tension within building unions that could open the spaces for a political debate about the principles of union organization and mobilization.<sup>27</sup> This doesn't seem so different to what Zeitlin is saying; and, indeed, the example of rank and file vigilance that he cites from the boilermakers union tends to support my argument about different priorities throughout different levels of the union structure.

It is difficult to judge the power of Zeitlin's fourth point – that unions respond to pressure from their memberships – because it is an extremely partial account being confined to the engineering unions within a narrow time frame. Equally, of course, we could cite cases where memberships responded to the pressure of their leadership – as in the case of the 1914 London building strike where after three tries the strike was ultimately settled unilaterally, or the case of the anti-Arab seamen campaign of 1929-1930 that was carefully orchestrated by union officials.<sup>28</sup> Obviously the extent to which these situations may be generalized depends, among other things, on the structures of governance within unions. Governance structures vary greatly; it was far easier, one assumes, to ignore the leadership in the Boilermakers' and Iron Ship Builders' Society or the Amalgamated Society of Engineers than it was in the National Union of Railwaymen in the 1920s or the Transport and General Workers' Union in the 1950s. But even if Zeitlin is right that membership pressure operates in the uncomplicated way he implies – unsullied by "leadership" control of the levers of power such as the union journal, or the ambiguous loyalties of local officials – then, in a curious way, it would reinforce a central contention of workplace history – that the "rank and file" are also among the determinant agents of labour's history.

In the absence of a major statement of empirically based research, it is difficult to know exactly how Zeitlin would improve upon the analysis of labour history that he finds so problematic. However, there are certain conclusions one can fairly draw from his paper and elsewhere that suggest the content of such an interpretation. The first thing to note is that Zeitlin does not seem much interested in the big question of why British labour developed the way it did. Yet it is precisely this question that has engaged the attention of those historians against whom Zeitlin directs his considerable intelligence. It is also a question that is recognised to be of some significance in understanding the course of modern British history as a whole. There is no reason, of course, why Zeitlin should be interested in this question, but if he is going to engage with those who are, he should endeavour to meet them on their own terms and, at the very least, his

<sup>27</sup> See also Richard Herding, *Job Control and Union Structure* (Rotterdam, 1972).

<sup>28</sup> *Masters, Unions and Men*, pp. 264-66; Tabili, *Black Workers*, pp. 379-97.

failure to recognize that there *is* a question to be answered here is very strange. Not once during his paper – and never elsewhere to my knowledge – has Zeitlin recognised that the dynamics of the “rise of labour” was the transcendent question. What this suggests is a curiously a-historical perspective, a lack of real interest in the historical context. And, indeed, when we look closely at some of Zeitlin’s controlling notions – especially in the paper – we see the same theme evidenced.

Let us take two phrases where Zeitlin puts forward his tentative suggestion for a “new paradigm”: “Conflicts within trade unions stem from two central tensions [. . .]. Externally, trade unions are torn between the demands of opposition and negotiation; internally, between those of centralization and mobilization.” And again: “The negotiating process in turn demands central coordination, but [. . .] trade union leverage at the bargaining table ultimately depends on the mobilization of their members’ active support; and this process of mobilization calls forth new demands and new forms of struggle which constantly threaten to overturn established accommodations with employers and the state.”<sup>29</sup>

In themselves, these are unexceptional statements. The problem is, however, that they exist in an empirical and historical vacuum: they remind one of statements from sociological textbooks which are so general as to be obviously true and hardly worth debating. What is missing is any suggestion that the processes they describe are *historically* conditioned and determined and that the categories themselves are open to debate and often revealed as historically problematic. It would be interesting to see whether the empirical data would fall naturally into the categories of “centralization” or “mobilization” and “opposition” or “negotiation”.

The questions of why the negotiation process demands central coordination (were there no alternative models of mobilization and winning consent available?) and through what historical processes it came to acquire that quality are prior questions that seem of little apparent concern to Zeitlin. Similarly, there is the interesting assumption that the official trade union structures initiate negotiations and *then* have to mobilize their members which, in turn, sets them thinking and, presumably, threatens to lead to oppositional or minoritarian currents. In fact, as much of the scholarship Zeitlin critiques has shown, pressure for negotiation often comes from below to which leaderships have to respond.

An article Zeitlin published in 1987 entitled “From labour history to the history of industrial relations” contains perhaps the clearest hints of how Zeitlin would prefer to see labour history written.<sup>30</sup> In this article arguments rehearsed elsewhere are restated (sometimes word for word) against “rank

<sup>29</sup> Zeitlin, “ ‘Rank and Filism’ ”, pp. 23-24.

<sup>30</sup> *Economic History Review*, 2nd series, XL, 2 (1987), pp. 159-184.

and filism" and against seeing institutions as reflections of social interests.<sup>31</sup> But, in addition, as the title suggests, there is a prescriptive message within his predominantly critical stance. Zeitlin is clearly worried about the way labour history has burst its bounds to become the "social history of the working class" as opposed to the history of institutional relationships. He wants to re-cap the bottle and return labour history to the boundaries established by the Webbs and represented amongst contemporary scholars by the "magisterial" work of Clegg, Fox and Thompson.<sup>32</sup> Zeitlin urges labour historians to turn away from the study of "informal groups or spontaneous economic and social processes" and towards the history of the institutions that link and mediate worker-employer-state relationships. The reason for this is his belief – more an assertion than a demonstrated fact – that it is institutional forces that have "played a crucial role in shaping relationships between workers and employers."

What this means in practice is not clear. At one level there is nothing objectionable and much to be gained from what Zeitlin seems to be suggesting. It is important that the institutional history of labour be re-written to replace the narrow and hagiographical tradition of the past. Indeed, "rank-and-file" historians themselves have been concerned to re-examine the internal history of institutions.<sup>33</sup> In addition, historians have always taken into account the importance of institutions as important influences on labour history. So, if Zeitlin is merely echoing a traditional concern to factor institutional influences into the dynamic of labour history, none would disagree.<sup>34</sup> But if the implication of this prescription is that institutional forces are determinate and therefore possess a conceptual and empirical primacy in the explanation of labour history, then we must wonder how such an analysis (could we call it "institutional reductionism"?) would be superior to those he is concerned to critique.<sup>35</sup>

<sup>31</sup> See "Trade Unions and Job Control: A Critique of 'Rank and Filism' ", *Bulletin of the Society for the Study of Labour History*, no 46 (Spring, 1983).

<sup>32</sup> For a different evaluation of this kind of history see my review of H.A. Clegg, *A History of British Trade Unions since 1889. Volume II 1911-1933 in International Labor and Working Class History*, no 33 (Spring, 1988). See also Zeitlin's review of this volume entitled "Trade Union History or the History of Industrial Relations", *Bulletin of the Society for the Study of Labour History*, 51, 3 (1986).

<sup>33</sup> E.g., Richard Hyman, *The Workers' Union* (Oxford, 1971).

<sup>34</sup> Just to cite a few examples of this concern from "rank and filist" historians: James Cronin "Politics, Class Structure, and the Enduring Weakness of British Social Democracy", *Journal of Social History*, 16, 3 (1983); "The British State and the Structure of Political Opportunity", unpublished paper; "Crisis of State and Society in Britain 1917-1920", unpublished paper; "Industry, Locality and the State: Patterns of Mobilization in the Postwar Strike Wave in Britain", (forthcoming in *Annali*). See also Price, *Labour in British Society*, Chs. 6-8.

<sup>35</sup> This is the main thrust of the article, although the last two sentences seem to suggest some confusion on Zeitlin's part. Having spent most of the article implicitly denying the

Two immediate problems present themselves with this formulation; one conceptual and one historiographical. The first is that such an approach ignores the origins of institutions themselves, but takes their presence and perhaps their function and shape for granted. Yet it is precisely those questions that are of profound importance in understanding the evolution of labour or any other history. Obviously, institutions are the product of historical circumstances which include economic and social processes as well as other institutions. But to say this is to resolve nothing and in this respect Zeitlin's approach leads up a blind alley.

Even more important are the narrowing implications of Zeitlin's proposal. It is not at all clear, for example, that the history of gender (which Michael Savage has shown to play an important role in labour politics),<sup>36</sup> kinship, community, popular culture, workgroups could be fitted into his vision of labour history. Even more serious, it is unclear how we could write the history of labour before formalized institutions were established. Zeitlin's prescription is relevant only to the late nineteenth century and has little to say to the vibrant historiography of labour before that date. The danger of Zeitlin's paradigm of the institutional nexus of labour history is that it will lead to a drastic shrinking of the scope of the field. It is precisely this institutional model that we have just escaped from and even though the historiographical landscape is a bit untidy as a consequence, it seems a little too early to be calling for the restoration of the *ancien régime*.

Zeitlin's model, then, is an avowedly conservative one in its implied tightening of the boundaries of labour history. It is likely to be conservative also in its historiographical assumptions; for the history of institutions is the history of the winners in history, of the local and national establishments whose procedures and ideologies tend to be treated as inherently rational and natural. One of the virtues of the break with the kind of history that

value of analyses premised upon economic and social processes, he first retracts then reasserts his earlier assumption about the determinant role of institutions over impersonal forces: "[it is not suggested] that impersonal economic and social processes have no impact on the development of institutions. But it is necessary to insist that social relationships, whether in the workplace, the family or the wider community, cannot be understood without reference to the operation of formal institutions, just as the latter can never be determined by reference to the objective interests of pre-existing social groups."

<sup>36</sup> Michael Savage, *The Dynamics of Working Class Politics. The Labour Movement in Preston 1880-1940* (Cambridge, 1987). This book is a highly intelligent attempt to relate politics to structure in a way that avoids the well-known pitfalls that usually accompany that effort. It is a welcome counter-weight to the recent tendency of social history to abandon that effort in its search for meaning through "languages of politics". One very effective critique that could be made of the historians Zeitlin attacks would be their total ignoring of the role of gender in the division of labour. For this see Richard Whipp, "The Stamp of Futility: The Staffordshire Potters, 1880-1905" in Harrison and Zeitlin, *Divisions of Labour*.

Zeitlin wants to reinstate was precisely that it allowed us to see how the "natural" order was in fact the product of an historical process in which there were losers whose alternative strategies of action and organization were equally rational and natural.

Zeitlin's call for a return to the safe confines of the institution – away from all the messy talk about class formation and consciousness – reflects the epistemological and organizational crisis that now characterizes labour history. The nature of the field and its assumptions are very much a matter of debate and uncertainty. In large part this crisis is the result of the historiographic revolution in the field that began with Edward Thompson's *The Making of the English Working Class* and the subsequent efflorescence of scholarship that demonstrated the possibility of entry points to the study of the working class other than the gateway of the institution. The comforting certainties of what labour history is, of how it is to integrate the many divergent tendencies and themes, of how it fits into the history of British society, have all been shattered. But it is no prescription to this crisis to leap back a generation and – with a few new twists – restore the lost primacy of the labour historiography of Clegg, Fox and Thompson. Instead of narrowing the scope of the field in this way, we should endeavour to encompass the heterogeneity of labour's experience in the family and community (national as well as local) in addition to the workplace. The rich historiography of economic, social, cultural and political processes that has marked the past thirty years presents the opportunity to bring labour history into a close conceptual relationship with such areas as the history of women and family, leisure, popular culture, popular and high politics. A new-style history of institutions would necessarily be a significant part of this enterprise, but it would not be the whole and the field would likely be impoverished were it to become a new orthodoxy.