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Robert Brym and R. James Sacouman, eds., *Underdevelopment and Social Movements in Atlantic Canada* (Toronto: New Hogtown Press 1979).

IT IS AN ALMOST shocking reflection on the quality of socialist thought in Canada that this is the first book-length study of the "Eastern Question" to come out of the left. What a long silence! But it would have been impossible to assemble ten serious Marxist scholars a decade ago to produce a book like this one. Given this long silence, one might have expected a weak, tentative exploration of the problems of Atlantic Canada. But this collection of essays is not weak; in fact, it is not even "uneven," to use a word much beloved by reviewers of collected essays. Although there are differences between the authors in tone, method, and subject, each of the essays is carefully constructed and analytically clear. The Marxist discovery of Canadian regionalism is now a *fait accompli*, and both socialist thought and regional studies should be transformed.

Four of the essays present a regional analysis. Henry Veltmeyer assesses "The Capitalist Underdevelopment of Atlantic Canada" by situating regional disparity within the categories of *Capital*. The other three regional essays are more concerned with social movements: R. James Sacouman describes the differences between the Maritime and Prairie Co-operative Movements to 1940, and corrects a serious blindspot in the literature of Canadian protest movements; Robert J. Brym discusses "Political Conservatism in Atlantic Canada" and concludes that such conservatism comes from a deep-rooted structural weakness within the working class and the primary producers; and David Frank and Nolan Reilly show that workers in the region responded in fairly large numbers to socialist and radical doctrines — a solid (if somewhat implicit) challenge to the identification of Canadian socialism with the West. These

four regional studies are followed by three fairly detailed analyses of Nova Scotia: one by Sacouman on the Antigonish Movement which stresses the structural factors which conditioned its rise; another by L. Gene Barrett on the fishing industry to 1938; and a third by Rick Williams which carefully examines the struggles for trade unionism among inshore fishermen. Then we turn to three studies of Newfoundland. Steven Antler discusses both neo-marginalist and Marxist analyses of monopoly in his "The Capitalist Underdevelopment of Nineteenth-Century Newfoundland;" Robert J. Brym and Barbara Neis discuss the regional factors which contributed to the remarkable success of the Fishermen's Protective Union of Newfoundland, by far the most successful third-party movement in the region; and James Overton, in "Towards a Critical Analysis of Neo-Nationalism in Newfoundland," offers a severe critique of various types of "populist" writing in the province. Merely to list the titles is a way of conveying both the broad sweep and ambition of the collection, and its close attention to detail. Historians who have so often belaboured sociologists for their neglect of the particular might find this collection puts this criticism to rest, and in fact starts turning the question to the historians' own abject neglect of explanation.

At the risk of imposing on *Underdevelopment and Social Movements* a unity it does not profess, one can conclude that the various authors all want to root the study of underdevelopment in conditions of *production* which are considered to be crucial to any understanding of the conditions of *exchange*. The scholars in this collection all take their stand with those critics of dependency theory who suggest that Gunder Frank and others played down or even ignored class relations in the sphere of production. The underlying theoretical premiss of the volume is derived from Marx's discussion of accumulation in *Capital*. I think three

challenges (or problems) have still to be confronted, if the analysis is to proceed further from the basis outlined in this volume.

The first and most fundamental challenge is to sort out what weight to attach to the Law of the Tendency of the Rate of Profit to Fall (or TRPF for short). The answer of these authors, particularly Henry Veltmeyer, is to assert that the Law of TRPF is absolutely basic. Essentially the development of the capitalist system (and consequently the regional disparities involved in it) is shaped by the attempts of capitalists to counteract the system's inherent tendency toward a declining rate of profit. (19) One of the most puzzling and unsatisfactory aspects of the book, however, is that it fails to present this law in any kind of detailed manner — so that while the law is seen as a basic and determining one, the reader is never shown how it works or why it is basic or determining. It seems to operate as a kind of talisman of orthodoxy, both here and in the articles which have set out this theoretical perspective at greater length. While one might argue that a consistent approach to underdevelopment is impossible within Marxism without the Law of TRPF — the argument Veltmeyer has used elsewhere — one succumbs to a fairly obvious tautology when one uses this supposed impossibility as a means of justifying the law.<sup>1</sup> As everyone knows, the Law of TRPF is precisely the section of *Capital* which made the greatest appeal to the theorists of the crash in the Second and Third Internationals. Essentially the theory of the crash suggests that the capitalist system is a motor which runs into trouble because of the very mechanisms which allow it to function; the end of this system, its crash, is not centrally influenced by the class struggle nor by the consciousness of anyone. The adoption of

this law is also associated with Marx's prediction — which Veltmeyer adapts to a regional analysis — of a growing concentration of capital and wealth at one pole, and immiseration and massive proletarianization on the other. Instead of this working across the social formation in classes, it is seen to be working within the regions. The stark landscape of the *Communist Manifesto* is back with us.

A central dilemma of those who seek to use this law is the immense weight which must be assigned to the countertendencies and counterweights which enable capitalism to endure and expand. What makes this argument exasperating, of course, is that countertendencies and counterweights end up postponing indefinitely the crucial test of the original theory — that is, the fall of capitalism. Although the theoretical sections of this collection tend to place countertendencies in a secondary position, the most recent and intelligent defenders of the Law of TRPF argue that the "counteracting influences" are not in fact *effects* or *results* of the tendency of the rate of profit to fall. They are, rather, equally the effect of capital accumulation with its necessary concomitant of a rising technical composition (reflected in Marx's analysis by a rising organic composition but a value composition which does not necessarily rise).<sup>2</sup> Consequently to speak of a Law of the Tendency of the Rate of Profit to Fall is profoundly misleading; we should speak, rather, of a Law of the Tendency of the Rate of Profit to Fall and Its Counteracting Influences. But even this more nuanced version of the Law (which would have been adapted to the interesting discussion of the benefits derived by capitalism from non-capitalist relations to production on its periphery) does not get around the central questions of verification and coherence which many critics have raised. If we retain Maurice Dobb's interpretation of the law (as tendency and

<sup>1</sup> Cf. Henry Veltmeyer, "Dependency and Underdevelopment: Some Questions and Problems," *Canadian Journal of Political and Social Theory*, 2 (1978), 69, n. 12.

<sup>2</sup> Ben Fine and Laurence Harris, *Rereading Capital* (New York 1979), 63.

countertendency as elements of conflict out of which the general movement of the system emerges),<sup>3</sup> we are obviously left without any clear notion of how such tendencies and countertendencies work or what predictions, even over a very long term, might be made about the outcome of such interaction. It is simply not clear that the dogged defence of this "law" serves any useful purpose, either for historical materialism or for the study of regional underdevelopment. Perhaps this criticism can be confronted and turned back by the theorists; but the point remains that these criticisms are not particularly new, and the sociologists who use the Law of TRPF must either show that they have effectively combatted them or that they are doing something altogether different. To write of the laws of capitalist development and not mention the critiques of Sraffa and others smacks of genuflection, not of analysis.

A second problem this volume suggests is that periodizing regional history is an immensely complicated business. When did the region become capitalist? Why and when did it undergo "underdevelopment?" Some of the analysts — Veltmeyer especially — present a fairly stark portrait of confrontation between the local primary producer and the external industrial and financial capitalist. This approach glides over the local working class and the local industrial and commercial bourgeoisie, omissions which are not minor. L. Gene Barrett's analysis of the fishing industry is subtler. He sees two distinct forms of capital appearing in the fishing industry: a nineteenth-century indigenous merchant-trader class (a branch of which was responsible for the development of large-scale salt fish companies and the offshore schooner fishery, which dominated the industry until the 1920s), and a large-scale class of fresh-fish companies formed in the years immediately preceding World

War I, which came to dominate the industry in the 1930s and 1940s. Thus his analysis presents a detailed historical model that recalls Marx's understanding of the transition from mercantile to industrial capitalism. What is suggestive, of course, is how late this transition seems to be occurring: any simplistic notion of the transition to industrial capitalism that fails to account for unevenness is simply inadequate.

It is on this point of the history of various modes of production in the region that the essays (especially those by Barrett and Williams) make a fine and striking contribution to our knowledge of regional history. I still wonder, though, if some excellent points might not have been made against the neo-marginalist position that did not come through very strongly. Surely the strongest case Marxists have against the neo-marginalist insistence on resource deficiencies as a covering explanation of underdevelopment, is that roughly the same resource base has produced strikingly different economies according to the mode of production predominating in the region and in the external authority governing it. One cannot help observe that there seems to be some difference of opinion among the authors on this point. If some passages suggest that the region was characterized primarily by non-capitalist *relations* of production (within the *mode* of production of mercantile capitalism), until it confronted, at some point, the external concentration of industrial and financial capital, there is much evidence cited for the view that the precipitous decline of the region was determined by the advent of monopoly capitalism, which effectively absorbed indigenous industrial and mercantile capital. Rather than seeking, as Veltmeyer tends to do, a "turning point" at Confederation or earlier, the theorists should turn more close attention on the early decades of the twentieth century. The period of 1900-25 — a period of immense regional economic growth as

<sup>3</sup> Maurice Dobb, *Political Economy and Capitalism* (New York 1945), 109-110.

well as crisis — must be considered more closely before we assent to any simple view of economic history which sees the course of events as determined by the reallocation of economic roles within Confederation. What the initial work on mercantile capital notoriously missed, and what these essays tend to overlook as well, is the specific stage of Canadian capitalism, indicated by the tight interlocking of banks and industries and the massive consolidation of industry, which clearly arrived in the first quarter of the twentieth century.

A third challenge which the volume presents, concerns the overall impact of the Marxist approach. One good way of putting this is to ask, as directly as possible: "Why did Atlantic Canada decline?" Why did the eastern periphery decline as opposed to the centre? Why did the centre not become the hinterland of the east, as some Nova Scotians thought inevitable in the 1860s? The Marxist theorists have made everyone understand that capitalism entails the subordination of weaker economies to stronger ones within the market, but the traits that constitute strength or weakness tend to be left unexplored. Everyone attacks and criticizes resource determinism, and for good reason. It is obviously misleading (and "ideological" — in the Marxist sense) to confer on the workings of Nature the same status that the devout once reserved for the Almighty. Steven Antler's essay on nineteenth-century Newfoundland contains the best critique of geographical determinism. As he notes, the resource determinists merely transfer the workings of the Unseen Hand from God (or the Market) to nature. In this interpretation, "Underdevelopment may be an ugly phenomenon, but given the workings of the invisible hand, it must be judged to represent the best of a wide variety of less appealing possibilities." (198) Obviously this critique is right. It is clearly a priority to dismiss the simplistic equation of geography and destiny which is made by con-

ventional theory. But however urgent this task and right the arguments, they will not demolish the opposite view until the questions of the general evolution of capitalist economies (why some regions must develop to a lesser extent than others) are fully integrated with the particular questions raised by this case (why the Atlantic region in particular came to be the type of region which developed less than others). Here, again, we want to focus much more closely on the period from 1900 to 1925, since it was in this period that the region was clearly weakened vis-à-vis central Canada.

This outlines some of the challenges which are raised by the essays on the structure of underdevelopment; but there are many additional arguments in this collection about social movements that deserve comment as well. The general conclusion of these essays is that the stereotyped image of the Atlantic region as a conservative area is overdrawn, if not precisely inaccurate, and that the future of the working-class movement holds promise. What seems to strike us most forcibly is the sheer diversity of social movements. Frank and Reilly, for instance, question the adequacy of some all-embracing theory of regional conservatism and recall the widespread influence not only of the Socialist Party of Canada, but of a whole range of reforming groups as well. Sacouman sees the rise of the Antigonish Movement co-operatives in the context of a struggle between capitalism, co-operation, and trade unionism/socialism — a formulation which seems difficult to accept, although it again reminds us of a diversity of responses and popular struggles. Turn over the pretty picture postcard of the region, with its tranquil peasants whittling by the sunny sea, and you find an amazingly complicated nexus of social classes, economic conditions, and political ideologies. Some of these ideologies have questioned the continuing survival of the capitalist system: the pleasant villagers of

Peggy's Cove, as J.F.B. Livesay found out, talked about the CCF and socialism when they wearied of being picturesque. Of course we want to be careful and not effectively block off the avenues which the earlier political scientists opened up. There is a problem of mobilizing people; there is widespread cynicism and apathy. Brym is surely right to ask whether this is really "conservatism" or merely resignation to necessity. The best way of understanding it is surely to get a firmer idea of its history. When, for example, did the various political fiefdoms — those seats which are passed on from father to son, presumably as part of the settlement of an estate — take shape?

At the risk of belabouring the point, I might point out that the analysis of the social movements tends to raise questions about the inadequacy of the analysis of social structure. If we follow *Capital* closely enough, Veltmeyer urges, we should analyze social movements in Atlantic Canada in terms of the "industrial reserve army," and this as characterized by three categories: the floating (temporarily out of work with alternate expansion and contraction of production), the latent (workers expelled from capitalist production who are thrown into self-subsistence or independent commodity production), and stagnant (workers in marginal and very irregular employment, who are degraded and pauperized, and consequently unable to sell their labour at any price). Veltmeyer goes on to suggest that it is this stagnant form of reserve labour which "creates the economic and social foundation of the class structure, formed with variations in each of the four Atlantic provinces." (31)

I do not agree. Veltmeyer is suggesting that the economic and social foundation of the class structure is actually *created* by the lowest strata of the surplus-population: by the marginally employed and the lumpenproletariat. The analysis of class structure in the Atlantic region would thus logically commence

with the "hospital of the active labour-army" (to quote Marx), the pauperized lumpenproletariat. But without minimizing the extent or the tragic impact of unemployment, it can be seen from the labour participation rates and the unemployment rates that what we are dealing with is not a radically different economy than (say) Quebec or Northern Ontario, but a basically similar economy in somewhat worse condition. The Atlantic region is an area of old industries and depleting resources but it is not the equivalent of Latin America. It is simply not clear that an analysis based largely on analogies drawn from Latin America really brings to light the central features of regional economic history. More apt parallels might lie in such places which were, like the Maritimes at least, heavily into coal, heavy industry, and primary resources: Wales, Scotland, Northern Ireland, Belgium, Pennsylvania. . . . And this further suggests that it is simply wrong to insist that the most important question concerns the lumpenproletariat or the primary producers. The proper analysis of the regional class structure begins with the working class, which is curiously neglected in this volume. The proper analysis of regional class structure, unless it is to be forever branded with the label of Marxist hyperbole, should not attempt to suggest that the wages of workers in the region have been (or are going to be) dragged down to a bare subsistence level by the existence of a swarming mass of the absolutely pauperized.

The point is that the working class has been a major force in the region since at least 1900 (and in the coalfields since 1880). We cannot jump from the fact that people live off the land, or fish, and assume that they are incapable of acting in class ways. Williams' study of fishermen, for example, is especially valuable, because it charts the movement from primary production to wage labour in a sensitive and understanding way. Rather than clucking over the "petit-bourgeois"



attitudes of fishermen and chortling over their imminent proletarianization, Williams sees that the autonomy at work which fishermen once had may well be a resource for them in the struggles with the monopoly fishing companies. In other words, when we move from the analysis of structure to the analysis of consciousness, we cannot offer any simple reduction from ideology to the economic base; people carry ideas with them through time, and these may not neatly reflect any "underlying" present structural reality. The point brought up so often in this book — about the heterogeneity of structures and of responses to them — is absolutely crucial. The divergent interests and preoccupations of primary producers and workers (think, for example, of the fishery: polarized between inshore/offshore, unionized/"artisanal," and Newfoundland/Maritime Provinces bases) makes it all the more difficult to generalize local struggles into an understanding of common goals and purposes. There are class struggles here, to be sure: each small social "island" in this archipelago has its working class and capitalists and a whole range of others, each of whom struggle for their interests; but there is perhaps no regional class struggle, no regional working class, capable of imposing order on the many small struggles which take place and making them into something bigger and more challenging.

This brings us back to the question of conservatism. Brym tends to argue that the continuing hegemony of the traditional parties comes about because people have resigned themselves to necessity. Perhaps. But these traditional parties did, in the past, appeal to the very local but quite serious class conflicts within the region. The Liberal Party can hardly be construed as a radical threat to the established order, but it secured its hold on the populace by offering comprehensive mining reform, working-class representation (of a sort), and eventually the welfare state. Men such as Louis Robichaud or

Angus L. Macdonald did not merely thrive on passivity; they aroused people, and more especially they aroused classes of people. So did the Conservatives, when they enjoyed success. Maritime Rights was a fairly explicit attempt to win back from the socialists and independent labour the working-class constituencies. It is probably misleading to think of politics in the Atlantic Region as the consequence of passivity or desperation. There was an *active* component in the creation of these ideologies, for they arose only by appealing to masses of people. This may seem like a pessimistic conclusion, at least for those who do not want to see the forces of reaction dominate the region forever, because it suggests that these forces are not merely suffered by working people but were, in part, actively created by them. In fact, however, it is a more optimistic conclusion than that drawn by the analysis of social structure or of social movements in this volume — and one might note that at times the social movements are *reduced* to their underlying social realities in a rather mechanical fashion. For it suggests that the failure of the left in the region may be not the consequence of unavoidable structural realities but the result of tactical error. One possible reason why the left has been confounded in Atlantic Canada is that it has never confronted the very special problems that the region represents. A centralist, bureaucratic, and flaccid ideology is precisely what is doomed to fail in the region; but the imaginative, flexible, and grass-roots approach is not so necessarily fore-doomed. One reason why socialists dislike the Antigonish Movement so much is, as Sacouman shows, that it effectively undercut the socialist alternative in Cape Breton; but another reason is simple jealousy: they performed with the effectiveness and cogency that the traditional left in the region has never approached. People did not support the Antigonish Movement (or the Robichaud or Macdonald Liberals, or the Fishermen's Pro-

tective Union) because they were resigned to their fates, but because they wanted to change them. This volume of essays is remarkably successful in conveying the diversity of the region it studies. Could this be at least a beginning to using this diversity itself as a resource, rather than an obstacle, for the left?

Ian McKay  
Dalhousie University

Grant Reuber, *Canada's Political Economy* (Toronto: McGraw-Hill).

THIS IS AN edited collection of 22 articles and five appendices, mostly written by Reuber since 1970. They are mainly overviews and not highly technical. In both content and perspective they are what might be expected at a Bay Street Rotary Club dinner — neither enlightening nor enlightening.

The discussion of Government and Economic Policy reflects a general theme, that "big and growing government as often as not has been the source of difficulties rather than the key to their solution." (xi) It is not the private market economy, but the "avoidable failures of analysis and policy" which explain economic performance in Canada. And government's failure has been too much intervention in the economy, not a failure to deal with the power of the corporations in the economy: "As the state has tried to take over the market, so interest groups have tried to take over the state. . . . With the market suppressed and the elite overwhelmed, no effective check resists the clambering interest." (17) This shows both Reuber's economics and politics. The state should stay out of the economy and policy should not be determined by other than the elite. He recognizes that reforms, such as proposed by Carter for taxation, are fought by the vested interests; yet he opposes increased access to decision making, because there is already enough access and because it would lead to "less consensus and more intensive dispute, albeit better informed dispute." (30) (Don't confuse me with the facts, the elite

has made up my mind!)

The section on Confederation is relatively mild, arguing that disparate regional pressures make for more inefficient government intervention in the economy. He cites the dependency of provinces like Newfoundland on federal transfers but does so in a way which implies twice the dependency which actually exists. (61,63)

His discussion of the "Just Society" again shows Reuber's ideological bias. He implies that state intervention has promoted social justice, yet cites evidence that there has been no change in the income distribution. (88) He claims that poverty has diminished, but if poverty is defined relative to national income, then the constancy of the income distribution implies that the percentage of poor in the population has also remained constant. He ignores the economic basis of poverty, having quoted Harry Johnson regarding its "deep social roots." (16)

The cop-out on poverty is further justified by a nice bit of jargon: "The Family Benefits line . . . can be considered an approximation of revealed social preference for a minimum necessary income." (80) Interpretation: we do not have to worry about the poor because the current support reflects society's feelings about what is adequate. This is astounding from an economist who attacks comfortable middle class conservation values and whose economic theory of democracy leads him to conclude that government intervention reflects vested interests, not the public interest. Somehow, when it comes to the poor, the government is able to reflect the public sentiment. As Osberg points out, the poor lack political clout, so consistency for Reuber should mean that transfers to the poor are too low since they would reflect the desire of the non-poor to avoid taxes.<sup>1</sup> On the other hand, if one can conclude that government reflects society's values with respect to the poor, why does it not also reflect society's values with respect to tariff levels or equalization pay-

<sup>1</sup> Lars Osberg, *Economic Inequality in Canada* (Toronto: Butterworths 1981).

ments? Let's have consistency in analysis, not in bias.

There are three sections dealing with international matters. They all reflect the underlying assumption that freer international trade leads to a more competitive (more efficient) economy. Reuber does not ignore the power of the giant corporation to exploit workers, consumers, and governments, he denies it! (219, 241) He considers the post-war international arrangements "largely a success," turning a blind eye to their blatant abuse, as in the case of Chile. He is not concerned about Canada's increasing integration with the U.S., even though "closer economic integration... particularly with the United States, has constrained national policy in many areas." (55) Moreover, Canadian economic nationalists are sneered at and the use of foreign aid in "the interests of the West" is discussed. This reflects his assumption that Canada's interests are the same as those of the U.S. It might do him a great deal of good to decorate his office with the poster declaring "Being Canadian is Un-American!"

Reuber's benign attitude towards the multinationals is explained by his claim that the "giant modern corporation, such as General Motors or IBM... provides a highly effective means for economic progress..." (207) He obviously believes that it is economies of scale, not market power, which generated GM's past high profits. He accepts the output of the firms, including the gas guzzlers requiring massive public expenditures on roads, as economic progress. His naiveté also shows in the claim that the foreign firm buying a Canadian company will expand its operation rather than maintain it or close it down. He ignores the important distinction between financial and real investment. The former is the inflow of foreign currency, to be converted to Canadian to be spent here. The latter is the purchase of foreign capital goods to add to our productive base. We only need the former to finance the latter and our need for the latter is diminished

when the Canadian economy is in recession and we are better able to generate our own capital goods. Reuber implies the reverse. He claims that "it is widely recognized that Canada continues to reap substantial economic benefits from foreign investment," (229) conveniently ignoring studies such as Powrie to the contrary.<sup>2</sup> He also damns expansionary monetary policy in the 1970s, ignoring the significant role played by the inflow of foreign funds.

The analysis of the current stagflation is unsurprising. Changes in the unemployment insurance regulations make it harder to consider 4 per cent as the upper acceptable limit of unemployment (despite the evidence that about 0.7 percentage points of our unemployment is attributable to unemployment insurance). Unions are cited as a source of stagflation, but corporate power may only "possibly" be a factor "in some cases." (259)

The fundamental flaw in Reuber's thinking, common to mainstream economics, is the inability to recognize that the real world has little to do with the competitive model. Reuber sees the use of power in politics, but not economics. Even in politics, he implies that the corporations are just one of many vested interests to which the government listens. His brand of elite politics and pollyanna economics fails to deal with the fact that political and economic power go hand in hand. Nor does he explain why politicians, occasionally accountable to the public, can serve the public less well than the economic elite which is democratically unaccountable and irresponsible. This is hardly the basis upon which to develop a realistic and useful analysis of political economy.

<sup>2</sup> T.L. Powrie, "The Contribution of Foreign Capital to Canadian Economic Growth," Unpublished Paper, Department of Economics, University of Alberta, 1977.

Desmond Morton with Terry Copp, *Working People. An Illustrated History of Canadian Labour*. (Ottawa: Deneau & Greenberg 1980).

CE N'EST PAS UNE MINCE tâche que d'écrire une synthèse d'histoire du mouvement syndicale dans un pays profondément divisé par l'appartenance régionale et ethnique, et où la législation dans le domaine des relations du travail relève de onze niveaux de gouvernement. Desmond Morton, avec l'aide de Terry Copp, ont relevé le défi avec succès, à plusieurs égards. Leur tâche fut facilitée par l'abondance des travaux en histoire des travailleurs depuis quelques années. Cependant, cet apport ne minimise en rien leurs efforts car le travail qu'ils ont accompli est immense.

Malgré ce que laisse sous-entendre le titre du volume, c'est une histoire du syndicalisme canadien que les deux auteurs nous offrent. Les quelques paragraphes sur la condition ouvrière ou l'action politique servent à illustrer l'action syndicale. Leur synthèse renouvelle considérablement celles qui commençaient à vieillir, soit les travaux de H.A. Logan et Charles Lipton. Il y a un effort pour dépasser l'histoire institutionnelle d'un Logan et pour éviter de sombrer dans l'histoire à ces deux derniers ouvrages, l'histoire du syndicalisme après la guerre reçoit une attention équitable. Il leur a fallu sur cette période récente tracer l'historique d'événements sur lesquels personne ne s'était encore penchée et présenter des interprétations sur des questions encore brûlantes. Les cent cinquante dernières pages du volume me semblent la partie la plus neuve et la plus intéressante du volume.

On peut toujours reprocher à un ouvrage de synthèse d'avoir ignoré tel événement, d'avoir escamoté tel autre ou de laisser de côté de éléments d'interprétation. C'est inévitable dans une synthèse qui couvre près de deux cents ans d'histoire. Les auteurs ont du faire un choix d'événements et insister sur certains aspects plutôt que d'autres. A mon sens,

ce choix a été fait avec intelligence et beaucoup de métier. Si on peut pardonner des lacunes au niveau de la connaissance et de l'interprétation, il faut être cependant plus sévère du côté de la connaissance de l'historiographie du mouvement syndicale.

L'auteur d'une synthèse doit bien maîtriser les sources secondaires de documentation. L'essai bibliographique en fin de volume montre que leur connaissance de l'historiographie canadienne-anglaise s'avère irréprochable. Par contre, ils n'ont manifestement pas lu les ouvrages les plus importants parus sur le syndicalisme québécois depuis dix ans. Je souligne entre autres, les *Mémoires d'Alfred Charpentier* (1971), l'analyse des idéologies de la CSN et de la FTQ par L.M. Tremblay (1972), les *Travailleurs contre l'Etat Bourgeois* de Ethier, Piotte et Reynolds (1975) et, ce qui aurait pu leur être très utile, la bibliographie rétrospective de A. LeBlanc et J. Thwaites sur *Le Monde Ouvrier au Québec* (1973). Leur lecture aurait pu étoffer les quelques paragraphes sur le syndicalisme nationale et catholique et éviter des erreurs comme de définir le Second Front de la CSN à un front de consommateurs (282) et de prêter des intentions anarcho-syndicalistes au Front commun de 1972 (283). Cette lacune me semble d'autant plus décevante que les auteurs ont fait un réel effort pour tenir compte des régions dans leur synthèse. Mais, après tout, ce n'est quand même pas si grave; le Québec n'est qu'une province et la Canada en compte dix.

A un autre niveau, il est intéressant de relever la conception que les deux auteurs possèdent de l'action syndicale et de situer leur perception de la présence du syndicale et de situer leur perception de la présence du syndicalisme international au Canada. L'approche adoptée dépasse l'idée que le syndicalisme est un simple problème de relations industrielles; ils situent son évolution dans le cadre de l'émancipation de la classe ouvrière. Le syndicalisme et l'action politique ouvrière leur apparaissent comme les moyens par

excellente de promotion des travailleurs. Ils ne minimisent pas les luttes ouvrières, mais ils évitent de les interpréter dans le sens d'un conflit irréductible entre classes sociales. Leur analyse laisse penser que notre société capitaliste et démocratique possède la flexibilité nécessaire pour assurer l'avancement des travailleurs.

Il faut noter à ce propos qu'ils attachent une grande importance à l'évolution de CCF-NPD et aux liens qu'il a entre tenue avec le mouvement syndical. Ils mettent en relief souvent les bienfaits de cette association pour les travailleurs. Les gains obtenus dans le domaine de la législation sociale proviendraient de la crainte de la montée du CCF-NPD ou de la voie tracée par les partis sociaux-démocrates au pouvoir dans les provinces. Les auteurs laissent l'impression que des législations comme les pensions de vieillesse, le C.P. 1003 et l'assurance-maladie seraient davantage l'oeuvre du CCF-NPD que du mouvement ouvrier.

En outre, ils ont tendance à faire du syndicalisme internationale la norme syndicale au Canada. Les autres courants syndicaux sont présentés comme l'oeuvre d'irréductibles conservateurs manipulés par les gouvernements, ou encore la fait d'irréalistes révolutionnaires. Le militantisme syndical dans l'Ouest de Canada qui a débouché sur la formation de l'OBU et la grève de Winnipeg est expliqué par la frustration et l'utopie de chefs syndicaux. Les syndicats nationaux qui ont disputé l'adhésion des travailleurs aux "internationaux" deviennent sous leur plume une "nuisance" et une source de division parmi les syndiqués. Le nationalisme n'aurait guère de prise sur les travailleurs; il serait davantage la préoccupation des intellectuels et des politiciens.

Ces quelques critiques sur lesquelles nous avons insisté, ne doivent pas faire oublier que l'ouvrage constitue un apport précieux à l'histoire des travailleurs. En général, il révèle une bonne connaissance de l'historiographie du mouvement ouvrier et ne contient pas d'oubli factuel majeur. Les auteurs insèrent adroitement

l'évolution du syndicalisme dans le contexte économique et politique de chacune des époques. Au niveau de l'interprétation, ils évitent les raccourcis faciles et cherchent à présenter un point de vue balancé en serrant les événements du plus près possible. Leur volume représente, à n'en pas douter, la meilleure synthèse du syndicalisme canadien.

Jacques Rouillard  
Université de Montréal

Kenneth W. Osborne, "Hard-working. Temperate and Peaceable" — *The Portrayal of Workers in Canadian History Textbooks*, Monographs in Education IV (Winnipeg: University of Manitoba, 1980).

THIS VOLUME IS NOT the sort of fare that one normally finds on the menu of the academic historian. It touches upon of all things how learning and values can be influenced by what the student reads. Along the way it appears to snipe at Clio's handmaidens. Finally and worse still, it is the production of an organization that must inhabit the first circle of Dante's *Inferno*: a Faculty of Education. For all of these reasons — and then some — it probably will not be read by those who could profit the most. This is unfortunate for if the book is slim in size (80 pages of clear prose) it is weighty in thought-provoking matter. It is also written by a scholar who has carved out a reputation for himself in the realm of curriculum development and along the way has paid his dues as a practicing historian; his *R.B. Russell and the Labour Movement* (Agin-court, 1978) is known to many devotees of this journal.

What K.W. Osborne does is survey twenty-nine of the principal textbooks used in the public schools of English-speaking Canada since the end of the nineteenth century. What he comes up with is not that earth shattering but it bears heralding and repeating: the textbooks "say very little about working Canadians"

and they have to date not been greatly influenced by the "approaches and findings of the new social history." (73) Simply stated, despite the tremendous changes that have taken place in our so-called "classless society" during the past few decades, despite the efflorescence in interest showered on Canadian working men and women themes, the message is not getting back to the textbooks where it can do the most good.

It would be nice to think that this perceptual gap is being covered by classroom teachers who are aware of the new social history. This is far from the case. For the most part these individuals are the products of the late 50's, 60's, and very early 70's and they were nourished in the universities and teachers colleges by such standard works as McNinnis' *Canada*, Morton's *The Kingdom of Canada*, Creighton's *Dominion of the North*, Careless' *Canada: A Story of Challenge* and Lower's *Colony to Nation*. Mainline interpretations great for their time but often not even suggestive of other avenues of development. For many of these teachers the bloom of classroom interaction has faded. Often hamstrung by provincial syllabi requirements, left to fend for themselves by the Canadian Historical Association and its numerous specialized sub-groups, and, according to the research, not always terribly motivated by the thought of professional development, the textbook with a little enrichment is often all that the student gets. Need more be said!

Not to despair. Osborne at least feels that some encouraging signs of change are appearing. As he points out, "the increasing newsworthiness, prominence, and activity of organized labour makes labour history appear more 'relevant' to textbook writers." (60) Some recent texts have a section here and there on labour and the ubiquitous Winnipeg General Strike is even written about from an "implicitly pro-strike position." (62) It is obvious where Osborne stands with all of this. As a curriculum designer he feels that much

more of the new social history is necessary and he concludes his study by asserting: "To date, the Canadian working class has not been able to give much attention to its relationship with the schools, but the potential is there. And one place to begin is with the textbooks." (80)

If Osborne is clear in his own mind on the need, he does not use the forum to provide a prescription. He does, however, offer an insight that merits being expanded upon within the confines of this review.

Along the way Osborne points to the fact that in Canada it is far from the norm to see academic historians writing texts for the public schools. Indeed, when one glances over the field, with the exception of K. McNaught, *Canada and the United States: A Modern Study* (Toronto, 1963) who with an interest in labour history is doing any textbook writing for the public schools or, for that matter, the universities? Recent articles by G. Kealey and D. Bercuson in *Labour/Le Travailleur* (Spring 1981) on the state of labour history are eloquent on where the priorities are to be found and it is not in the classroom. The New and Not-So-New Labour History with its ideological underpinnings of activism and social involvement is strong on symposia and well-researched articles and monographs, but what about the basic tools of the craft so necessary to reach the vast majority of the school age population that will never do any other history and, for that matter, will never go beyond secondary school studies?

The financial crunch now hitting education throughout the land means that the basic classroom text will take on more and more importance in the public school curriculum. This trend is to be decried, but the handwriting is on the wall. The challenge facing the social historians in general and the labour historians in particular, hence, is to get out of their safe ivory tower. Their work is being lost within the restrictive circle of their colleagues and to the ever-shrinking cohorts of history majors in the universities. Now more than

ever is the time for them to get into the action by adapting and providing for the needs of the public schools. With this their energy will create not only advancement in knowledge but it will also serve to generate change in mentalities, something that Osborne demonstrates as being long overdue.

André E. LeBlanc  
Champlain Regional College

Desmond Morton, *Canada and War, A Military and Political History*, (Toronto: Butterworths, 1981)

THIS STUDY OF CANADA and war is the first in a series of books that attempts to deal with contemporary political issues in their historical perspective. Desmond Morton examines some aspects of the interaction between politics, society, and war in Canada from the mid-nineteenth century to the present day in order to illustrate the very formative influence that the war experience has had on Canadian history.

The first two chapters explore some of the political and social dimensions of the Canadian militia in the nineteenth century and the slow, but gradual, emergence of a military tradition after 1885. Canada's role in the South African War and increased defence spending after 1900 helped to create a more confident and professional military establishment in Canada that was, in many ways, prepared for the Great War. In strictly military terms, Canada emerged from World War I justifiably proud of its contribution to the Allied war effort. The militia may have been prepared for war, but no one else in Canada could have predicted in 1914 the broader impact of the war experience on politics and society.

Professor Morton's discussion of the Great War and the post-war years is first rate. While Canada did not experience "total war" in the European sense, the Great War eventually touched nearly every facet of life in Canada as demonstrated in the expanded role of the state during and after the war particularly in the

area of social welfare responsibility. The war was not a unifying experience either, argues Professor Morton, but rather it accentuated the divisions and tensions within Canadian society and politics. For the inter-war years, the author traces the usual themes of Canadian isolationism and defence cutbacks, but he also looks closely at the problem of the veteran and the failure of re-establishment as a means of illustrating the unsettling influence of war. All of these concerns were swept away in September 1939 with the outbreak of World War II. The long truce was over.

Two chapters are devoted to the 1939-1945 conflict, one on the home front, where the war experience brought a "social revolution," and a second on Canada's contribution to the overseas war effort. Throughout this portion of the book, Professor Morton continues to elaborate on the theme that the war had a significant impact on Canadian politics, society, and economic development. The effects of the two wars on Canada are compared and contrasted in an instructive way, particularly on the conscription crises and immediate post-war economic and industrial readjustment.

The final chapters examine post-war Canada, the Cold War, the development of North American defense systems, and a host of other military issues that have determined Canada's role and position not only on the international scene but more importantly, in her relationship with the United States since 1945. The author concludes that in the dangerous world of the 1980s, it is essential that we understand the war experience in our history because it has been a significant factor in determining the political and social complexion of Canada today.

A few comments on the format of the book are necessary. There are no footnotes to the text, although the major sources for each chapter are described at the end of the book with a brief annotation. It is obvious that Professor Morton has drawn on his extensive knowledge of

primary source material in the preparation of this study and it is regrettable that the format did not allow him to discuss some of the available documentation in more detail. Nevertheless, the recommended readings constitute an excellent introduction to the literature of Canada and war.

*Canada and War* is a well-balanced and readable study of a complex subject, a study that will undoubtedly encourage further research into other aspects of Canada's military experience and its influence on our history.

Glenn T. Wright  
Public Archives of Canada

Donald Creighton, *The Passionate Observer: Selected Writings* (Toronto: McClelland and Stewart 1980).

IN AN EARLY ESSAY on the writing of Canadian history, Donald Creighton drew an analogy between the career of Goldwin Smith and the failure of intellectual life in Canada. Smith, he argued, could have been a figure of national stature, he could have led the young Canada First movement, but he chose instead to descend to the level of a partisan propagandist for continentalism and liberalism. Goldwin Smith styled himself a "bystander" — and Creighton certainly accepts the ideal of the detached intellectual — but the ideal was not observed in practice. "Nobody was ever less a bystander. . . . He was an intellectual partisan. . . . He was a journalist among other journalists. . . . Smith was a one-man party. . . to which he attached himself with all his petulant vanity and rancorous vindictiveness." Creighton tried to rescue Canadian intellectual life. Almost all of his work can be seen as an attempt to create a national culture: in this sense Laurentianism provided the content for this culture, while romantic idealism provided the framework.

*The Passionate Observer* does not extend Creighton's vision of Canada, nor does it provide any new sense of depth to the man's extensive scholarly record. It does, however, show the frustrations (and

failures) that he encountered on his intellectual and cultural quest. If Creighton found the spectre of Goldwin Smith lurking behind the writing of Canadian history, it would seem that Smith's shadow fell across many other aspects of Canadian life.

*The Passionate Observer* was published after Creighton's death, and draws together a number of recent essays and reviews. The first section contains essays of contemporary issues (Quebec, the Pickering Airport, the LCBO), the second contains a short collection on the art and practice of reviewing books, and the third a series of character portraits of some of Creighton's well-known academic contemporaries. The second section is the most enjoyable. Creighton has often related the story of the origins of his own literary career, how he began by opening the great parcels of books that came for his father who was the editor of the *Christian Guardian*. These reviews not only show how Creighton mastered this art, but also how well this art form suited his own dispositions. He longed to be a part of a grand literary culture, like the late Victorian period, in which intelligent people could conduct an ongoing learned discourse at the highest level. Some of Creighton's own letters to the editor might well have been included here — I remember one that defended Peter Newman from the Canadian historical establishment that was a masterpiece of rhetoric and argument.

The other two sections of the book present an image of Creighton that is unsettling and even tragic. Something snaps when Creighton writes on contemporary events, and it is not a question of old age and prejudice. One can see the man searching for the grand historical principles that run beneath the contemporary world, the principles that he can use to organize events and personalities into an intelligible narrative. But he can not find them. Creighton is a moralist in the Victorian sense of that word, but the modern world has no morality; or to put



the matter another way. Creighton's morality is individualistic: he looks for personality rather than the moral dimension of institutions, systems of technology, and class (in the manner of George Grant). Consequently he tends to fall back upon a host of traditional villains, such as French influence and American domination. He is more like Jeremiah than Isaiah — a prophet who is certain of the corruption of the modern world, but not at all secure in the knowledge that God will lead history to a richer life.

His literary style suffers for the same reason. He certainly did not lose the ability to create the memorable phrase or draw the short, pungent character sketch (J.W. Dafoe was "a big, gross man, with a dogmatic, hectoring 'how-wow' manner, a truculent and abrasive style, an intense partisan dislike to both Tories and socialists, and a psychotic hatred of what he thought of as 'British imperialism'"). These literary talents were nurtured on the masters of the late Victorian period, men like Galsworthy, Zola, Balzac, and Wagner. Regrettably, the modern world marches to a different rhythm. The words might be devastatingly brilliant, but the aesthetic within which they live does not penetrate very deeply into contemporary life. Caricature might make us believe that Trudeau (and most Liberals) are morally bankrupt; but how much does this conclusion tell us about liberalism and modern Canada? The style did not fail but the world it had to explain had changed almost beyond recognition.

The biographical essays also suggest that there was a similar tension in his views on the character of scholarship and academic life. The God-like figure among Creighton's friends is Harold Innis: a man who was in Creighton's judgement at once detached from current affairs and a convinced Canadian nationalist, a first-class scholar who had an outspoken sense of what was moral and correct in human and university affairs. In short, the man who fulfilled the promise that Goldwin Smith

had foresaken. It is against this ideal that Creighton implicitly comments upon the lives of other men.

The collection republishes Creighton's essay on Underhill and the Ogdensburg Agreement. Not many people will applaud this choice. The war between these two was not stayed even by death. But beyond propriety is the question of the type of judgement that Creighton makes. He draws a very fine distinction between academic freedom and academic responsibility, so that he at least tacitly agrees with Underhill's analysis of the continentalist implications of the Ogdensburg Agreement (and must defend him from Cody and the Board of Governors). Nonetheless Creighton is quite willing to attack Underhill's academic standing because he supported publicly and with enthusiasm the course that history had chosen to follow. It is a mark of Underhill's lack of detachment that he went over to the enemy who rewarded him with curatorship at Laurier House.

The other three historians (Brebner, Stacey and Forsey) are given high praise but Creighton again adjusts the Innisian rubrics in a curious way. One might wonder what exactly separates the internationalism of Brebner from the continentalism of Underhill, other than the geometrical difference between a triangle and a straight line. Is Forsey any more detached from the world of contemporary political affairs? Was there any major historian who had never supped at the table of the Liberal government? This group of men are clearly important scholars, and Creighton is not being hypocritical in his judgements. The source of tension is to be found in the framework itself. He continued to hold on to the Victorian ideal of academic disengagement, he continued to believe that art and scholarship were abstract values, and he continued to judge people and politics in moral terms. The consistency of Creighton's position is remarkable and praiseworthy. But the

romantic framework itself had fallen apart. Universities, governments, and societies operate according to different interests, and this is one source of the sense of the tragic that runs through this volume.

William Westfall  
York University

Philip S. Foner, *Women and the American Labor Movement from Colonial Times to the Eve of World War I* (New York: Free Press 1979); James J. Kenneally, *Women and American Trade Unions* (St. Albans, Vt.: Eden Press 1978); Gerda Lerner, *The Majority Finds Its Past* (New York: Oxford 1979); and Lois Scharf, *To Work and to Wed. Female Employment, Feminism and the Great Depression* (Westport, Ct.: Greenwood 1980).

LIKE THE HISTORY of the working class, the history of women has come a good long way since the 1960s. Progress has been especially dramatic because the female experience, still more than that of the working class, had been so badly neglected. In the United States the richness of this new scholarship is reaffirmed every two years at the Berkshire Conference of Women Historians which brings together an impressive number of scholars committed to the recovery of women's individual and collective past. Among the foremost in these richly talented assemblies is Gerda Lerner, a figure who in many ways may be identified as the doyenne of feminist women's history in the United States. Since the mid-1960s she has set a powerful standard of revisionist excellence. That standard, so well displayed in *The Majority Finds Its Past. Placing Women in History* (1979), is a good measure by which to judge three entries in the field: Philip S. Foner, *Women and the American Labor Movement* (1979), James J. Kenneally, *Women and American Trade Unions* (1978), and Lois Scharf, *To Work and to Wed. Female Employment, Feminism and the Great Depression* (1980).

Lerner, first well-known as the author

of *The Grimke Sisters from South Carolina: Rebels Against Slavery* (1967), has provided a volume rich in intellectual and social history, not the least of which is the introductory "Autobiographical Notes." This confident assessment of a life and a career which began in Austria, included a flight before Nazi persecution, marriage, children and an MA and a Ph.D. in her forties at Columbia University, illuminates the process, often slow and hard by which feminist scholars have come into their own. This is no humble, self-effacing, "lady" academic. Gerda Lerner is one who knows, after hard testing, her own worth.

The 12 essays in this collection appeared originally between 1969 and 1977. There are five essentially historiographic contributions, including one which sets out some of the basic issues raised by the experience of black American women. These are deservedly well-known among practitioners of women's history. They raise the essential problems of methodology, sources, and interpretations for the new field. In their discussion of the relationship between the "old" and the "new" women's history, they also illuminate the theoretical and practical choices facing feminist scholarship. These should be required reading for all who claim to be students of the human past. Lerner's ability to meet the high standards she sets is successfully put to the test in two provocative articles illuminating blacks' experience as club-women and the interaction of black and white reformers in the nineteenth and early twentieth centuries. Equally valuable are two pieces on Jacksonian and abolitionist women which raise important issues of class differences, popular support, and conventional periodization. Lerner's breadth of analysis and research is confirmed further in three penetrating, hard hitting inquiries entitled "The Feminists: A Second Look," "Women's Rights and American Feminism," and "Just a Housewife." The range is impres-

sive, the treatment non-polemical, and the inspiration ever-present. The critical variable, sex, is assessed as working with, and as often against, class and race to produce a host of results over time and space. As a consequence, women's history demands, as Lerner so well argues, "a fundamental re-evaluation of the assumption and methodology of traditional history and traditional thought. It challenges the traditional assumption that man is the measure of all that is significant, and that the activities pursued by men are by definition significant, while those pursued by women are subordinate in importance." (180) This essential re-evaluation is a difficult task. Old training and traditions hang on grimly. Witness the majority of undergraduates who still fail to encounter serious treatment of women in their classes, history and otherwise.

As the author of more than 40 books on a wide variety of subjects, the writing mill which is Philip Foner has turned inexorably to a very ambitious study of American working women from colonial days to the present. In this the first of a projected two volumes the account is taken to 1914. Foner acknowledges that labour history today is something more than "workers on the job in their collective organizations and actions" but insists that *much remains untold* about "working women and the American labour movement." Their "organized resistance" contradicts "the tendency to view women's past as one of undifferentiated subjection and passive victimization, in short, a chronicle of failure." (x) In over 600 pages of text and notes which depend heavily on less senior, if earlier, workers in the vineyard, Foner superintends the recovery and commemoration of the self-conscious waged heroine. The result is uneven. The colonial and revolutionary years, for example, are reviewed briefly in 19 pages as a time of relative, if declining, equality. The entire period prior to the Civil War is dismissed in just over 100 pages, an imbalance reflecting the limited development of the American labour

movement to that date as well as the shortage of monographs, especially for the years before the break with Great Britain. Of most practical use here is Chapter Six's ten-page sketch of the situation of black women before the Civil War. Matters are suitably summed up for the undergraduate but with *none of the sophistication* Lerner evidences when considering the same group. The remainder of the book benefits from the extensive studies now available on the later period as well as some new research by the author. As such, chapters on "The Socialist Party and Socialist Women," "Birth of the National Women's Trade Union League," and "The Lawrence Strike" provide handy guides to both primary and secondary literature.

Indeed the bibliographic function constitutes *perhaps the most significant contribution* of the book. Readers new to the history of working women are introduced to a large, if of limited range, array of materials focusing on women's economic struggles in the United States. Of similar value for quick reference are the abundant sketches of heroic figures such as Mother Jones, Sojourner Truth, Elizabeth Rodgers, Leonora M. Barry, Rose Pastor Stokes, Addie W. Hunter, and Alice Hamilton. The identification of a host of women's strikes and economic protests of various kinds in which women participated serves as a convenient reminder of female activism on the labour scene. Foner's provision of such material is reminiscent of early examples of labour history which concentrated on unions, strikes, and personalities. In this case the women replace the men centre stage. To be fair there is some attempt to supply the social context in which women's lives evolved but this generally succumbs to the need to cover lengthy periods of time and large amounts of material. The result sacrifices in-depth research and suggestive analysis to the piling up of yet further proof that women were not passive victims of circumstance. It is a step in the right direc-

tion but it is an inherently limited account which sidesteps the task which Lerner would have historians of women undertake. Sources, methodology, and approach are traditional, only the faces change.

The same limitations and more evident weaknesses are found in James Kenneally's *Women and American Trade Unions*. This well-intentioned study begins with the period after the Civil War and moves quickly and largely superficially through to the 1970s. Like Foner's volume, which is almost three times its length, the focus is organized female protest. There is no significant assessment of women's wider lives either within the workplace or beyond. In addition, it is marred by petty errors such as the reference to American "suffragettes," (3) dubious assumptions, (see the sentence "Women contributed to their own degradation for despite the organizing successes of the Knights of Labor most continued to resist unionization," [18]), irrelevant, sometimes silly, digressions as the discussion of one woman's attempt to persuade Samuel Gompers "to dress more attractively," (21) and contradictions as with the confused treatment of Gompers' attitudes to working women in Chapter Three. According to the preface this book developed out of a series of short biographies. In some ways these still form the backbone of the volume, but Foner's efforts in this regard are again better. Seemingly without conscious intent, *Women and American Trade Unions* often in fact ends up as little more than an extended description of the Women's Trade Union League. A third of the chapters as well as material elsewhere concentrates on the largely familiar activities and personalities of that body. Unfortunately, even that very interesting feminist experiment has been appraised more successfully by others including the pioneer study by Gladys Boone, *The Women's Trade Union Leagues in Great Britain and the United States of America* (1942). Finally, scholarship which avoids necessary footnoting

hardly deserves the name. What we have here is a well-meaning but inherently limited endeavour to latch on to a supposedly "trendy," that is marketable, subject. This volume should be scrupulously detoured by anyone seeking the new territory recommended by Lerner.

All is not lost, however, since exciting new work is also appearing. Lois Scharf's *To Work and to Wed: Female Employment, Feminism and the Great Depression* opens up a rich landscape in which female characters assume leading roles. Scharf charts with great success the often debated question of what happened to feminism after suffrage. In doing so she treats readers to more than her title promises. The feminism of the 1920s is dissected very neatly to reveal an underappreciated integrity and intelligence. In contrast to the intellectual bankrupts portrayed by William O'Neill and others, there were feminists who knew very well where at least some of the important issues lay. In the decade after World War I they "turned... to proposing change in women's economic roles. They advocated paid employment for married women to improve the social and economic status and the psychological self-esteem of wives. They believed women had abilities beyond those required by familiar responsibilities and domesticity and deserved the opportunity to utilize them without relinquishing the emotional gratifications that derived from love, marriage and motherhood. Above all, they argued that women required financial compensation for productive labor, since economic dependence lay at the foundation of freedom and equality." (42) These champions of working wives were for the most part "native-born, well educated professional women." Few of these "new style feminists" (22) had been prominent in social reform or suffrage. Most ignored the predicament of working-class wives, for whom personal fulfillment on the job was a marginal consideration, and similarly failed to come to terms with the dull

dead-end employments which fell to the lot of the majority of female wage-earners. They also evidenced extreme ambivalence and finally undue optimism regarding the sharing of domestic responsibilities in families where both husband and wife went out to work. As Chapter Two on the 1920s makes clear the feminist "careerists" by no means had all the answers. Nevertheless in focusing on women's need for economic independence they were raising some of the right questions.

The onset of the depression in the 1930s reined in the intellectual debate centering on married women and their right to work. Instead feminists found themselves fighting a bitter rear-guard action to defend wives whose place in the labour force came under massive assault by those who wished to guarantee male economic superiority and the patriarchal family as a "haven in a heartless world." The refusal to hire or the discharge of married women had occurred in the 1920s, but a generally buoyant economy had provided important protection. In bad times working wives found few allies. Chapter Three documents the complicity of governments in the widespread effort to drive out female workers who were condemned as selfish, anti-social, and a threat to the family. In face of this attack women's defenders closed ranks in a show of unity unprecedented since the suffrage campaign. This was not the only familiar development. Depression feminists like their predecessors grew narrow and defensive, straying far from the equal right argument to emphasize the altruism and self-sacrifice which motivated working wives. Women were praised as they had been in the days before the vote was won for their self-denial and family orientation.

Even this pragmatic reduction of expectations, however, did not guarantee economic security. In the 1930s the employment of all women, both married and single, remained at best precarious.

Chapter Four describes the desperate situation facing female teachers in every state. Although their reception varied widely across the country, teaching wives upheld by feminist supporters managed to hold their own. Ironically, young single women lost ground almost everywhere to male competitors who once again found education a profitable field of endeavour. Such losses placed severe limits on women's power in one of the few professions they could hope to control.

Competition between women and between women and men for scarce jobs in other professional, white collar and even industrial employments had similarly heavy costs in "economic hardship and psychological disillusionment" (86) for female citizens. Status, wages, and working conditions deteriorated steadily for most librarians, social workers, and nurses but remained sufficient to attract a host of new male rivals. Women's predicament was still worse in male-dominated professional and managerial positions. The absence of female role models in law, medicine, dentistry, management, and universities reinforced reduced ambitions for female job-seekers.

Men's replacement of women in the better posts increased competition among women for the remaining jobs. The result could be dramatic downward mobility. When college graduates lined up to serve as secretaries and clerks, girls' limited horizons were yet again affirmed. Low status, low wage employments had, however, their own reward. With each step down the occupational ladder discriminatory practices decreased. Few wished to remove women, whether married or single, from menial jobs as laundresses, clerks, or domestics. The real impact of discouragement was not to eliminate married women from the work force, indeed their numbers increased, although more slowly during the 1930s. The result was to assign married women overwhelmingly to white collar job ghettos. Work with few rewards became the accepted pattern for

the wives who increasingly dominated the female labour force. Occupational aspirations were correspondingly undercut for all young women. Feminist hopes would take a long time to recover.

The New Deal did nothing to change this picture as Chapter Six demonstrates. Despite the efforts of sympathizers like Eleanor Roosevelt and Secretary of Labor Frances Perkins, recovery programmes pursued the prevailing policy of discrimination. As always, black women were the most disadvantaged. Even women's prominence in the strikes of the period did not change this picture. The difficulty was not so much lack of government sympathy for women as the sheer extent of the crisis. When all other values and traditions seem threatened, few had the stamina or the courage to tamper with male-female roles. The personal and psychological costs of the status quo could nevertheless be painful as Chapter Seven indicates. Notions of masculinity and femininity, even in near destitute households, were often linked closely to waged work for husbands and home responsibilities for wives. Yet levels of consumption depended increasingly on two pay packages. The result of this contradiction was, as sociologists refused to recognize, a double burden for married women. It was hardly any wonder then that many wives preferred, if they could, to stay home. In her Epilogue Scharf indicates that the return to prosperity did not change matters much. Married women continued to move into waged work. There, in pink ghettos, they laboured in increasing numbers. Only the rebirth of feminism would again challenge this subordination. Scharf's important study closes with an "Essay on Sources" which is a model of its kind. Here are critical signposts for those studying American women in the period.

To conclude, *To Work and to Wed* enters upon the type of exploration recommended by Gerda Lerner. Sources, methodology, periodization, and perspective all receive the careful, critical examina-

tion which is too often missing in the volumes by Foner and Kenneally. The result is a highly readable and provocative challenge to conventional histories of the period. This is the type of scholarship we need if the majority is indeed to recover its past.

Veronica Strong-Boag  
Simon Fraser University

Keith Wrightson and David Levine, *Poverty and Piety in an English Village: Terling 1525-1700* (New York: Academic Press 1979).

SOCIAL HISTORIANS of early modern England have turned in recent years to the village study to analyze the lives of "ordinary people" in a precise context. Keith Wrightson and David Levine, working within this tradition, have chosen the Essex village of Terling between 1525 and 1700 as their subject. They have a broad range of sources because Terling has an excellent parish register, and the records of most of the local administrative courts, civil and ecclesiastical, have survived. The book began with a study of illegitimacy, but the research led the authors to broader questions, as the title of their book indicates. *Poverty and Piety* is chiefly concerned with social structure, social control, and religion. This book is important but often frustrating, and it demonstrates both the possibilities and limitations of the village study as a method.

Wrightson and Levine argue that in Terling, as in most other English villages, the end of the sixteenth century saw a rapid increase in the population caused by higher fertility rates which resulted from a lower age of marriage than in other periods. This led to an increase in the proportion of labourers in the village. They also argue that the resultant polarization was not only economic, but also cultural; this is not unexpected, but it is significant. Cultural polarization was affected by the withdrawal of the parish elite from communal sociability — most notably in the alehouse — and by the imposition of "godly"

behaviour through court prosecutions. This polarization was related to the Puritanism of the village elite, but the significance of agrarian capitalism in the village for both these phenomena remains unclear. Wrightson and Levine go no further than noting that the village Puritans were largely drawn from within the middling and upper ranks of village society. This book provides our first glimpse of how membership in the local elite affected both an individual's behaviour and his perception of his fellows.

The impact of Wrightson and Levine's examination of economic and cultural polarization is weakened, however, by their evasions about the nature of the social system. Social structure is described by dividing the population into Groups I, II, III, and IV. The groups are based on the distribution of wealth in the thorough taxations of 1524 and 1671, and then linked to occupational or status designations which allow their use at other times. Cottagers, labourers, and paupers are placed in the same group, and any differences which might exist between them are hidden by the categories. By defining groups on the basis of status and occupational designations, the authors have avoided exploring what the designations mean. For example, they show that most of those holding certain offices belong to Group II (yeomen), but they never discuss why some members of Group III (husbandmen) also held those offices. They suggest that the meaning of the offices changes later in the period, but never explain earlier exceptions. Such a discussion would illuminate the sources of power and prestige in early modern communities. Early modernists are understandably chary of the word class, but some discussion of how the developments traced in Terling contribute to the emergence of a class society would have been useful, especially if it explored the complex of factors which affected one's social position.

A more serious conceptual weakness of a book that began with an interest in illegitimacy and which includes a family

reconstitution is the absence of any consideration of women's social experience. This is a book about men; differences between women and men are noted but not examined. The higher infant mortality of girls is mentioned only with the comment that it is unexpected. (57) The discussion of changes in fertility considers only economic factors, while cultural norms, health, and nutrition are never mentioned. This myopia is underscored by the proof that childbirth was not dangerous for mothers and children: "less than one child in seven died in its first year of life." (58) According to Wrightson and Levine's figures, this would mean that there was a 50 per cent chance that one child born to a woman would die in its first year.

*Poverty and Piety* is a difficult book because the chapters are not integrated, and they are also uneven. The "National Context" is a clear and accessible survey of general trends easily used by undergraduates. "Demographic Structures" is complicated even for someone familiar with demography; it also assumes there is a demographic "rationale" which leads people to limit fertility when there are fewer opportunities for children — an awkward assumption in a book which stresses the importance of culture in shaping behaviour. "A Local Social System" is merely a run through of questions asked about social systems; after a statistical description of one subject, the authors move on to the next with no further analysis. Yet this is followed by the most important and suggestive chapters in the book, those on "Conflict and Control," and "Education and Religion," where Wrightson and Levine describe the increase in the regulation of personal behaviour at the same time as the central group of local notables becomes Puritan. Only in the final chapter do the connections become clear, and then the authors regret that they have said so little about those on the bottom of the social scale. In their last sentence they admit they have been describing the oppression of the poor — "A shudder of pain vibrates across

the centuries" (185) — but it is awkward, not moving. Wrightson and Levine have written a straightforward and careful description of Terling, where the impact of capitalism and Puritanism on relations between the villagers is striking. Only by looking carefully at one village can the impact of these forces be so clear. In the end, however, one longs for more of a context, to know how common Terling's experience was. *Poverty and Piety* raises important questions, but it shortchanges itself by not exploring the implications of many of its own findings. Doing so might have brought more substantive criticism, but it would have added far more to our understanding of early modern village life.

Susan D. Amussen  
Brown University

John Stevenson, *Popular Disturbances in England 1700-1870* (London: Longman 1979) and Gwyn A. Williams, *The Merthyr Rising* (London: Croom Helm 1978).

ONE OF THE MOST exciting and important developments in the rapidly expanding field of social history in the past two decades has been the rediscovery of popular disturbance and the protean forms of social conflict which characterized the pre-industrial era. Ever since George Rudé's pioneering studies of the crowd and Edward Thompson's brilliant study of the early English working class, historians have been busily exploring those spheres of plebeian militancy hitherto consigned to the margins of labour history and reconstructing both the prehistory of the labouring poor and its relations with authority. Among the latest batch of books on the subject are the two under review. The first, by John Stevenson, is an attempt to summarize the current state of the English literature on popular protest from the post 1688 era to the mid-nineteenth century, charting the long-term patterns in popular discontent which characterized this transitional period in the growth of English capitalism.

The second, by Gwyn Williams, concentrates on a particular moment in Welsh history, the Merthyr Rising of 1831, which heralded the emergence of a working class in industrial South Wales.

Gwyn Williams' book is in a real sense a very personal record since the story of the Merthyr Rising has resonated in the oral tradition of the Welsh working class for over a century and he was literally weaned on the folk-lore of its martyr, Dic Penderyn. His total immersion in the rising gives his reconstruction a passion and immediacy that is quite infectious without doing an injustice to the historical record. One has only to compare his account with that of David Jones in *Before Rebecca* to recognize his deep involvement in the early history of his native town. It is also, predictably, informed by a wonderful sense of place. We are not only presented with an authoritative sketch of Merthyr as the matrix of the South Wales iron industry, but of its settlements, dominated by the Dowlais and Cyfarthfa ironworks, its immigrant population, and the immigrants' religious and political tradition. Merthyr was a booming industrial community in 1831, unsanitary, unplanned, prone to epidemic crises of debt and relief determined by the state of the iron industry, attracting migrants with the prospect of good wages, however short-time and seasonal they may have been, and administered by a predominantly Unitarian shopocracy which periodically balked at the reluctance of the ironmasters to contribute their fair share of the poor rates.

The influential Unitarian minority played a crucial mediating role in the rising of 1831, for it was largely under their aegis that radical ideas took root. This is not to deny the extraordinary democratic energy of the other dissenting chapels and the unevangelical populism of the pubs and *penillions*, but it was largely through the Jacobin tradition of the petty-bourgeoisie that working-class activism surfaced. It did so against a background of Reform euphoria and a severe economic depression which forced the ironmasters to cut wages



and to force workers on the parish. In this conjuncture the agitation for reform quickly assumed another dimension, generating a demand for a moral economy of provision and the abolition of the Court of Requests which culminated in the repossession of distrained property and a bloody confrontation with authority. What began as a reform movement under middle class leadership, abetted by the enigmatic ironmaster William Crawshay, was transformed into an insurrectionary protest against the shopocracy and by extension the real powers in Merthyr society.

Williams is particularly good at showing, with deep sympathy, the relative immaturity and traditionalism of the workers' initial protest which was directed at those middlemen and officials most visibly enmeshed in the chronic cycle of debt and penury. He argues convincingly that the moral hegemony of the ironmasters inhibited a real understanding of the crisis until the local contradictions of the situation, sharpened by repression, forced the insurgents to see themselves "as a working-class, defined against the rest of society." Once this happened trade unionism became a real possibility and a distinctive working-class presence was forged, to inform both the town's electoral politics and its Chartism. Not until the mid-century did working-class radicalism fuse with non-conformity to turn proletarian activism into the marching wing of Liberalism.

In reconstructing this crucial episode in Merthyr's history Gwyn Williams had adopted an almost Thompsonian paradigm, reasserting the primacy of agency and consciousness in the formation of a working-class identity. It has been modified, certainly, as Thompson's more recent work has been, by the Gramscian notion of hegemony; and indeed his account of the mediated but determinant power of the ironmasters, whose influence over their workers was at times almost tribal, is one of the more impressive features of the book. But it is a pity he does not follow his story through the post-

insurrectionary decades in more detail, for in what seems a tangled and complex political aftermath we have to accept some of his assertions about the centrality of a working-class presence, periodically fractured by a reassertion of the old rivalries between Dowlais and Cyfarthfa, upon trust. One would also like a more systematic treatment of the mid-century process of liberalization, a subject broached by John Foster in his study of Oldham, whose influence Williams specifically acknowledges. In particular more needs to be said about the role of the intermediate classes who are sometimes handled impressionistically, even described as the "elite" of the town and the "gentry." Yet despite these lapses and the occasional verbal exaggeration — the insurrection is sometimes characterized as revolutionary, "designed to effect Reform and overthrow a social order" (224) — this is a memorable evocation of a decisive turning-point in Welsh working-class history, written with characteristic exuberance and vivacity.

By contrast John Stevenson's book seems remarkably stolid and at times pedestrian. It is true of course that a survey of popular disturbances over a century and a half does not exactly lend itself to the drama of a particular rising. But the problem lies less in the subject itself than in the approach. Rather than highlight the new methodological departures in the field and their possibilities for a new exploration of eighteenth and early nineteenth-century English society, Stevenson has gone in for mass empirical coverage within a basically conventional narrative mode. In his introductory chapter, admittedly, he did outline the various approaches to the history of the crowd and popular disturbance. But his treatment of their theoretical implications is summary indeed, and nowhere does he define the problematic within which they are situated. The result is that the student has little sense of the current trends in crowd historiography or of their wider ramifications over and above certain technical problems of evidence. What he/she receives instead is a *vade mecum* of riot, a

loosely constructed, derivative and essentially episodic history of disturbance from the Augustan to the Victorian era.

This might have been an acceptable procedure in the days when crowd history was in the chrysalis stage, but it is scarcely appropriate today. Historians, save for the unrepentant quantifiers, have become increasingly aware that riots must be placed within very specific contexts, in time and place; and that more often than not they formed one phase of a complex series of negotiations whose history must be unravelled before their true meaning is disclosed. Consequently historians have examined riots to explore the relations between the labouring poor and authority, to challenge received truths about the paternalistic *timbre* of pre-industrial society, for example, or equality before the law. Alternatively they have analyzed the various modes of disturbance, their rituals and ceremonies, to reveal aspects of popular *mentalités* that would have otherwise remained inaccessible. In both France and England, for instance, there has been a flurry of inquiry into the cultural origins of riot, how forms of direct action grew out of older patterns of festive life, and what this may tell us about plebeian expectations and critical shifts in popular political consciousness.

As his references reveal, Dr. Stevenson is obviously aware of this literature but rarely does it inform his analysis. His discussion of folk ritual and popular festival, for example, is slotted into a miscellaneous chapter entitled "Manifold Disorders" when it might have been appropriately associated with politicization of the public calendar during the Stuart era and the riotous royal anniversaries of the post-Revolution decades. Had this been done, it might have been possible to analyse the changing idiom of political protest and what this meant in terms of popular political aspirations, prevailing ideologies, and plebeian self-assertion. Sections on recruiting riots, press gang affrays, smugglers, and anti-tax disturbances might have been linked to the increasing

intervention of the state in the eighteenth century and the imperatives of a war economy, dimensions that are never mentioned.

Above all, what is needed is some regional breakdown so that the conflicts between town and country, province and capital, agrarian and proto-industrial economies can be satisfactorily explored and their patterns of allegiance adequately defined. In the section on bread riots, in particular, one is surprised to discover no maps outlining the areas of disturbance during the principal crisis years and no analysis of their geography over time. Nor is there any discussion of the way in which the authorities handled riots in the absence of troops and the degree to which political divisions within the landed class may have influenced their conduct.

Much of the book is taken up with political disturbances and here Dr. Stevenson fares better in his own period of specialization, the early nineteenth century, than in the earlier decades. He makes the important point that many of the Regency disorders can be attributed to food shortages and war-time dislocations rather than to radical grievances and questions the extent to which Jacobinism took root among the common people. There is also an interesting portrayal of Burdettism and the Queen Caroline affair and of the problems confronting the popular radical movement of the 1830s, which eschewed violence but was forced to adopt "the language of menace" to overawe an unsympathetic Parliament. The earlier chapters, however, are less impressive. Too great an emphasis is placed on London, which while crucially important in mobilizing opinion "without doors," was not the only city with a reputation for political turbulence. And his treatment of popular Jacobinism, which can only be viewed within a regional context and in terms of the language of ridicule and satire, is remarkably thin. There are also some factual errors. Bristol was not the only town to experience serious disorders on coronation day 1714; the newspapers reported riots in some 30 centres. The

political calendar cited on page 18 was certainly more complex than Stevenson imagines and more importantly changed over time. By 1790 radicals were even commemorating the birth of George Washington! Alderman Sawbridge was not a member of the Protestant Association (88) and in the aftermath of the Gordon riots was even taken to task for his toleration of Catholics. Military law was not declared during these riots either; what the Privy Council did was to issue a proclamation allowing the Commander-in-chief of the forces to act without civil direction.

Errors of this kind are perhaps inevitable in a survey covering over a century of disturbance. But that is not the central criticism I wish to make of this book. It is that Dr. Stevenson has not addressed the problems which are currently engaging the historians of protest and popular disturbance, or the new methodological departures, both of which bear crucially on how we view power relations in this age of transition. This book merely embellishes George Rudé's early survey of the English crowd, taking the story to the liberal consensus of the Victorian period. It does not add much that is new and disconcertingly hedges on many of the major questions.

Nicholas Rogers  
York University

Iorwerth Prothero, *Artisans and Politics in Early Nineteenth-Century London: John Gast and His Times* (Baton Rouge: Louisiana State Press 1979).

"THE INDUSTRIOUS CLASSES are the wealth and strength of the nations, and nothing but their own conduct makes them poor and impotent." Thus did John Gast, shipwright and radical, exhort his fellows during the depression of 1826. This was not, however, a universal rallying cry, for he spoke not to the working class at large but to the artisanat, whom he long cham-

pioned in a variety of causes, as a necessarily select interest or order in society whose rights and status were perilously embattled in the post-war years. In this most valuable study, Prothero argues for the primacy of the skilled trades in the making of a militant political consciousness in early industrial society, emphasizing the interrelationship rather than the compartmentalization of trade and political movements. Plainly this lies in direct line of historiographical descent from E.P. Thompson, but it is a mature and self-sufficient piece of substantive scholarship that breaks new ground and attends more than previous studies to the recovery of workshop practice as a key to worker culture and politics. As in Thompson, there is an emphasis upon continuities of ideology and action, though Prothero's focus on London provides a more closely textured account which brings out the diversities in the artisan experience and the shifting locus of political initiative within the stratum. Here was a definable social group, at once homologous yet divergent. What seems contradictory and fragmentary, argues Prothero, is to be understood as complementary, if we properly comprehend the range and flux of artisan thought in what Thompson claimed was "perhaps the most distinguished popular culture England has known."

The political career of the artisanat is thus set within the context of the economy and culture of the trades in a city whose multiplicity of crafts and workshops made it "the Athens of the artisan." Artisan values have been examined in a variety of settings and eras and there is much that is familiar here — the regard for skill as a form of property, the concern for personal independence and "respectability" within a collectively regulated commonweal of small producers, the celebratory integration of work, leisure, and social obligation — but the virtue of this account is its meticulous analysis of a culture under pressure. Though spared the travails of extensive modern industrialization, the London

trades were threatened by a growing labour surplus and foreign and provincial competition in an economy subject to abrupt cyclical depressions. Traditional controls on recruitment, hours, and earnings came under attack from the state, the developing capitalism of the bigger masters, and the price-cutting of unemployed journeymen forced into dishonourable practice. Artisans suffered too from over-taxation and underrepresentation in a closed and corrupt political system that fought political and economic combination by proscription, harassment, and infiltration. If Prothero's account is overall a record of progressive gains in worker consciousness and organization, it is not a conventional linear treatment, but a reconstruction of the immediate and often obdurate historical present within which artisans struggled to comprehend and respond to crisis. This they did through strikes and unionization, lobbying and demonstration, co-operative and educational organizations, and an independent press. A great deal of new evidence is brought to light here, particularly for the 1820s, a decade badly neglected by historians. The autonomy and resourcefulness of artisan culture revealed here was formidable and dramatic.

John Gast was prominently involved in the main worker movements of the period, and though the materials for a full blown biography are frustratingly incomplete, Prothero makes good his claim that the conventional labelling of pre-Chartist London as the London of Francis Place ought more accurately to read the London of John Gast. Unlike Place, Gast remained a working craftsman all his life and placed his faith in artisan action independent of middle class support. Unlike Place he spoke for the majority of his fellows in contesting the claims of Malthus and political economy, and though a rationalist his career involved him among other things in millenarianism and fortune telling — not as eccentricities but as part of the restless quest and experimentation

that characterized the working-class radicalism of the period. He talked of resort to violence in contrast to Place's moderation. But Gast (though himself briefly a publican) was at one with the diminutive sage of Charing Cross in his abhorrence of working-class addiction to "pot and pipe" as conduct which jeopardized political mobilization and the respectability of the craft interest. The other self-inflicted impediment he bemoaned was the particularism of the trades — what he sharply labelled their "presumptive self-consequence." Gast was foremost in the campaign for general union and there were attempts to enlist workers outside the crafts, but the problems of consolidation were exemplified in his own trade which reverted to a traditional sectionalism in the last years of his life.

*Artisans and Politics* is clearly indispensable to the study of pre-Chartist popular radicalism and beyond. Looking ahead, Prothero sees a continuity between the radicalism of Gast's world and that of mid-Victorian artisan liberalism which still preserved an irreducible core of independence among its apparent contradictions. The emphasis throughout is upon the complexity as well as the integrity of artisan values. There are no simple formulae here. At times this makes for difficult reading, yet the extensive detail provides many an incidental bonus. We learn, for example, that the LWMA limited members to one contribution per debate, obliging them to remain seated while speaking, thus discouraging declamation — a telling illustration of the urgent but principled world of English artisan politics.

Peter Bailey  
University of Manitoba

Ian Carter, *Farmlife in Northeastern Scotland, 1840-1914. The Poor Man's Country* (Edinburgh: John Donald Publishers Ltd., 1979, distributed in Canada by P.D. Meany Publishers, Port Credit) and M.L. Parry and T.R. Slater, *The Making of the Scottish Countryside* (Montreal: McGill-Queen's University Press 1980)

THESE TWO VOLUMES are as different in method and philosophical underpinnings as two books can be. Yet, they share two points of commonality, in addition to their obvious similar focus on Scottish rural history: each is a volume of history produced outside the confines of traditional academic departments of history, and, each is an excellent specimen of its own genre.

Readers of Ian Carter's fascinating study of farm life in five northeastern Scottish counties are apt to split radically and see it either as a brilliant achievement or a Quixotic failure. I think that he succeeds splendidly, but as I will indicate in a moment others who may disagree are not without grounds for their misgivings. In either case, the book demands reading by social historians and certainly it should be purchased by every serious Canadian library.

Carter embarked upon his study for the best possible reason: he wanted to understand the society in which he lives. As an Englishman, trained in sociology, he found himself teaching at Aberdeen University. In that city, he found the people friendly and, at a superficial level, the culture of the hinterland accessible. Yet, in many ways the culture was both impenetrable to an outsider and, seemingly, contradictory. In particular, the northeast of Scotland evinces a mixture of strong parochialism and socio-political radicalism. To come to terms with the present-day culture of the region, Carter turned to local agrarian-social history. This he contends, explains not only the configurations of the rural culture of northeast Scotland, but also that of the urban

sphere of Aberdeen.

Being based on a real (and fundamentally non-academic) curiosity about his friends and neighbours, Carter's book has great virtues. Not least, it is genial and humane in presentation. Carter writes very well indeed: he must be the only English-trained sociologist in existence who is able to use the word "I" in a monograph without sounding embarrassed. And, moreover, his theoretical arguments are sharp-edged and clearly presented. Because he is interested in understanding the social and economic life of real people, his methodology and his theoretical framework are crisp and efficient. His viewpoint is clearly and unambiguously Marxist, but, unlike so many similar exercises, he does not waste his time in theoretical vapouring — theoretical concepts are either used for direct explication of data, or he ignores them, presumably leaving them to be argued about forever by the theoreticians of class and production.

Within British historiography, the agricultural revolution in Scotland largely has been written from the imperial viewpoint of Edinburgh. In particular, the Lothian pattern has been projected upon the entire lowlands. This pattern implied a swift and revolutionary polarization of agrarian life into capitalist farmers and landless farm workers and this revolution was complete by the mid-nineteenth century. Carter's book, amongst its other achievements, rescues the lowlands from the hegemony of the Lothians. He shows that in the Aberdeen region the revolution took longer and was much more complex than previously was believed. He suggests that an alternative title for his book might be "The Breaking of a Scottish Peasantry." That would be accurate, but only if one added the subtitle: "and why it took so long."

Carter's answer to the question of why it took so long to proletarianize the northeastern peasantry is theoretically sophisticated, but here one can only note its two signal characteristics. First, instead of the

usual capitalist farmer-peasant dichotomy, he analyses the empirical data to show a three-class structure in the beginning of his period: pre-capitalist peasant, medium sized farmers who were market oriented, and the "muckle farmers," that is, the large and growing capitalist agriculturalist. It is in the triadic interrelationship of these three groups that the explanation for the regional social structure is found. Second, Carter argues convincingly, that over quite a long period of time, it was not in the interest of the large capitalist to destroy the intermediate and lower social fractions. In fact in the local context, social accommodation, rather than social friction, was the most efficient way of organizing production from the viewpoint of "metropolitan capitalism."

To a considerable degree, Carter's book is compelling because he does not try to force his data into his framework. He pays admirable attention to the idiosyncrasies of his region, both to the geology and climatic characteristics of the region, and to the peculiar nature of the local market: Aberdeen cattle being a highly specialized and very successful product which were marketed at a premium throughout the British Isles. And, further, when he turns to cultural matters, he is highly insightful. In particular, he provides a convincing explanation of famous and often misinterpreted "bothy ballads."

I find Carter's argument convincing. However, other readers may have misgivings and these probably will centre on two points, the one evidentiary, the other conceptual. Although Carter has assiduously corralled a wide and largely-fugitive literature on his subject, as well as printed census data, it is true that he has not done nearly the amount of primary research that a professional historian approaching the same topic would have done. In particular, there is scanty use of estate papers and no use of manuscript census material. Perhaps the manuscript census sheets for the area did not survive. If they did, however, it would have been very useful for him to

have tested his theoretical argument by an in-depth study of a single parish, farm by farm, labourer by labourer. I do not find this failing crippling, but others may.

On a conceptual level other readers may feel that his discussion of culture of the peasantry, middle farmers, and large capitalist farmers is weak. Each of these groups had a cosmology that requires delineation in terms not derived from social classification. There is a good book in the making here, and one hopes that Dr. Carter having done the ground work, now will go on to the much more arduous — but infinitely more rewarding — task of defining and analyzing the popular culture of the rural Scottish northeast.

The contrast between Carter's book and the Parry and Slater volume is sharp. Fundamentally, it is the contrast between the humanist and the scientist. Whereas Carter is interested in geological and topographical matters as they impinge upon the condition of the rural people in northeast Scotland, the contributors to Parry and Slater's book are interested in human beings only to the extent that they affected the physical landscape. This is not to say that they slight such human social configurations as field systems and agricultural settlements — far from it — but always the human factors are treated as variables that produce the final effect: a specific rural landscape at a specific period in time. To many humanists and social scientists this approach may seem repugnant, but, in my view, it is no more objectionable than the traditional fields of topography and descriptive geology which certainly have their value.

Although there is no explicit theory of rural social structure in the Parry and Slater volume, most of the writers seem to take as given that, from earliest recorded times, large landowners predominated throughout most of Scotland. Thus, implicitly they accept the formulation of Loretta Timberley: "if the basic continuity and slowness of change in the pattern of landholding is considered, along with the growing impor-

tance of non-economic motives for the possession and acquisition of land, one is almost justified in regarding this pattern of landholding as a social system within which economic change operated." (151) Nothing could be further removed from Carter's view, that economic change was determinative of social change. One would love to witness a direct debate between the proponents of the highly scientific history of the Scottish agrarian scene and their opposite number, the less objective, more humane Marxist.

Should Canadian historians pay any attention to these two books? They should read the Parry and Slater volume if they wish to become familiar with a rapidly-developing school of agrarian history. As for Carter's book, Canadian historians should read it only if they wish to understand the behaviour in Canada of the Scottish immigrants and their descendants.

D.H. Akenson  
Queen's University

Roger Elbourne, *Music and Tradition in Early Industrial Lancashire 1780-1840* (London: Brewer/Rowman & Littlefield 1980).

THIS IS A NEW ANGLE upon the mills and factory towns of eighteenth and nineteenth-century England. As usual we are asked to concern ourselves with the pace and process of change in the course of the original industrial revolution, but in this case it is not change in technology or economic standard of living which is argued, but that of change in the self-expression of the people through their folk-songs, their use and appreciation of music, and their communal participation in its enjoyment.

Although the author is good enough to tell us that we, as historians, may ignore his learned chapter on the definition and nature of folk song in the abstract, it does in fact contain a concept central to the subject matter he explores: namely, that

the essence of folklore creation lies in what he calls "a social misalignment that must be confronted and rectified. . . . Folklore items are useful to performers who want to bring a subject to the attention of the group." Industrialization created such misalignment on a gigantic scale and it is Roger Elbourne's contention that it gave rise to industrial folk songs which can legitimately be so called, although they differ significantly from the songs of the rural culture to which they refer back in style and tune.

Beginning with a careful, though lightly-sketched, portrait of preindustrial Lancashire in the eighteenth century, Elbourne shows us an area where cotton cloth was produced on the putting-out system by weaving villages, communities where the population was still part-agricultural, though already dominated by a centralized commercial capitalism. Already they were an important active ingredient in the nation's economy, as their population and occupation-concentration thickened with the boom in handloom weaving between 1780 and 1826. He shows us a musical tradition very much alive in these villages, in the form of dialect songs and stories presenting local incident in traditional forms, in the fiddle-playing, clog and country dances of the public house, and through participation in the harvest-homes and haymakings of the countryside. Along with these he reveals the existence of a devoted interest in the performance and propagation of classical music on the part of groups of weaver-musicians who copied the works of Handel and Haydn, walked miles to music festivals, created church orchestras and choirs, fashioned and cherished their own string instruments. Methodism had a part in this enjoyment of music (for the weaving, like the mining, villages took rapidly to that faith) but the weaver's trade enabled him to pursue it, for he could control his own hours, working long stints to save leisure time for music (or the public-house).

Weaving families sang at their work to ease the monotony and keep themselves awake. "We chorussed 'Christians awake' when we ourselves were almost gone in sleep" says one autobiographical account. When steam-power drove a reluctant population into the towns this musical tradition went with them.

There it was transformed again; driven from the workplace by supervision and strict rules it survived in the public-house, where the semiurban culture saw the beginnings of the music-hall, and also in the Mechanics Institutes and churches where serious music became an ingredient of temperance and respectability. Meanwhile the industrial ballad took literary form through the medium of the broadside sheet and the ballad-pedlar. The broadside sheet was an old urban form, but as the wages of the weavers sank and life became more grim it could be used to confront the issues through the dialect song. Thus, the author argues, emerged a hybrid musical form, created in many cases by singers of the people, but diffused by commercial means and for commercial profit. He gives us a number of examples of this form in an enjoyable appendix.

In general the culture of the people did not suffer so rough a transformation as their working-conditions and as long as the towns remained of a manageable size the inhabitants could renew their contacts with the traditional culture through family visits and attendance at country fairs.

Closely related to ongoing work by scholars like Vicinus and Stedman-Jones on the creation and survival of working-class culture, this vignette adds a dimension by its perception of an evolution of musical tradition in one particular segment of that class and links it to the nature of their work and the special manner of their experience of industrialization.

Mavis Waters  
York University

Jane Lewis, *The Politics of Motherhood: Child and Maternal Welfare in England, 1900-1939* (London: Croom-Helm 1980).

FOR SOME REASON Jane Lewis' publishers decided that the public should not be informed that her book first saw the light of day as a 1979 University of Western Ontario Ph.D. dissertation. Such an omission is a pity because Lewis' work is a fine example of the important contribution North American scholars are making to English social history. The chief interest of the study, however, lies in the fact that it touches on so many questions in the history of social policy that are being broached by researchers on both sides of the Atlantic.

Lewis focused on the provision of child and maternal welfare, which as a result of the investigations precipitated by the Boer War, became the subject of furious debate in the first decade of the twentieth century. Governments were spurred on to take some action as it was pointed out that sickly children provided the pool from which ultimately the defenders of the Empire would be drawn. Infant and maternal mortality figures were readily adopted by experts as the criterion of the nation's health, but what no government could accept was responsibility for the poverty which produced such figures. Though school meals and medical inspections were in force before World War I, old-fashioned liberals worried about the dangers of intervening in the family while new-fangled eugenicists often adopted the "better dead" view that even the infant had to be subjected to the struggle for survival. Lewis follows through the 1920s and 1930s these representatives of the *derrière-garde* and the three groups most concerned by social intervention — the social and medical science "experts," the various women's organizations, and the actual women who were the subjects of the new policies.

The experts' role can only be fully understood, Lewis argues, by analyzing



their underlying assumptions concerning sex and class. These Medical Officers of Health, economists, doctors, and so on chose to pursue some policies while they ignored others. And whatever the conclusions of the administrators and the expressed desires of the clients there was the inescapable fact that government would never admit that simple poverty was the root cause of most infant and maternal distress. Accordingly the experts had to fall back on supporting temporary or indirect policies that were aimed at "encouraging" individuals to be self-supporting, at parents to be more responsible. Thus infant mortality was attributed primarily to the "ignorance and fecklessness of mothers" who, it was asserted, killed off their babies by poor feeding, filthy care, and overlying. Efficient mothercraft was to be obtained not by the provision of economic aid, but by education via domestic science classes, infant welfare centres, and a welter of pamphlets which contained such gems of information as "Remember that a baby that has had a dummy is like a tiger that has tasted blood."

Even in the more significant responses to illness and poverty it was always apparent that the concerns of policy makers and clients necessarily differed. The Medical Insurance Act of 1911, for example, had from the Treasury point of view the purpose not of securing health but of guaranteeing economy by keeping the sick out of the workhouse. Similarly Beveridge's family allowances were conceded, not as a recognition of the role of the mother, but as a way to stifle social unrest without conceding a general advance in wages. The experts, of course, often quarreled amongst themselves, a point Lewis illustrates by an analysis of the medicalization of childbirth. The medical profession's response to high maternal mortality rates was to call for more medical intervention, but the question was whether or not it would be administered by hospital consultants or general practitioners or midwives.

Hospitalization with the ever-increasing number of inductions and episiotomies eventually won out. The importance of the struggle lies in the fact that what settled the issue were the relative strengths of the rival professional groups, not the expressed desire of the clients.

Protests against this state of affairs were voiced by a second group that Lewis examines, a variety of women's organizations that presented a broader interpretation of the social and economic needs of women and children. The Women's Co-operative League, the Women's Labour League, the National Union of Societies for Equal Citizenship amongst others declared that what ordinary mothers really wanted was economic aid, family allowances, and birth control. Such groups occasionally set up their own clinics and educational outlets. Though Lewis presents their activities in a sympathetic light, their practical impact is difficult to determine and at times they appeared to act as patronizingly as the official experts they themselves castigated.

It is when one turns to the question of the response of the clients to social policies and the criticisms made of them by the women's groups that one feels the most need for great clarification. In the introduction Lewis states that she will deal with the demands and reactions of clients but in the main, given the paucity of information, she falls back on arguing from inference concerning attitudes to experts, hospitalization, etc. Her arguments are usually sound. For example, as regards hospitalization she acknowledges that medical professionals by discouraging old neighbourhood networks — in which the new mother relied on midwives and older women — in effect created a "need" for medical intervention which doctors then filled. But at the same time, Lewis also argues that working-class women in their pursuit of the best health care went willingly to hospitals and that it would be foolish to suggest that such

institutions offered no real benefits. It would help if such insights were fleshed out. In both Lewis' preface and in an appendix references are made to 83 women who spoke to the author about their memories of maternity between the wars but in the body of the book there is scarcely a mention of such information.

The fact that *The Politics of Motherhood* is primarily a study of the debate over policy rather than an analysis of the child-bearing experiences of ordinary women will disappoint some; most readers will be more than happy to find in this slim volume a clear, concise account of the relations between health and family economy, the control of health care, and the control of reproduction. Lewis' study of the social and economic context in which social policy originated will be necessary reading for all who interest themselves in both its history and its reform.

Angus McLaren  
University of Victoria

Harold Smith, comp. *The British Labour Movement to 1970: A Bibliography* (London: Mansell 1981).

THIS BIBLIOGRAPHY COVERS books, pamphlets, and periodical articles in English dealing with the history of the British Labour Movement published in 1945-1970. It is planned to produce decennial supplements with that for 1979-1980 scheduled for publication in 1982. Harold Smith is a noted Labour History bibliographer and he has drawn on his years of experience as a collector and librarian to comb bookstores, major libraries, and smaller specialist collections in search of material which is truly ephemeral in nature. The arrangement is by broad subject headings, for example, Early Radicalism, Trade Unionism, etc.

In addition there is a detailed index leading back to the numbered item. After an introductory bibliography of bibliographies, Smith includes 3838 individual items. Labour historians will owe a great debt to the compiler for his efforts in producing this bibliography and so also will all interested in British political history.

Norman Horrocks  
Dalhousie University

Michael R. Weisser, *Crime and Punishment in Early Modern Europe* (Hassocks: Harvester Press 1979).

THE PURPOSE OF THIS study is to "analyze some aspects of the transformation of early modern European society through the perspective of criminality," and "the incidence of crime and the behaviour of criminals as a measure of how early modern society was being transformed." In an attempt to fulfill these objectives the author makes some interesting observations. These include viewing crime as an index of social development as well as social distress; that crime "is the quintessential activity that draws people together." Weisser argues that this activity involves intra-class relations in rural settlements and inter-class disputes in urban areas. Assessing the impact of industrialization on crime, he concludes that what the upper class saw as disorder the lower class saw as fun, and that the criminal process was for the upper class the maintenance of law and order, and for the lower class simply misery. Pleading for historians to compile a geographical catalogue of crime, he acknowledges that criminal records may not be an adequate source to examine its structure or extent.

This work, however, cannot be classified as a sustained account of the subject. The organization into sections on the social environment, legal environment, change, new crime, modern crime, police, and punishment becomes superficial.

Countries appear and disappear at random, as do centuries, and the great catalogue of relevant scholarly literature is scarcely touched, let alone digested. Despite the great strides made in the methodology of the history of crime in the 1970s, and the creation of various models — especially the labelling and conflict theory models, there is no attempt to present any kind of model for the conceptualization of the material let alone its discussion. In the end one has approximately 120 regular book pages covering Europe from 1350-1850 with no ideological perspective, methodological approach, or understanding of the evidence. One does have, however, a large amount of contradiction and much repetition both within chapters and throughout the text.

The work begins with the claim that it is the first attempt to regard crime and punishment as a whole, and that there have been no histories of crime to date. Weisser praises "wholly-researched work" such as Radinowicz, assumes that most of the specialized studies published so far are adequate, and declares that he will provide the synthesis. However, since he excludes political and religious crimes, riots and rebellions, female offences, and other areas, he can conclude that most crime was professional, writing the history of deviance out of the literature on the subject. Thus he concludes that crime has a simple, logical development of its own, and can be found and made known without recourse to the original records.

Any attempt to summarize research on the history of crime and punishment, to provide a useful methodological approach, and to suggest challenging hypotheses would be welcome. But this book is a failure. One hopes that individuals in related fields will not be seduced into accepting the study as a genuine representation of the historian's craft.

Louis A. Knafla  
University of Calgary

Christopher Kent, *Brains and Numbers: Elitism, Comitism, and Democracy in Mid-Victorian England* (Toronto: University of Toronto Press 1978), and Robert Mandrou, *From Humanism to Science, 1480-1700* (Sussex: Harvester Press 1979).

THESE TWO books make strange bedfellows; one being a detailed monograph on the origins, career, and significance of the ideas and attitudes of an influential group of mid-Victorian thinkers; the other (the third volume in the Pelican History of European Thought) being a broad and eloquent overview and synthesis of the "ceaselessly refreshed" intellectual life and tensions from the Renaissance to the eighteenth century. What unites both books for consideration here is their common concern with that aspect of history which, if absent, ultimately makes all other aspects not only unintelligible but vacuous: namely, the concern with ideas (and those who spread them) in relation to material, social, political, and cultural life and change. Students of labour history least of all can afford to ignore this, for to do so is to fall (from the opposite side) into the same naïve internalism that once led historians of ideas completely to divorce matters of the head from the real world of the hand. That said, however, neither of these books goes far toward advancing a constitutive understanding of mental and manual labour.

Of the two works Mandrou's is by far the more readable, much of this readability being attributable to the excellence of the translation by Brian Pearce. But it is more than prose style that recommends Mandrou's book: he has an authoritative command over a wide range of diverse primary and secondary sources and has a majestic scope that is truly European. To

those nursed on standard European histories of this period Mandrou's forays into Poland, Hungary, Spain, Sweden, and elsewhere will be welcomed.

Mandrou's approach is also satisfying to the extent that a correction of emphasis can be satisfying. Butting primarily against Paul Hazard's influential *The European Mind*, Mandrou cautions against drawing sharp contrasts between "old" and "new" intellectuals in any period, and against imposing arbitrary labels upon periods and so construing all the thought within them in terms of discontinuities with the past. He perceives, rather, that ideas and the dynamic cultural and social hegemonies they serve or undermine must be treated with an extended contextual finesse: not in terms of period pieces or simple linear progressions, but in terms of the endless movement of contextual totalities. Innovating ideas are to be seen as the products of specific (though deeply-rooted) social and institutional contexts which, over time, nudge or jar those contexts and, as they do so, are in turn themselves transformed, thus allowing for still more innovations, adjustments, reactions, absorptions, and so on. It is an approach that makes for a fairly messy history, yet Mandrou carries it off without needless simplicity or confusion and without always trying to discover and convince us of a single ideological or political "line" in the thought of this or that intellectual or intellectual group. Mandrou is fully aware of the manifest and latent contradictions and ambiguities in every social and intellectual environment.

It is of course almost inevitable that in a book as geographically and chronologically expansive as this that the ire of specialists in the history of science, religion, politics, witchcraft, literature, and art will be raised over detailed points of fact and interpretation; that the author will, on the one hand, be damned for what he has done and, on the other, for what he has not. But

these would be minor carpings and largely misdirected at a book aiming to integrate the more salient and accepted features of modern research as they apply to the history of thought over this crucial 220-year period in which the modern capitalist and scientific mindscape of the west can be said to have been incubated. Yet for this very reason of integrating historical advances, it is legitimate to criticise Mandrou's book as seriously flawed by its total neglect of economic realities. It is not only that Mandrou, with such a magnificent epochal panorama before him, has neglected to consider in any way the notion expressed (rhetorically) by Marx and Engels in the *Communist Manifesto*: "What else does the history of ideas prove, than that intellectual production changes its character in proportion as material production is changed?" it is also the case that by eschewing consideration of economic changes between 1480 and 1700 he has failed to fulfil even the modest promise of his introduction, "to analyse and reconstruct the complex relations which linked intellectuals with different social milieux." (13) Aware that the diffusion of ideas is not to be understood without reference to social relations, Mandrou seems to think it possible to understand social relations and the ideas connected with them without ever heeding economic structures and changes in modes of production. Like so many liberal historians, Mandrou, "the contextualist," rests satisfied with a mere parting of company from the old internalist tradition in the history of ideas wherein ideas were treated autonomously, as if they had descended from heaven.

The neglect of economic realities is contingent with Mandrou's failure fully to conceive of ideas as social products. For all his concern with the social, political, religious, and cultural contexts in which ideas were developed and deployed, he still thinks of ideas as things functioning

in an essentially mechanical interactionist way, rather than as mutually constitutive with material reality, that is he does not perceive the ideational and the material as interpenetrated by the dialectics of change and growth. Mandrou lacks any conception of "mediation" or of the subtle and complex ways in which ideas — in being generated by real people under real socioeconomic conditions — reproduce the ideological and social interests of that real world in alternative (often highly abstract) intellectual forms. Forsaking, or perhaps being ignorant of, the now considerable body of literature that reveals scientific knowledge (including mathematics) as socially constituted, Mandrou cannot hope to relate material and intellectual realms without ending up in the mire of economic determinism. Wisely choosing to avoid that fate, he avoids economics altogether and thus ventures not the slightest speculation on the relation between changes in thought in this period and changes in socioeconomic structures and relations.

This is a shame not only because Mandrou obviously has the fine ability to handle complex matters sensitively, but also because the price paid is an overwhelming shallowness. A thousand times one cries "why?" to observations like: Mersenne and his friends in early seventeenth-century France granted primacy to mathematics and mathematicized all natural phenomena (208); or one feels cheated by remarks seemingly pregnant with contextual significance but left untouched, such as: the printing establishments set up in the fifteenth century "were like a production line, in the modern sense of the expression . . . employ[ing] detail workers at each stage of the manufacturing process" (29); or one tosses up the book in disgust after an unelaborated observation such as: "Scholarship was given a fresh impetus in the early seventeenth century through the coming of new participants" (183). By the latter, Mandrou makes his point about the "ceaselessly refreshed"

nature of intellectual life, but he travels no distance at all towards providing any understanding of how and why and from what the "impetus" arose or what deeper social significance it had. It is also from passages like this that one sees clearly the weakness of the continuity-tending interpretation of intellectual history: it too readily allows the historian to avoid altogether any careful explanation for change: Mandrou too often surfs over the historically relevant problem of change in a smooth wave of mere narration.

The least that can be said of *From Humanism to Science* is that its title is wholly belied by its vigilantly anti-Whig approach. The title of Christopher Kent's book, however, simply belies. *Brains and Numbers* has nothing at all to say about the masses, while even the theoretical and actual alliance between brains and numbers takes up less than 50 pages. Kent's main interests are with describing the intellectual milieu out of which emerged the disciples of Auguste Comte's *Système de politique positive* (1851-54), and with examining both the élitist nature of their radicalism and its practical social and political manifestations and ambiguities.

Kent's book is divided into three sections. The first relates how the Cole-ridgian "ideal of clerisy" (the notion of an intellectual élite serving as a cultural priesthood for an organic society of class consensus) strongly appealed to certain members of the generation at Oxford in the 1840s and 1850s who, in that placid "end of ideology" atmosphere, were conscious of their social and political irrelevance. Kent then goes on to discuss the Comtist Movement in England during the 1850s and 1860s as a systematic and explicit attempt to organize and extend the authority of the university élite. Within that context he endeavours to dispell the various myths about the appeal of Comtism in mid-Victorian England. Comtism was, he indicates in a prefatory passage that adequately sums up his concerns and his findings:

not the exotic aberration of a coterie of eccentrics but rather a comprehensive ideology remarkably harmonious with certain established English currents of thought and remarkably well suited to the needs of a middle-class intellectual élite. It not only legitimized their pretensions to intellectual authority but also provided a powerful ideological solvent for social irritants to those who were ready to entertain fairly radical notions of social change for essentially conservative ends — a sort of prospective conservatism rather than the more common retrospective variety. Comtism met the anxieties of classically-educated, amateur-oriented intellectuals by providing a scientifically sanctioned blueprint of the future which assured their continued relevance despite the increasing industrialization, specialization, and democratization of society. (xiii)

The third part of Kent's study is directed to the relation between intellectuals and politics, a relation that became problematic to Comtists after the defeat of the intellectuals at the polls in 1868. Here Kent pays particular attention to John Morley and Frederic Harrison and treats us to an extended exegesis of their most considered wrestlings on the problem in, respectively, *On Compromise and Order and Progress*. Through this Kent illustrates two varieties of engagement with and responses to the Comtean elitist substitute for democracy.

The book lacks imagination and originality. Virtually everything Kent says on the English Positivists has been said before, including the cheap, if relevant, comparisons with Marxists (and Positivism with Marxism), the secular religiosity of these intellectuals, the ambivalent middle ground they occupied between Utilitarianism and Fabianism, their special, if not always certain, regard for trade unionists, their sense of guilt at being bourgeois, their prolific output relative to their small numbers, and so on. Royden Harrison, besides treating the Positivists' involvement with British labour, discussed all of these matters succinctly in a

chapter in his *Before the Socialists* (1965). At much greater length, Kent has spoken on less, and it may even be that in concentrating so heavily on Frederic Harrison, Kent has slightly distorted historical understanding, for Harrison was, of all the English Positivists, the most self-consciously elitist. It's also slightly disappointing that in warming over the relatively familiar, Kent stylistically reveals a firm attachment to the drab historical orthodoxy that the ultimate reality to which the historian can aspire is party politics. But what is more worrying is the insubstantiality of Kent's contribution to serious historical concern over the nature of the transmission of ideas from intellectuals to masses and vice versa, and over the role of intellectuals in relation to the political necessities of production at any given historical moment. The mid-Victorian Oxford intellectuals would seem to offer a convenient point of entry to both these concerns. Yet Kent never takes them up — never refers to such important writings on the function of intellectuals as Gramsci's (as does Mandrou), nor does he ever seek to bring into his discussion any other relevant critical sociological writing. Indeed, it almost seems that in writing on a group of intellectuals who were preoccupied with the problem of the intellectual's place and role in society, Kent is afraid to lift his sights from his file cards lest it lead to any self-questioning of his own role as an academic. Nose to the exegetical grindstone, Dr. Kent never breaks historical rank, not even for so much as a wisp of an allusion to, say, presentday Oxford-connected radicals also seeking relevance and comprehensive understanding through the structures of Parisian intellectualisms.

*Brains and Numbers*, then, should be especially recommended to devotees of history for history's sake or to anyone else seeking to make a match between mind and atrophied facial muscles. By contrast,

it is easier to recommend *From Humanism to Science*.

Roger Cooter  
Dalhousie University

Robert Darnton, *The Business of Enlightenment, A Publishing History of the Encyclopédie 1775-1800* (Cambridge: Harvard University Press 1979).

EMMANUEL LEROY LADURIE at one point suggested that advances in the historical sciences were obtained either by the discovery of new sources or by the innovative use of standard archival material. In this, justifiably, much acclaimed work Robert Darnton has demonstrated that these are not mutually exclusive approaches. Basing his study upon the unique and formerly unused records of the Société typographique de Neuchâtel while attempting, in his own words, to mix "British empiricism with the French concern for broad-gauged social history," Darnton succeeds in creating an "original blend of the history of books." (3) And as "arcane" (1) as it may appear, this is a history that will appeal not only to bibliophiles. Indeed, Darnton's claim that it constitutes a contribution to the history of ideas, of business, of labour, and to the historiography of the French Revolution is, with some qualifications, borne out.

The scope of the study is a reflection of the subject matter itself and of the richness of the source material. The commercial success of Diderot's first folio edition of the *Encyclopédie* had established this work as the "*affaire du siècle*" and incited publishers to conceive plans for reprints and new revised editions. Published at over 8000 copies between 1777 and 1780, the "Geneva-Neuchâtel" quarto edition is the most successful example of this effort to capitalize upon the popularity of the *Encyclopédie*, and since it also constitutes both the largest single run and approximately 60 per cent of all of Diderot's *Encyclopédie* sold in France, it may also be considered its most

representative version. Thanks to letters and papers of the STN, one of the most important eighteenth-century printers in Europe and an associate in the quarto venture, Darnton is able to follow in almost intimate detail the history of this edition, from the creation in 1776 of the first association between the powerful Parisian publisher, Panckoucke, detainer of the works "privilege," and the STN, to the final acrimonious settlement of accounts in 1780 involving the two original associates and a third partner, Duplain, a Lyonnaise "pirate" publisher. The unfolding of events, involving intrigue and duplicity, makes for fascinating reading.

If this diplomatic history of entrepreneurship forms the thread of Darnton's study, analysis of the technical aspects and of the intellectual and political significance of the publication of the *Encyclopédie* constitutes its substance. Ledgers and correspondence of the STN to paper manufacturers, foundry makers, and ink suppliers enable Darnton to elucidate the relative costs of materials and to examine the inflationary pressures arising from the publication of a work that for over two years tied up roughly 200 presses from Lyons to Neuchâtel.

These pressures also affected labour costs. Confronted with the *Encyclopédie* boom, labour and capital adopted different strategies, the one to take advantage of the situation, the other to limit its consequences. Neither was passive, and if eventually employers gained the upper hand by coming to an understanding among themselves over wage scales, the printers did maintain their mobility, leaving shops for better conditions if not better wages and continued to exert some control over their working environment, determining work rhythm and developing techniques for easing the work load at the expense of quality. One might argue that much, especially the cabal of employers, was already known to Adam Smith. But the STN archives, with their memoranda, description of workers, and pay slips con-

fer to these moments in labour history an immediacy rarely found elsewhere.

Finally, the records of the STN provide insight into the geographic and social diffusion of the *Encyclopédie* and thus of the Enlightenment. Far from being the ideological expression of the bourgeoisie, the *Encyclopédie* reached a mixed elite drawn from the three orders of the Ancien Régime. Darnton's findings would thus seem to substantiate the claims of Cobban concerning the conservative nature of the social configuration of the Revolutionary class.

However, it remains to establish the link between the *Encyclopédie* and the Revolution. Panckoucke's *Encyclopédie méthodique* 1780-1834, conceived as a perfected remake of Diderot's work provides means of tackling the problem. After a review of the conduct of the Encyclopedia's contributors during the Revolution, Darnton concludes that there is no necessary link between the two. In short, he has undermined the significance of his earlier findings, especially since he makes no apparent effort to tie them together. The study of the *Encyclopédie* is a contribution to the fields that it concerns but because these are so dispersed the conclusions end up being rather weak.

This raises another problem with Darnton's study: it is too wide ranging. In particular, the reader may question the advisability of pursuing the study past the dissolution of the quarto association through to the publication of the *Encyclopédie* whose connection to Diderot's work remains problematic.

Nonetheless, this procedure does enable the author to consider the transformation and "modernization" of the printing trade brought about by the Revolution. But this brings us to the major difficulty with Darnton's study, which centres on the question of changes in business practice and Darnton's use of terms such as archaic and modern. Put simply, Darnton subscribes to a technological interpretation of historical change, one in

which present American practice constitutes the norm for modernity. Thus Panckoucke's proposal for reorganizing the trade along corporate welfare lines is rejected as an example of archaic thinking. One may regret that not only the Swiss but also the Japanese remain unaware that they are following backward practices. Similarly Darnton's concept of modern business seems to remain on the one hand, too courteous and civil and, on the other, too impersonal. To believe, as the author suggests, that socializing and the dining that characterized business in the Ancien Régime (84), and the necessity of patronage from highly placed state officials were things of the past is to show a surprising ignorance of both modern business practices and current business history. To take but two examples, Dieter Buse has discussed the importance of salons as a place for social and business contacts over dinner between state and business administrators in Weimar Germany, while Don Davis has been working on the role of sponsorship in the rise of individuals to the elite. Finally, to believe that the cupidity characteristic of a Duplain no longer motivates modern business men is simply naive.

Professor Darnton promises us further studies based upon the records of the STN. The shortcomings pointed out above are minor in comparison to the contributions that he has made in this book, and thus, one can look forward to these subsequent studies with pleasure. If Harvard continues to publish them, it is to be hoped that they will be more conscientious in the proof reading of the French quotations.

Peter Simoni  
Laurentian University

Michael P. Hanagan, *The Logic of Solidarity: Artisans and Industrial Workers in Three French Towns, 1871-1914* (Urbana: University of Illinois Press 1980).

TO LEARN MORE about the reasons for



French workers' protest in the late nineteenth and early twentieth centuries. Michael P. Hanagan studied three towns with different economic bases in the strike-prone *arrondissement* of Saint-Etienne. By contrasting the factors facilitating militance in the predominantly artisanal town of Le Chambon-Feugerolles and the mixed artisanal-industrial town of Rive-de-Gier, to the factors inhibiting activism among the industrial workers of Saint-Chamond, Hanagan makes a compelling case for the role of late, slow industrialization, and the co-operation of artisans and industrial workers, in promoting strike actions. More specifically, his selection and comparison of towns enhances our understanding of how artisanal workers remained in the factories, indeed retained control over apprenticeship programmes and job hierarchies; how new technologies and increased employer, engineer, and foreman supervision gradually eroded workers' autonomy and hence their ability to influence peacefully the work environment, and how artisanal workers reacted defensively by unionizing, demanding artisanal wage rates for machine work, and winning the support of industrial workers on the wage issue.

In the first three chapters of *The Logic of Solidarity*, where Hanagan outlines his explanations of mass mobilization for strikes, he highlights socio-economic changes but also indicates that the political liberalization of the Third Republic encouraged artisans to rally industrial workers in order to dominate local elections. The next three chapters, which treat the three towns successively, give specific, often quantitative information about the socio-economic transformations, but rather imprecise, qualitative information about local, departmental, and national policies. On the one hand, Hanagan offers interesting vignettes of the personal and often paternalistic nature of local and regional politics, as, for instance, in his description of Georges Claudenon, an important, benevolent employer who was mayor of Le Chambon-Feugerolles from

1898 to 1919 and deputy from the Loire from 1898 to 1906. On the other hand, he provides no breakdowns of local or departmental voting patterns. Historians hoping to learn more about the character of local radical/socialist politics will have to await Joan Scott's book on these subjects.

Similarly, Hanagan rightly stresses the limited impact of revolutionary syndicalism but only suggests that workers may have ignored socialist ideologies because the ideologies ignored local issues. When he does discuss national policies like protective labour legislation, he is brief and, about laws restricting women's work, misleading. Historians of the national labour movement, particularly its political manifestations, may wish to confine their reading to the first three chapters of *The Logic of Solidarity*, where the analytical framework appears. Historians interested in international comparisons of labour movements should also consult the important section on "Union Organization" buried in the chapter on Le Chambon-Feugerolles. Here Hanagan contrasts the French industrial unions to English craft unions and accounts for French artisans' acceptance of industrial unions by late industrialization where there was no heritage of craft factionalism or competing hierarchies of conservative, paid union officials. Here too he tentatively challenges the accepted argument that French unions were poorer than their English counterparts. His idea that local French unions had more money to devote to strikes (as opposed to benefits) ought to be explored further in other contexts.

The three town studies systematically examine work and wage structures, residential patterns, and leisure activities to see how these variables influenced militance. In the process, the reader gets a vivid picture of glassmaking, filmmaking, metalworking, and braidweaving, of the physical layout of the shops, of the interactions between workmates on and off the job, as well as the neighbourhoods and cafés where artisans socialized and "politicked." Hanagan shows that the co-

operative work teams and high family wages, plus the residential clustering and cafés, of the (artisanal) glassworkers of Rive-de-Gier and the (artisanal) filemakers of Le Chambon-Feugerolles explain their cohesion and activism. Although these ideas are not new — in fact, the author might have referred to studies of earlier, militant artisanal groups — they have rarely been so thoroughly elaborated or integrated into a multifaceted theory. More significantly, Hanagan includes counter examples in the form of the industrial metalworkers and the town of Saint Chamand. There the female braidmakers remained quiescent because they were isolated on and off the job owing to being scattered throughout the factories and living all over town or in heavily supervised dormitories, because they had low wages, little commitment to their work, which they perceived as a short phase of their lives, and few opportunities to organize, thanks to familial responsibilities at home and the "unrespectable" character of the cafés/union meeting places.

Hanagan's hypothesis about why artisans united with industrial workers are not as consistent or compelling. In both of the towns where artisans supported industrial workers' strikes, the artisans were motivated by alarm at the industrial employers' anti-unionism. In Rive-de-Gier, the glassmakers also approved of the metalworkers' demand for shop floor representation and saw the metalworkers as a crucial constituency in their struggle with anti-union local officials. But in Le Chambon-Feugerolles, the filemakers were activated by threatening technological change and familial ties to the striking boltmakers. More case studies of this type may clarify whether combatting anti-unionism was the key reason for artisanal unions' solidarity with striking industrial workers.

If Hanagan has not proved all of his hypotheses, he has introduced an important thesis: craft interests were not necessarily different from class interests. Furthermore, *The Logic of Solidarity* points

out an area needing research — local labour history — and provides a comparative model for others to follow.

M.L. McDougall  
Simon Fraser University

Paul-Henry Chombard de Lauwe, *La Vie quotidienne des familles ouvrières* (Paris: Editions du Centre National de la Recherche Scientifique, 3<sup>e</sup> edition 1977).

THE DAILY LIFE of the working-class family has been a subject of interest among sociologists for some time. In France, Chombard de Lauwe's *La Vie quotidienne des familles ouvrières* was first published in 1956. Since then, the book has reappeared in 1959 and 1977 with little attention from labour historians. Yet this study of Parisian worker families during the quiet years of 1950-52, deserves reconsideration for its sophisticated analysis and unconventional insights.

Chombard de Lauwe asserts that the social life of worker families revolves about two major spheres of activities: place of employment and place of residence. At work, men labour long hours at tiring tasks for mediocre pay. Constant insecurity, coupled with an exhaustive routine of commuting to the jobsite, induce occupational dissatisfaction and deprive workers of valuable time about the hearth. At home, women are burdened with daily chores that never end, preventing more efficient planning of the workday and curtailing projects of recreation. Each partner follows a timetable that prohibits activities in common. And the housing shortage forces the family to live in cramped quarters to the detriment of the children's education and sanity. What differentiates the situation of the proletariat from that of the bourgeoisie is not simply that work and home experiences differ, but that the labouring population lacks leisure — what the author calls an intermediate milieu between the poles of workplace and abode. Consequently, the worker family's social life is predominantly centred on the neighbourhood district. This is the global portrait that Chom-

bard de Lauwe presents as a prelude to his analysis of the consumption patterns of Parisian households.

He observes initially that household expenditures vary according to the seasons and the overall state of the economy. Then he remarks that those living at a subsistence level are caught in a vicious circle: having suffered deprivation during the workweek, they tend to splurge when payday arrives, as a form of compensation, which throws them back into dire straits again. Unsure of being able to repay his debts, the worker instinctively fears borrowing on credit. The rhythm of household expenditures is further elucidated by a perceptive account of the spatial dimension of market engagements: the immediate area of the neighbourhood is the site of daily shopping (for example, bread); the periphery of the district attracts some weekly buying (hardware items); and at greater distance still, special purchasing forays take place intermittently (furniture). The mode of expenditures and distribution of "economic space" structure workers' perceptions of themselves as a class *vis-à-vis* other socio-professional groups such as the petty bourgeois, who serve as commercial intermediaries in the marketplace.

The author offers a typology of consumption behaviour as well. He notes that *préoccupation* about the necessities of life (food, shelter, and clothing) dominates daily concerns. The emphasis he places on this type of comportment makes the picture of the worker family gloomy indeed; however, he does point out that misery does not breed quiescent pessimism. The household remains remarkably generous, comradely, and resiliently combative in a latent fashion. Thus workers have certain interests that transcend material concerns; and such consumptive behaviour, he names *intérêt-libre*. This is a broad schema of dual-motivations formulated to elucidate workers' "needs," ultimately in order to explain lower class aspirations and attitudes. In the case of family meals, for example, the author observes an open festive reciprocity between husband and wife, host and

guests, that differs sharply from the cold formality of bourgeois family dinners. And in the workers' desire to eat meat more often, he sees the proletariat trying to defend its social position so as to maintain family group-cohesion. In short, the shift from *préoccupation* to *intérêt-libre* is evidently the crucial indicator of working-class aspirations and the avenue of popular cultural development. Chombard de Lauwe does not deny that such aspirations involve desires for prestige and social recognition, but he points out that materialistic individualism is restricted to the upper stratum of the wage-earning population, notably state and commercial employees. Workers generally, he asserts, reject individual upward mobility in favour of a collective social rise (*montée*). Hence, the dual motivational schema is a teleological class interpretation of consumption behaviour that interprets workers' latent resistance to bourgeois domination as a badge of group defiance written large on the most ordinary household concerns.

While Chombard de Lauwe's work constitutes an impressive theoretical statement on the daily life of worker families, it has a number of shortcomings. Form interferes with matter as a maze of footnotes takes the reader on a meandering journey. From chapters I to IX, the author refers us to various parts of the book at least 45 times. In the text, a key factor like *culture* is introduced in diverse senses but it is neither defined comprehensively nor treated directly; rather, discussion of this determinant of consumption behaviour is continuously postponed until it simply falls out of view by the end, where definitions of structure, objective, and subjective social space, take precedence. More significantly, the author takes for granted what deserves to be disproven at the outset. He assumes that place of employment is a less decisive influence on the attitudes and behaviour of worker families, than place of residence. Consideration of the workplace is condensed into two chapters and the brief treatment is inconsistent in certain places. The male worker, for example, is

portrayed as subject to a high degree of occupational dissatisfaction and insecurity, but the point is forgotten when the author later prescribes equal liberty for women in choosing a profession. (49) After all, the author stresses in an accompanying article entitled, "Le Prolétariat a-t-il disparu?" that the state of dependence is the essence of being proletarian. While workplace receives summary attention, habits of food consumption attract intensive analysis, giving the focus of the study a lack of symmetry.

The orientation of Chombard de Lauwe's study is doubtless set by practical considerations. In the introduction to the first edition, he acknowledges that his core data derive from a survey carried out by *Section Nutrition of the Institut National d'Hygiène*. Given the small size of the sample of families, he duly warns against comparing his work with other studies (mainly American) based on samples 10 to 20 times larger. (225, n. 2) Although admitting the limits of his sample, he never specifies the exact degree to which statistically significant differences within the variables isolated are indicative of socially significant characteristics other than to present the whole as tentative. Moreover, other source material is used without sufficient control of the data's composition and the conceptual framework in which the documentation was gathered. For example, the author refers to a variety of national surveys which, in effect, lump together residents of Paris with those of the provinces, such as the Nord and the Loire, including diverse urban centres like St. Etienne, Lyon, and Marseille. Yet conditions at work and at home in these distant locations can differ greatly from the Parisian environment. Shift work, for instance, is a notable predicament of mining and metallurgical workers; and the spread of commercial concentration of transactions involving goods and services is bound to be spatially distributed within neighbourhoods in a fashion distinct from that of the capital. Moreover, when the

author discusses "auto-affiliations et stratifications sociales," his core sample of families was questioned in terms of social layer (*couche sociale*), while larger national surveys had respondents answer in terms of "bourgeois" and "proletarian" categories. (132) Stratification and class viewpoints of society intermingle in Chombard de Lauwe's analysis, and the framework of his core sample and national surveys does not always coincide with the thrust of his arguments. Despite his assertion that research led him to Marx, (14) the result is structuralist, undialectical, and static. Besides citing Pavlov (183) to introduce the importance of food for worker families would make many Marxists salivate in anger.

Although the book has certain limitations, it represents a valuable contribution to labour history by theorizing about neighbourhood life and market behaviour — a field that is all too often overlooked because of ideological preference or just plain fear of the complexity of life outside the place of employment. In a sense, Chombard de Lauwe manifests what American sociology could have been at its best: when in the 1950s, it had become the vogue of students of industrial relations on our continent to proclaim that the working class was institutionalized into quiescence if not extinction, this French sociologist employed the same techniques to observe that hope still transcends misery and that the fight for social justice continues in the daily life of worker families.

Arthur Borghese

University of Lethbridge

Denis Poulot, *Le Sublime ou le travailleur comme il est en 1870, et ce qu'il peut être*. Introduction d'Alain Cottereau (Paris: François Maspero 1980).

WHEN THE SOCIAL PROBLEM — that burning issue of the nineteenth century — attracts the attention of a manufacturer of nuts, bolts, and threading machines, the result is as provocative as it is intriguing.

Denis Poulot's brochure *Le Sublime* appeared prophetically in 1870, on the eve of the Paris Commune; it is now republished in the useful collection of *Actes et Mémoires du Peuple*. The brochure is preceded by a lengthy introduction by Alain Cottureau, who skillfully deciphers Poulot's diatribe against labour in order to reveal workers' collective practices of resistance to class domination. Both *Le Sublime* and the introduction offer the social historian valuable insights into matters of radicalism, "artisanal" deskilling, popular protest, civil hegemony, and the working-class movement in general.

Poulot adopts an enigmatic title on purpose. He coins the term *sublimisme* to designate the "dissolute" ways and insubordination of the labour force during and after working hours. Writing from the point of view of an experienced worker in the metallurgical trade, he perceives *sublimisme* as a unique and contagious disease, the principal obstacle to resolving the social problem. His objectives are to diagnose the "morbid sickness" and to prescribe the required treatment. Initially, he draws up a "pathological" typology of workers, based on some common criteria of the period: absenteeism, indebtedness, lack of familial affection, drunkenness, and political extremism. In addition, he depicts the demoralizing influence of the workshop environment, in which the conventional mode of apprenticeship allows skilled recalcitrant journeymen to stifle emulation among trainees and to convert newcomers to cynical anti-authoritarianism toward bosses. Poulot is anti-Caesaristic and anti-clerical, accusing State and Church of exacerbating the social problem; yet his own moralistic and elitist values shine through in the solutions he presents. To eradicate the negative effects of workshop apprenticeship, he advocates compulsory education at the primary level, to be followed by training in special trade schools. Entering the industrial sector in ever growing numbers,

the graduates from these schools would facilitate mechanization, rendering obsolete those pockets of production that perpetuate artisanal control over professional expertise and circumscribe patronal authority. Moreover, this select contingent would inherit an *esprit de corps* sustained by associational ties enabling them to assume leadership of existing working class organizations, coopting the movement in the process.

In the context of technocratic infiltration, Poulot addresses the questions of strikes and workers' aspirations. The response is a curious combination of individualism and collectivism. Strikes occur because employees (pressed by rising costs of living) demand the highest wages possible, while industrialists (undercut by competition from outmoded enterprises) aim to reduce payrolls inversely. He recommends that both parties form their respective unions and negotiate differences. To overcome impasses, he proposes a system of binding arbitration carried out by a supreme tribunal composed of workers' and bosses' delegates elected from *Conseils des Prud'hommes* — originally introduced by Napoleon I in the 1800s. What of foreign competition nullifying the national tribunal's decisions? Poulot counters that wages could be set across frontiers by a universal syndicat; and he intimates that the model exists already in the International Workmen's Association. Although such references may seem strange today, they aptly illustrate the author's desire to find practical remedies within the institutional framework of the Second Empire. For Poulot, the ultimate satisfaction of workers' aspirations resides in generalizing property ownership, and to this end he advocates the extension of producer organizations which distribute profits on an individual basis in proportion to the labour contribution of each person. He encourages "democratic" participation so as to foster group pressure against the *sublimes*, whose survival is to become

impossible through excessive dues and obligatory membership in mutual aid societies and social insurance schemes, ostensibly established to ensure everyone minimum material security. Poulot's vision is one of harmony between labour and capital summed up in the phrase: "solidarity obligatoire en présence de la nécessité."

Why read this anti-labour pamphlet? Cottureau notes that it is of direct interest to the social historian, for in its minute portrayal of popular derision and contravention of patronal power, it gives insight into workers' collective practices of resistance, seldom accessible in private or public inquiries of the time. He shows that Poulot's combination of criteria for distinguishing "good" from "bad" tradesmen is unorthodox: *sublimes* may indulge in the usual vices, but they are also highly skilled personnel, capable of self-restraint, leadership, intellectual discourse, and concerted action. Cottureau presents biographical information that elucidates the manufacturer's predilection for technical training and mechanization as "moralizing forces." He offers admonitions for the reader too, remarking that historians unwittingly accept simplistic stereotypes concerning industrialization as something confined exclusively to factory production while supposing Paris is inhabited mainly by independent artisans looking "backward" to a corporate ideal suited to the Middle Ages. He asserts that the invisible web of capitalism embraced the capital and countryside alike, making the situation of nineteenth-century artisans extremely complex and heterogeneous. Cottureau also warns against artificially isolating "vie populaire" from everyday resistance to the dominant social order; he dissuades the researcher from using schematic categories like spontaneity or structuredness; and he maintains that there is a fundamental continuity between social and political comportment. Thus *Le Sublime* is a detailed revelation of the pervasive set of capitalist

dependencies that have transformed the Parisian artisanat into an adjunct of the monopolistic national market; at the same time, it constitutes a handbook of upper class strategy bent on shattering the very continuity it exposes as a prelude to thorough domination of the working population.

Cottureau's introduction is coherent and convincing, but for reasons of economy it does not completely develop certain threads of the argument. While the commentator elaborates upon some institutions like producer co-operatives, he omits reference to others like *Conseils des Prud'hommes* to which Poulot assigns a crucial role. Cottureau's sensitivity to the complex situation of artisans is positively enlightening, as when he traces certain links between resistance in the workshop and familial life styles (which Poulot assails with "bourgeois" bias); however, his Marxist labour-primacy approach inherently flows in a bi-polar direction. A complementary Weberian focus on "market situations" as conducive to manifold class antagonisms might have specified the mediating position of the lower middle class more clearly. In fact, Poulot mentions numerous relationships between "good" and "bad" workers on the one hand and wine merchants and small proprietors on the other — raising the question: what is the net effect of these relationships on the changing orientation of workers' techniques of resistance and their language of protest? Similarly, the ambiguous — indeed, contradictory — political position that Poulot adopts, warrants clarification in terms of upper class hegemonic ideology — a Gramscian subject. Just how dominant was the dominant social order in the final years of the Second Empire? Despite Poulot's managerial élitism and boundless faith in technological innovation, in political affairs he falls captive to the so-called *rez-de-chaussée* (petty bourgeois of the traditional and modern type, occupying ground-floor apartments): "C'est lui qui assure l'avenir

de la République et la tranquillité. Il n'y a rien de possible sans lui. . . . Donnez satisfaction au *rez-de-chaussée*, c'est montrer que l'on sait gouverner." (112) Ironically, this is the class that embodies anti-monopolistic prejudices essentially inimical to Poulotian capitalist technocracy. Finally, what about the countryside? It was in Lyon of the 1830s that silk-weaving artisans first raised the battlecry. "Vivre en travaillant ou mourir en combattant!", and it was in Mulhouse, Alsace, in 1870 that textile and metal workers engaged in the most massive strike of the century. Cottureau concentrates on Paris and thus avoids this terrain. Nevertheless, his introduction and Poulot's *Le Sublime* offer the social historian fresh avenues for research and synthesis.

Arthur Borghese  
University of Lethbridge

Alex Hall, *Scandal, Sensation and Social Democracy. The SPD Press and Wilhelmine Germany 1890-1914* (New York: Cambridge University Press 1977); Max H. Kele, *Nazis and Workers. National Socialist Appeals to German Labor, 1919-1933* (Chapel Hill: University of North Carolina Press 1972); Leila J. Rupp, *Mobilizing Women for War. German and American Propaganda, 1939-1945* (Princeton: Princeton University Press 1978); and Barrington Moore, Jr., *Injustice. The Social Bases of Obedience and Revolt* (White Plains, N.Y.: M.E. Sharpe Inc. 1978).

THE HISTORIOGRAPHY of the German workers' movement is in an uncertain, transitional state, and the four works under review reflect the problems facing German labour historians. Three of these works fall within a traditional type of labour historiography that deals with ideology and propaganda. Barrington Moore's contribution, which is one of the few recent attempts to base a theoretical analysis of lower-class unrest on the social history of workers, is written by a

non-historian who relies upon secondary literature and who has himself done no primary research. Despite the increasing interest in social history, these works are not atypical of the way the history of the German working class is still being written — in particular by overemphasizing ideology rather than social practice and by separating theoretical hypothesis from primary research.

Alex Hall has exploited the rich resources of an extensive national and regional social-democratic press to investigate the interrelationship of political agitation, legal repression, and the uses of propaganda in the party press of the SPD in Wilhelmine Germany. Hall analyzes the treatment of political scandals in the SPD press in three contexts: judicial harassment of the SPD and legal restrictions on freedom of speech and of the press; opposition to constituted authority (the police, municipal authorities, and the German military); and political and social scandals affecting the capitalist system and monarchical regime. The major weakness of Hall's study is that it lacks an overall focus. There is no real connection between the three main chapters of his study except that they all deal with (widely differing) scandals reported over a 25-year period in the party's press. Hall has posed no central problem: his work is neither a study of the party press, nor a systematic analysis of the measures adopted by the Wilhelmine state to restrict or repress the SPD, nor an attempt to investigate the role of agitprop in the party's overall political work. To be sure, Hall touches on all these problems, but in the end he remains in the nebulous, undefined area between them. By concentrating his attention narrowly on the party's official, printed propaganda, he fails to come to terms with the political practice of the SPD or with the social assumptions behind its agitprop line. Nevertheless, Hall's work is useful in that it is the only major English-language study to deal specifically with the legal position of the SPD from 1890 to

1915 and with the kinds of harassment and limitations on freedom of speech and the press under which the party operated.

Max Kele, on the other hand, has chosen what should have been a well-defined, major historical problem — the relationship of German Nazism to workers during the Weimar Republic — but he does not know what to do with his subject. Kele unnecessarily restricts his analysis almost exclusively to the written propaganda and ideology of the NSDAP, thereby ignoring all the major questions of politics and social history which a subject of this kind ought to raise. Even on the level of propaganda, Kele fails to put the Nazi approaches to workers in the overall context of the party's agitation and propaganda: it is simply impossible, on the basis of Kele's study, to measure or judge the extent of importance of Nazi agitation toward workers in relationship to the totality of the Nazi movement. Kele's work lacks a necessary comparative element. The problematic nature of this study, however, runs deeper, for nowhere does Kele define what in fact he means by "worker." Instead of critically analyzing the Nazis' ideological, propagandistic definition of "worker" — a definition used to obscure class divisions by inflating the working class to include virtually everyone, even employers, who contributed to production through mental or manual labour — Kele implicitly adopts it as his own. As a result, he confuses both the flexible nature of Nazi propaganda and the specific social groups toward which it was directed. Finally, where Kele could (and should) have added a social historical dimension to his study, he shows no knowledge of social historical methodology. For example, in analyzing election statistics, Kele is unfamiliar with the principles of electoral analysis and electoral geography, and he does not even adequately summarize the conclusions of reliable secondary works by historians who have analyzed the social distribution of Nazi voting strength. Likewise, in

describing the NSBO, the Nazi attempt to build a factory-based organization of workers during the Depression, Kele asks none of the relevant questions — and therefore has done none of the primary archival research — that might have illuminated this side of Nazi history. Who belonged to the NSBO? Where did they work? In what trades, industries, factories, offices, geographic regions? To what extent did the NSBO draw its support from older yellow (company) unions? One could start by gathering statistics on NSBO participation in works councils' elections to determine where and among whom the Nazis won support. This is a rich, unexploited source of information of which Kele seems to be unaware. As a strictly empirical, chronological summary of Nazi propaganda and ideology about workers, Kele's study is not without usefulness, but in relation to the larger historical questions of Nazism and the German workers' movement it is a very disappointing and inadequate work.

Leila Rupp's study of the propagandistic mobilization of women for work in the U.S.A. and Germany during World War II is the most successful and satisfying treatment of the uses of propaganda. She defines her problem clearly as the attempt by the state to change the public image of women in the short run to encourage them to enter the work-force in the place of men drafted into the military. She begins with a summary of the public image of women in the U.S.A. and Germany in the decades prior to World War II in order to put her analysis of wartime propaganda into a longer historical perspective. This in itself broadens the scope of her study, which Rupp reinforces by choosing to make an international comparison between the two major protagonists of World War II. The conclusions she is able to draw from such a comparison constitute an important contribution to the historiography of the women's and workers' movements. In particular, she underscores both the flexibility of Nazi propaganda and the inflexi-



bility of Nazi political practice; that is, although Nazi propaganda left open the possibility of mobilizing women in support of the régime, when it came to actually doing so after 1939 the Nazis would not mobilize women extensively for fear of reproducing the social antagonisms that had undermined the state during the World War I. More generally, Rupp demonstrates both the similarities of U.S. and Nazi propaganda toward women and their differential success in mobilizing women for war. She thereby raises the historical question of the greater ability of a democracy to wage total war, despite sexual and class divisions, even though Nazi totalitarianism possessed on paper much greater organizational and coercive potential for mobilization. The major limitation of Rupp's study is that it deals only with the public image of women in relation to work; it lacks the social-historical dimension of the actual practice of women. Rupp has covered her subject very well, and it should now be the task of historians to extend their studies to this latter theme.

Barrington Moore looks at the labour movement from the workers' side in order to determine why workers at times passively accept their subordination while at other times revolt to change it. Moore's main concerns are theoretical, and he consequently organizes his book into three sections: a general theoretical introduction, a lengthy consideration of the historical example of the German workers' movement 1848-1920, and an extended theoretical discussion of the implications of the first two parts. Looked at more closely, however, Moore's work is not that much different from the other works under review, for his approach is not really social or sociological at all but rather moral and his main sources of information on German workers consist of workers' biographies and statements by workers about their attitudes and aspirations. Moore has merely reversed the traditional emphasis on ideology: where other historians treat the workers' ideology

from the point of view of the state and workers' organizations, Moore considers it from below, from individual workers. Unchanged is the primary emphasis on ideology. The most problematic aspect of Moore's approach is nevertheless his pervasive moralism. His main concern is to show how "moral outrage" out of a sense of injustice can lead to revolt. As one precondition of working-class discontent, this hypothesis is not without merit, but Moore makes it a general explanation of revolt. Moreover, he bases his analysis on a series of unfounded assumptions about human nature and society; indeed, he admits at the beginning that his assumptions cannot be scientifically proven. He posits *a priori* biological and psychological determinants of human behaviour, although they only manifest themselves socially and cannot be investigated by themselves. He also treats society as a collection of discrete individuals (a return to Robinson Crusoe and eighteenth-century individualism) and assumes the existence of a social contract (another return to the eighteenth-century ideology of early capitalism). As a social theorist, Moore makes too many such unverifiable assumptions, and his concentration on "moral outrage" as the explanatory factor is much too diffuse and unspecific to bear the history of the German workers' movement.

Moore's treatment of German workers is also open to a number of specific objections. He overemphasizes workers' biographies as a source in a country where, more than anywhere else, working-class consciousness developed through the organized workers' movement. He adopts an arbitrary and unsatisfactory definition of the "proletariat," limiting it in effect only to workers in the largest and most technologically advanced factories, and he manipulates German statistics to argue that it was small in size. Finally, he selects only a small segment of the German working class to discuss at length (miners and steelworkers in the Ruhr); he

ignores equally important sections which might have changed his conclusions; and even in Rhineland-Westphalia he makes elementary errors about the geographic, socio-economic, and organizational structure of the working class and the workers' movement. (Moore includes all of Rhineland-Westphalia in his discussion of the Ruhr, though in fact the Ruhr is not only a small part of Rhineland-Westphalia as a whole but also constitutes only half of the very diverse Rhenish-Westphalian industrial region narrowly defined.) Moore tends to preselect his evidence to fit his argument and discards the large bodies of information and the alternative historical approaches which contradict it. On the other hand, Moore's work has great interest when read as speculative social philosophy. He exhibits a healthy skepticism in refusing to accept any rigid historical determinism, and he repeatedly makes stimulating critical remarks on specific aspects of German labour history in the course of his commentary. These remarks should encourage historians to rethink conventional, often partisan interpretations of German history (for example, what possibilities were open in the 1918-19 revolution). Theoretically, this work is unconvincing and overstated; its real value lies in the many critical comments and analyses Moore makes along the way.

In conclusion, these four works illustrate two serious failings of German labour historiography. They continue to emphasize traditional historiographical problems and methods and show a reluctance to broaden the subject matter to include social historical questions. And they maintain a rather studied divorce between social theory and primary research, instead of guiding the latter by social theory or revising theories through carefully defined empirical research.

Larry Peterson  
New York

Ian MacDougall, ed. *A Catalogue of Some Labour Records in Scotland and Some Scots Records outside of Scotland* (Edinburgh: Scottish Labour History Society 1978).

THIS CATALOGUE ARISES out of many years of compilation by Ian MacDougall and is based partly on an earlier interim bibliography published by the Scottish Labour History Society in 1965. The present greatly expanded catalogue is notable for two reasons. First, there is the sheer size of the book. It contains over 500 pages of tightly packed references to both primary and secondary sources on Scottish Labour History. Second, it is important to realize that the expression "labour records" is interpreted widely.

Some idea of the scope of the book can be gathered from the following breakdown. As might be expected the largest section (200 pages) is concerned with trade union records of the nineteenth and twentieth centuries which have been organized by industry. In a very few cases the records on deposit of this type relate to the late eighteenth century.

There are also 42 pages devoted to Friendly Societies and 35 to the Co-operative movement — all arranged by geographical location. Over 100 pages of the catalogue deal with political movements and this essentially refers to archival materials on deposit in Scotland which are concerned with the Labour party, Communists, Anarchists, and other left-wing groups of the mid-nineteenth century onwards. In an effort to give a deeper chronological dimension to the catalogue some material relating to Chartists and other early nineteenth-century radicals has been included. This has been largely accomplished by an index of series 102 of the Home Office papers, for the years 1792-1835 as they relate to Scotland. While this is not a comprehensive listing, it is an invaluable aid to research in this area. To all this has been added a 120 page index of biographical materials

on prominent members of the Scottish labour movement. Finally the whole catalogue has been meticulously indexed for easy reference.

Perhaps the best recommendation for this book is a personal one. If I had Ian MacDougall's catalogue available to me when I plunged into research on early nineteenth-century Scottish radicalism, I would have been able to proceed much more efficiently. This is a specialist's work which anyone researching modern Scotland will best appreciate. The catalogue should also be acquired by institutions with research programs in British and/or general labour history.

F.K. Donnelly  
University of New Brunswick

*Revolutionary Situations in Europe, 1917-1922: Germany, Italy, Austria-Hungary*, Proceedings, 2nd International Colloquium, 25-27 March 1976 (Montreal: Interuniversity Centre for European Studies 1977).

THE SECOND INTERNATIONAL Colloquium of the Interuniversity Centre for European Studies brought together 21 leading scholars from six countries to discuss the problem of revolution in Central Europe during the turbulent and decisive years 1917-22. The speakers represented a variety of disciplines and discussed the historical, political, social, and economic dimensions of the upheavals in Germany, Italy, and Austria-Hungary after World War I. The central theme of the colloquium was the problem of revolutionary situations — whether revolutionary situations existed in the countries of Central Europe after World War I, the extent to which they were expressions of the socialist and workers' movements, and the role of other social classes and groups. The papers given at the colloquium and the general discussions which followed them have been collected in this volume. Despite a growing literature, the revolu-

tions of 1917-22 (outside the Russian) have not been studied to the same extent as other revolutionary periods, and little of the existing literature is cross-national in scope. Thus, these papers constitute an important contribution to the comparative and interdisciplinary historiography of the revolutionary period 1917-22 and serve as a useful summary of the current state of scholarship.

After an opening session, during which Pierre Broué analyzed the bolshevik evaluation of conditions in Central Europe and Francis Carsten discussed the general problem of revolutionary situations in these countries, the colloquium proceeded in two parts. The first set of papers dealt with each country — Germany, Italy, and Austria-Hungary — separately. The approach was national in orientation and limited to the short-term crisis in each country. The second set of papers, on the other hand, were topical, cross-national, and concerned with long-term trends. Sessions were devoted to agricultural, political, and ideological problems, as well as to the role of industry. Oddly, no separate session was set aside to discuss long-term problems of development in the socialist workers' movement or the evolution of social and economic conditions among industrial workers. Such problems were touched on only to a limited extent in Gerald Feldman's analysis of changes in industry during the war and in Miklos Molnar's discussion of ideological approaches to revolution in the international socialist movement. In light of the socialist and proletarian nature of the attempted revolutions, a separate session on this problem would have been useful. André Donneur concluded the second set of papers with a theoretical analysis of what constitutes a revolutionary situation and whether the concrete situations in Central Europe after 1917 were in fact revolutionary. Finally, Arno Mayer closed the colloquium with a discussion of the role of internal crisis and war in European politics from 1870 to the

early 1920s, in which he shifted emphasis from the threat of socialist or proletarian revolution to the aggressive attempts of conservative elites throughout Europe to consolidate their political power and economic privileges even as socio-economic changes undermined their traditional power base.

The format adopted at the colloquium allowed for an in-depth look at the revolutions in Central Europe from a variety of perspectives. The colloquium balanced monographic national historiography with an equal number of presentations by social and economic historians, political scientists, and historians of ideas. And, although the upheavals in Central Europe were limited to the years between 1917 and 1924, they need to be seen in terms of long-range developments. The colloquium gave ample room for the investigation of such trends. Nevertheless, despite this format, the colloquium only partially achieved its goal of furthering the comparative and interdisciplinary study of the revolutionary situations in Central Europe. The content of the individual papers is too diverse to review in detail. But, in two key respects — theoretical and methodological — the speakers failed in large part to grasp the problem that faced the conference.

Although the colloquium was devoted to the problem of revolutionary situations, only two speakers — Charles Maier and André Donneur — attempted to define theoretically what they meant by this term. The other speakers ignored or hedged the problem. Yet it is impossible to discuss the nature of the upheavals in Central Europe without a general theoretical definition of a revolutionary situation. Maier defined a revolutionary situation as the conjunction of a long-term crisis of representation, arising from socio-economic and political changes, with a short-term crisis of legitimacy, while Donneur discussed at length the kinds of objective and subjective preconditions that give rise to revolutionary situations and

actual revolutions. Donneur's paper, which is essential reading for any historian trying to grapple with this problem, would have been better presented at the opening session, for it gives the kind of central focus the colloquium lacked. On the other hand, the opening papers by Broué and Carsten failed to set out the terms of the discussion — Broué's because he concentrated on the interesting but secondary question of bolshevik assessments of concrete possibilities in Central Europe, and Carsten's because he gave no more than a narration of events and did not attempt to analyze the historical problem of revolution. Thus, the colloquium by and large failed to address the problem it was convened to discuss. It missed the opportunity to define what constitutes a revolutionary situation in the twentieth century and how this concept relates to the concrete revolutions and revolutionary movements in Europe at the end of the World War I.

The second limitation of the colloquium — methodological — arose from the division of the sessions into national histories and analyses of long-range trends. The first group was dominated by political historians who approached the period as a chronological narrative of events, and their presentations underscored the inability of narrative political history to focus on the analysis of problems rather than the explication of events. For example, they used social and economic history only as an explanatory factor brought in on an *ad hoc* basis, and not systematically to analyze political processes. And they were weak in defining the subject for historical investigation beyond the course of political decisions, with a strong bias toward the actions of political leaders "above" rather than the socio-economic trends, conditions, and movements which sharply curtailed the significance of individual decisions and actions.

The papers presented in the second part of the colloquium were both more

original and more solid contributions to the history of this period. Nevertheless, the division of the colloquium into two sections precluded the linking of national events in the short run to long-range trends. How did the long-range trends cause, condition or counteract the emergence of short-term revolutionary situations in Central Europe after 1917? No speakers directly addressed this central question because none broke out of the disciplinary and methodological divisions of institutional scholarship. The colloquium offered no resolution of its juxtaposition of national political and cross-national interdisciplinary, topical approaches. Charles Maier and Gerald Feldman offered some useful points of departure for such a combined approach, but André Donneur was the only speaker to focus his paper rigorously on the relation between the general and the concrete.

In conclusion, the papers collected in this volume leave unresolved the question of whether revolutionary situations in fact existed in Germany, Italy, and Austria-Hungary after 1917, and, if so, what this meant in the long-range development of modern Europe. As Charles Bertrand remarks in his introduction, the speakers tended to split over their evaluation on the nature and existence of revolutionary situations according to the overall political and ideological perspective from which they approached their subject matter. Those, mostly liberal, scholars, who place this period in the long-range "modernization" of Central Europe, tended to discount the revolutionary potential of the upheavals after World War I, seeing them more as the result of partial and disjointed industrialization rather than a socialist or proletarian challenge to bourgeois society, whereas those scholars who draw on theoretical Marxism saw far greater socialist revolutionary content in the popular movements and political crises of this period. Whichever side one takes, the papers collected in this volume will provoke further and, one hopes, more inten-

sive discussion of the interpretive, theoretical, and methodological problems in understanding the nature of revolution in modern Europe.

Larry Peterson  
New York City

Edward Acton, *Alexander Herzen and the Role of the Intellectual Revolutionary* (Cambridge: Cambridge University Press 1979).

MR. ACTON writes old-fashioned history. It deals with ideas, has a biographical focus, and is restricted to published materials. Consequently, the most that can be expected from his efforts is a new synthesis, or reinterpretation of materials which have been quite thoroughly covered before. Such reconsideration is common in intellectual history, especially when — as with Herzen — there is unanimity about his importance yet controversy about his ideology. For Alexander Herzen (1812-70) was Russia's first and greatest pre-Marxist social democrat; his theoretical contribution (especially through his own London press) to western understanding of Russia was seminal; and his personal odyssey set the standard for all subsequent Russian emigrations.

The present work is written in the shadow of Martin Malia's *Alexander Herzen and the Birth of Russian Socialism* and, to a lesser extent, E.H. Carr's *The Romantic Exiles*. Acton's effort can best be understood as an attempt to extend and revise these two heuristic monographs. Thus, in terms of the time frame, Acton picks up more or less where Malia leaves off, the years between 1847 and 1863. Acton's methodology is also quite orthodox; it stresses the impact of "public events and private tragedy upon [Herzen's] political thought and activity." The operative premise is that the key to Herzen's politics may be found in his personal life. Specifically, Acton argues that Herzen's optimism about human

nature and his enthusiasm for the struggle to build socialism (in Russia as well as in the west) suffered a combination of decisive public and private blows during the period leading up to the serf emancipation of 1861. The two major instances cited by Acton in this regard are the defeat of the 1848 revolution and the nearly simultaneous affair between Herzen's wife and his best friend, the German poet Herwegh. Allegedly this tragic coincidence was at the roots of Herzen's growing reservations about social engineering in general and socialism in Russia in particular. Underlying these reservations, no doubt, was the dilemma of making peasant revolution in a "backward" society.

There is an excessively polemical quality, especially in the first half of the book. Too much is made of minor differences with Malia of detail and emphasis. For instance, Acton's attempts to revise the latter's picture of Herzen's attitude towards the Russian peasant commune (19) and Russian nationalistic messianism (27-29), as well as his wife Natalie's death (84) are either not entirely convincing or they do not significantly alter our understanding of Herzen's motivations anyway. On the positive side, Acton makes excellent use of the extensive secondary scholarship available in Russian and the major western languages. This is no small accomplishment, given the complexity of the issues raised by Soviet scholars alone. Contrary to conventional western stereotypes, differences between the interpretations of the Ginzburgs, Koz'min and Smirnova, on the one hand, and those of the Nechkina school, on the other, are quite basic.

In addition, Acton has a fine sense for the libertarian side of Herzen's nature, and its significance in defining his rather unique ideological position between Russian liberalism and the new radicalism of the 1860s. Almost alone, Herzen saw the terrible antinomy between the individual and the state in Russia, and he despaired of it. It may be that this, even more than

the double tragedies of 1848-52, accounts for Herzen's growing frustration and resignation. But he tried to fight it with all the intellectual tools at his command. Initially that meant ignoring the political issue in the name of social and economic progress under the aegis of the benevolent autocrat Alexander II. When it finally became clear (at the very end of the 1850s) that peasant emancipation and related reforms were not being carried through in good faith, Herzen became much more radical.

What emerged from this process of radicalization was a dialectical analysis of the advantages of Russia's backwardness. That is, Herzen argued that precisely because of her apparent backwardness, Russia would be able to make the transition to socialism easier and sooner than the institutionally encumbered capitalist west. This formula, with its roots in left-Hegelian thought, appealed to both national pride and revolutionary imagination, and became the chief article of faith of *narodnichestvo* — Russian populism or peasant socialism. It was based upon an understanding of the role of the peasant commune in making Russian historical development unique. Thanks to the communal nature of land ownership among the Russian people, capitalism could be bypassed on the road to socialism, and the last would get there first.

Whatever his earlier differences with Chernyshevsky and the "revolutionary democrats," or his reservations about the political maturity of the Russian peasantry, after 1861 (and especially the suppression of the Polish rebellion of 1863) Herzen openly espoused the overthrow of the imperial régime and full social democracy. In the process, he had come to respect the intelligence and consciousness of the people. "Manna does not fall from heaven," Herzen commented; "it grows from the soil." And the wisdom of German Idealism was historically less important than the crude materialism of the Russian peasant. But Acton is

wrong to conclude from all this that Herzen's "role had been written out of the revolutionary drama." In fact, Russian Marxists and non-Marxists alike have seen in his thought the bases of their own positions. Surely Herzen is not to blame that his views could in time lead to contradictions. Had he not himself warned of the fundamental tension between the egalitarian and libertarian principles, as well as the potentially tragic consequences of either prevailing?

N.G.O. Pereira  
Dalhousie University

Paul W. Drake, *Socialism and Populism in Chile, 1932-52* (Urbana: University of Illinois Press 1978) and Colin Henfrey & Bernardo Sorj, *Chilean Voices: Activists Describe Their Experiences of the Popular Unity Period* (Hassocks, Sussex: The Harvester Press 1977)

IN RECENT Latin American history the event that probably had the widest popular impact was the election of the Marxist president, Salvador Allende Gossens, in Chile in 1970 and his violent overthrow three years later. This period has spawned a wealth of literature, much of which should never have been published. However, this is not true of the two books under review for, despite being written from totally different perspectives and about completely different aspects of the period, both add to our understanding of the events. The book by Drake, in fact, deals specifically with the Allende period only in a concluding "Epilogue" which covers the years from 1952 to 1973 and which has been written, as he admits, with "no pretense of complete coverage or in-depth analysis." Nevertheless, his examination of the earlier period introduces some of the major problems of the Allende presidency, including the reformist nature of the Socialist Party and its commitment to democratic ideals, and the lack of unity of the Chilean Left.

Drake links these problems to the early development of the Socialist Party when it resembled a populist party rather than a party with a firm, class-based ideology. Both socialism and populism emerged in Chile in the early twentieth century in response to the economic modernization and social mobilization which were undermining the domination of the traditional élites. Workers were being radicalized by agitation and unionization and they began turning to Marxism at the end of World War I in response to the Russian Revolution and local economic problems. In the 1920 presidential election, however, most workers were won over by a populist figure, Arturo Alessandri, who promised them much but delivered little. Nevertheless, his victory was important in establishing links between middle class leaders and the workers, and the relatively open political milieu of his presidency allowed the formation of a Communist Party (PC) in 1922 whose strongly ideological line attracted organized labour.

The emergence of the socialists was associated with a general alienation sparked by the Depression. It transformed large sections of the population into "socialists," including army officers who in June 1932 took over the government and declared a "socialist republic." In fact, these officers owed more to populism than socialism for their movement was directed from above and called for evolutionary rather than revolutionary change. It enjoyed little support and fell within two weeks. However, it set the pattern for subsequent developments and led to the formal foundation of the Socialist Party (PS) in 1933. Like the socialist republic the party was dominated by the middle class who stressed reform rather than revolution and believed that success could be achieved via the electoral system. The workers had little influence on the new party since unions were weak at this time and few workers were politicized. The PS made little effort to

correct this, focussing their appeals for support on those who had already been politicized. It produced a multi-class party with a weak base led by a charismatic middle class leader who could attract adherents.

The reformist nature of the socialists was reinforced by their political success in 1938. At this time the PC, although still dominated by class elements, was also prepared to accept a multi-class approach in order to draw the middle class away from the right. Thus, the two Marxist parties united with the middle class Radical Party and other minor parties behind the Radicals' rather conservative leader in the "Popular Front" and won the presidential election in 1938. According to Drake, this success had a profound impact on the Marxist parties. Since they now shared political power their commitment to democracy increased and their calls for change were blunted. They did not take advantage of their power to organize the harshly exploited rural workers, and newly-formed urban unions were dominated by middle class leaders. Apparently, the left did not want to cause the government problems with the right which might provoke military intervention.

The Socialist Party's reformism eventually caused its disintegration and reorganization. In 1941 it left the Front because of the continued inclusion of the PC which the PS accused of being anti-national and authoritarian. Already radical members had begun splitting away and now others followed. The fortunes of the party reached their nadir in 1946 and then recovered behind new leaders who were determined to transform the party into a Marxist vanguard party and less of a populist mass movement. Moreover, from 1948 the PS was the only legal party on the left following the Cold War influenced suppression of the PC. The radicalization of the PS was followed by a restoration of relations with the Communists and together they nominated Salvador Allende

as their presidential candidate in 1952.

Despite this radicalization Drake feels that the early development of the PS continued to affect it and consequently also affected the Popular Unity government of Allende. Between 1970 and 1973 the party still adhered to constitutionality and democratic forms, now with the support of the PC. But there was a significant difference from their earlier alliance under the Popular Front. The workers were now allowed unprecedented participation in the system. It was a development which neither the right nor the military was prepared to countenance and it helped lead to the tragic coup. Drake concludes that Allende and the Marxists ultimately failed because they never resolved the "contradiction of using evolutionary institutions of formal democracy in a highly dependent capitalist system to pursue a theoretically revolutionary ideology aimed at the ultimate replacement of that system." (337)

The author presents substantial evidence to support his thesis. His one constant source is electoral records which he analyzes thoroughly. However, accustomed to the contradictory voting habits of Canadians I was not convinced that electoral results can be used to prove ideological shifts — although Chileans may be more committed voters than Canadians. My only other reservations about Drake's book are its rather ponderous writing style and its lack of some human elements which might have counterbalanced the pages of dry statistics and political analysis.

The weakness of Drake's book is the very essence of the work by Henfrey and Sorj. They have transcribed the recollections of individuals who participated in the developments between 1970 and 1973. Those interviewed do not represent all Chileans for, as the title indicates, they were activists operating in the industrial sector, the countryside, shantytowns, and universities, and they were members of the parties that comprised the Popular



Unity. In trying to explain what went wrong they agree in general with Drake's conclusions but also feel that Allende did not go far enough, that he did not give the workers "a real role in the process of building socialism." (48) The government was more interested in production than mobilizing the workers so that the factory and rural workers had to mobilize on their own. But this was an incomplete process and thus there was no adequate counterweight to the united strength of the right and the military. When the coup came the workers were either unable or unwilling to resist and the Marxist experiment came to a violent and bloody end.

Peter Blanchard  
University of Toronto

Michael T. Taussig, *The Devil and Commodity Fetishism in South America* (Chapel Hill: The University of North Carolina Press 1980)

A PRINCIPAL FOCUS of much of the present research on rural labour systems in Latin America is the impact of modernization or expanding capitalist relations on the "neophyte proletarians and their surrounding peasant kinsmen," as Professor Taussig describes them. This book describes the plantation workers in the Cauca Valley of Colombia and the mine workers of Bolivia, with frequent references to various groups of Indian workers throughout much of highland Latin America. Professor Taussig argues that since the time of the Spanish Conquest the concept of evil, as portrayed by the existence of the devil, has been firmly adopted by the Latin American masses in response to the deterioration of their traditional style of life at the hands of the Spanish overlords. Over the last 100 years with the increasing proletarianization of the peasantry the awareness of the devil has intensified and he has come to symbolize the disruptions that have taken place in their daily lives as they have been drawn into a market economy. Taussig has found that in some areas the peasants try to

avoid the devil's malevolence by performing elaborate rituals to him. Elsewhere workers have entered into contracts with the devil in order to obtain more money, but traditional customs have limited the use of that money by transforming it into a product of barrenness and death, thereby preventing complete absorption into the capitalist economy.

In the case of the Cauca Valley, Taussig is dealing with the descendants of black slaves who were imported to work on plantations and in the mines. The workers were influenced by their African heritage and their resistance to the slavery system. Following abolition in the 1850s there was continuing conflict in the area as *hacendados* confronted both marginal smallholders and their own labour force of cash and service tenants. The latter part of the nineteenth-century was marked by constant political unrest as well, much of it with a religious flavour as liberals challenged conservatives for power, and this provided the various groups with the opportunity to press their claims, frequently resorting to violence. The peasantry was finally dislodged in the present century following improvements in communications, expansion of the population with a simultaneous increase in the demand for food, and rising land values. Commercial agriculture expanded and "rural proletarianization began in earnest." Once more the peasants reacted violently but the gradual shrinking of their landholdings forced them to turn to the cultivation of cash crops and employment on the estates as casual labour in order to survive.

It was in response to this absorption into the capitalist economy that male plantation workers began making secret contracts with the devil to increase productivity and hence their wages. But traditional values continued to predominate for this capital was viewed not as a route to long-term improvement, but rather as a means for immediate gratification. Any attempt at using the money for investment purposes would fail and the person who tried this

would die an early and painful death. There was only one way which capital could earn a return safely and it, too, occurred within a religious framework. The money had to be baptised along with a child, but in such a case the soul of the child was lost. The risks were obviously great and Taussig states that instances of this, like the devil contract, were rare and may in fact only exist in rumour.

In the case of the Bolivian tin miners, Taussig points out that the presence of the devil is more apparent. A small figure sculpted of clay, pieces of metal, and glass with a gaping mouth awaiting offerings of cocoa and cigarettes and hands ready for liquor is found in the mine shafts. Rites to it are essential to prevent accidents or some other catastrophe. Taussig again ties the devil's presence to modernization: "proletarianization of peasants into miners and the modernization of Indians have led not to a disenchantment of the world but to a growing sense of destructiveness and evil as figured in the devil."

The arguments presented in the book are not entirely convincing. One obvious problem is the material used for the two areas. While the section on the Cauca Valley is based largely on primary research and personal interviews, the section on Bolivia relies entirely on printed material. Taussig brings together a variety of information to support his hypotheses, but the reader learns nothing new about the Bolivian miners. On the other hand, the material on the Cauca Valley workers is new and consequently a valuable addition to existing works on the impact of modernization on rural workers.

In the section on Bolivia there is also a limited sense of change over time. Taussig's picture is of present conditions — although he uses some sources that were written 30 or more years ago — and, while anyone who has been to La Paz and seen the market stalls lies very heavily on the Bolivian Indians, there must be some transition of the type which makes his Colombian chapters so valuable.

The book might also benefit from less

jargon and greater objectivity. Taussig's hostility to capitalism leads him along some strange paths, like his contention that the idea of a pervasive spirit of evil was an import of imperialism. His own research indicates that in fact it was an import of Christianity, a more reasonable view, although some historians and archeologists who have studied indigenous American religions might disagree.

In sum, the book is an interesting and thought-provoking examination of the related impact of religion and modernization on the Latin American peasantry. It points to one of the glaring weaknesses in Latin American historiography, the lack of information on the motivations and changing relationships of the workers over time, and while providing one explanation of a particular aspect, Professor Taussig leaves an enormous vacuum that begs to be filled.

Peter Blanchard  
University of Toronto

Martin A. Klein, ed. *Peasants in Africa: historical and contemporary perspectives* (Beverly Hills: Sage 1980).

PEASANTS IN AFRICA is a collection of studies solicited especially for this volume, and includes several by French scholars whose work often is not easily available in English. Klein's apologies for the volume being "top-heavy on West Africa" (7) notwithstanding, it is perhaps more important (with respect to its aims to present "historical and contemporary perspectives") that it cover a representative range of socio-economic formations during different epochs of the penetration of capitalism into Africa. In this, the selection of contributions succeeds admirably. Following an introduction by Klein are chapters on: "Peasants in the Colonial Congo" (Bogumil Jewsiewicki), "The Evolution of Peasantries, Senegal" (Jean Copans), "Social Differentiation among the Mawri, Niger" (Elaine De Latour Dejean), "A Historical Perspective on the Baule Region, Ivory Coast" (Jean-Pierre

Chauveau), "Landholding in Nigeria" (Julian Clarke), "The Bakosi of Cameroun" (Michael D. Levin), "Differentiation in Broken Hill, Northern Rhodesia" (Maud Shimwaayi Muntemba), and "Rural Resistance in the Transkei" (William Beinart and Collin Bundy).

In the introduction, Klein answers the question "what is a peasant" with the broad definition of a producer who provides his own subsistence, though involved in the market, and is subject to the exploitation of other social classes. The difficulty lies in differentiating a "peasant" from a "cultivator," who may also produce for the market and be subject to some degree of exploitation, or from a "capitalist small commodity producer," whose *real difference from a peasant may be measurable only in the amount of produce he sells on the market. "The line [between them] is often a fuzzy one."* (13) *If this is the case, one is tempted to question the usefulness of choosing "the peasantry" as a framework of analysis. Although Klein does not raise this specific issue, he goes on to develop a convincing argument for the aims of his approach. Essentially, he is examining a particular form of labour which seems to be differentiated from other forms of labour according to three criteria: the amount of surplus appropriated by an exploiting class, the means by which this is accomplished, and the proportion of surplus which is intended for market exchange.*

If the first two factors were frequently more significant in the emergence of a peasantry in pre-colonial societies, the second and third tend to dominate discussions of the peasantry under the colonial system, that is within the development of "peripheral capitalism." (17) This is true with reference to debates about the origins and future of the peasantry in general, and is clearly reflected in the seven studies presented here. The analyses tend to centre around the issue of "reproduction," both in the sense of how the needs of the colonial economy affected the ability of peasants to

reproduce themselves, and, in turn, how their success or failure affected the reproduction of the specific social formation of which they were a part. With reference to Marx, Klein argues that whereas in the "centre" the original process of capital accumulation tended to destroy earlier modes of production, in "peripheral capitalism" (in this case, as it has developed in Africa), the process is in fact dependent upon the survival of these "earlier modes" to absorb the costs of reproduction. "Essentially then, . . . the process involved . . . the articulation of capitalist and non-capitalist modes in a way that made possible a continued transfer of value." (17) The relations of production which characterize specific articulations aim to maximize this transferable surplus within the confines of the means of production available.

In the chapters which follow, the authors try to explore these theoretical issues further, while addressing themselves to the historical problems Klein identifies as being "crucial," namely, how do cultivators become peasants and why do they remain so. These questions are not easily answered nor is the historical perspective required to do so easily achieved within such a limited space. While the results vary, on the whole the analyses seem to cope better with the first question than with the second. In fact what emerges most clearly is the degree to which the peasantry underwent processes of economic differentiation which frequently reduced those near the "bottom" of the scale to subsistence cultivation or wage labour, and created of the "upper echelons" a class of petty commodity producers. This process was especially striking in the accounts of De Latour Dejean and Muntemba, and in the latter case, socio-economic inequalities were accentuated by the growth of marked regional disparities. Perhaps we need to emphasize more strongly that while the peasantry appears to have outlived its life-expectancy as predicted by Marx, in any given historical period its composition was likely to

have been an extremely fluid one.

Some of the authors identify the existence of a pre-colonial peasantry, (Copans, De Latour Dejean, and Muntemba, for example), but none really devotes analysis to the nature of its emergence. Rather, attention is generally focused on the colonial period and the ways in which the colonial economy subjected peasants to its own dynamics. Jewsiewicki and De Latour Dejean attempt to explain the changing nature of rural life in terms of the differing needs of merchant and industrial capital. Under the first, there was no fundamental necessity to alter the structure of production, colonial intervention affected only the sphere of circulation. Industrial capital, on the other hand, required that the rural sector absorb the costs of reproduction, as well as providing food and labour; in other words, it necessitated a restructuring of agricultural production and the creation of a peasantry.

In terms of marrying a sophisticated theoretical approach to a well-developed historical investigation, these two studies are perhaps the most successful. Copans raises important questions about our ability to write the history of peasants as opposed to constructing an anthropological view, and introduces the stimulating concept of "multi-ethnic space." — "the natural environment of African social formations. . . . Social formations, modes of production, and class . . . operate within a social dimension. But Africanists do not agree on the level of social reality that should be studied first." (81) I am not certain his chapter takes us that much closer to resolving this problem: it would have been useful for our understanding of his conceptual framework to have been presented with a more exhaustive analysis of historical data. On the other hand, Muntemba's fascinating material on the changing composition of the Zambian peasantry, particularly the tantalizing bits about the roles played by women, provided an excellent case study with which to further attempts at constructing a viable theoretical framework. How disappointing that she

chose a basically descriptive approach emphasizing the significance of low technology and "the inability to increase productivity due to the lack of more efficient factors of production," (263) thereby sacrificing analysis of the relations, in favour of the means, of production.

With respect to the individual contributions to this volume (several of which have not been discussed because of limited space), one further word should be added about the decision to include Beinart and Bundy's piece on peasant resistance. At first glance, the focus of this chapter seems to lie beyond the specific questions Klein poses; but, to the contrary, it proves to be one of the most interesting and valuable contributions to the book. Beinart and Bundy's analysis of resistance highlights, from a unique perspective, the nature and degree of social differentiation among the peasantry, the relation between socio-economic position, the ways in which political interests are perceived and defined, the conditions under which "class consciousness" emerges, and the repercussions of these conditions for the development of overt political action. Furthermore, their study illustrates vividly the instrumental role played by women both in defining the issues to be contested and in directing the actual resistance movements. Clearly, more attention is going to have to be given to the changing nature of the female, as distinct from the male, African peasantry if our understanding of its past (let alone its future), is to be furthered. Klein's book should go a considerable way in stimulating exactly this sort of attention.

Ann McDougall  
Duke University

Ellen Malos, ed., *The Politics of Housework* (London: Allison and Busby 1980).

ONE OF THE central issues of the last fifteen years for the women's liberation movement has been women's work in the home.

One of our biggest victories has been that we have defeated the notion that the sexual and family roles of women are rooted in an immutable biology of timeless "women's nature." Instead, the women's liberation movement has insisted, what women do in the home is historically determined work.

In *The Politics of Housework*, Malos has assembled eighteen articles which represent some of the key feminist formulations that have emerged as part of the struggle to define the sexual division of labour and the character of domestic labour. As a collection, the book's strength is that it draws together various analyses so that the working out of the ideas becomes visible. As one moves through the collection, the reader becomes aware of the growth of a debate. The articles move from a critical description of the situation of women in the home to a strategic debate about how to transform that work and end the oppression of the women doing it.

Malos sets off the whole book with a good introductory essay which summarizes the main points in these debates and shows the impact of these arguments on the English women's liberation movement. I was particularly impressed by the way she untangled the wages for housework debates, showing some of the intricacies of the development of those ideas, especially the way the original formulation of a guaranteed income for all was reduced to wages for housework. This focus on the wages for housework debate and on the impact of their demands on the women's liberation movement was the one aspect of the book that struck me as odd. Their representation in this book seems out of proportion to their impact. In her essay, Malos herself summarizes many of the alternative arguments put forward by other strategists. I would have preferred to see more of these writers represented in the collection. In summing up her introduction, Malos draws on the lessons of all the articles in the book to put forward a number of fundamental demands: 1. an end to the sexual division of labour both inside and outside the home; 2. socializa-

tion of as much domestic labour as possible; 3. adequate social services and benefits for everyone; 4. fight against sexism in all its forms, especially violence against women. (36-41)

The first article (Catherine Hall, "The History of the Housewife") sketches a rough outline of the main changes that have occurred in the occupation of housewife since the fourteenth century. Hall stresses the origins of some of the main features (such as the ideology of domesticity) of the modern housewife job. Her central argument is that the occupation of housewife is crucial for understanding the position of women and that "the history of women in the home — of the changing nature of marriage, childcare, and domestic labour, for example — is an area which badly needs exploration." (44) The next three articles, (Charlotte Perkins Gilman, "The Home: Its Work and Influence," 1903; Margaret Bondfield, "Women as Domestic Workers," 1919; and Marjorie Spring Rice, "Working Class Wives," 1939) are particularly interesting because they are now historic pieces. Written in the first few decades of the twentieth century, they are all striking for the depth of understanding they demonstrate about women's domestic situation. Compared with contemporary writings, they show that while the situation in the home has changed dramatically throughout the twentieth century, the structure of domestic labour has remained the same. What I found particularly disturbing about these three articles was the fact that, although these issues were part of an ongoing public discussion prior to World War II, the disintegration of the women's movement and the reaction of the Cold War years meant that these ideas were lost. When the second wave of the women's movement re-emerged in the late 1960s and began to examine the character of women's oppression in the family, we had to start from scratch. These articles reaffirm the importance of knowing and retaining our history — a task this book fulfills very nicely.

The remainder of the book consists of

14 articles published between 1970 and 1975 in Canada, England, Italy, and the United States. They illustrate first of all the international character of the women's liberation movement for they draw on each other and elaborate each other's arguments in an integrated and exciting way. They clearly demonstrate the relationship between personal experience, anger at our oppression, analyses, and strategies for change. The very moving piece by Suzanne Gail, "The Housewife," in which she describes the process by which a vibrant, intelligent woman "dwindles into a wife," aptly expresses the bewildered pain so many women have experienced. The next step toward feminist consciousness is represented by Pat Mainardi's famous "The Politics of Housework" where, with biting humour, she documents the struggle between women and men to challenge the traditional sexual divisions of labour in the home. From this pain and anger emerged the impetus to understand women's work in the home both theoretically and strategically.

Other articles demonstrate the evolution of our understanding and the development of different positions within the women's liberation movement. They particularly reflect some of the complexity of the debate which has been generated in the last decade between feminism and marxism. For example, Margaret Benston's article "The Political Economy of Women's Liberation" made the first big breakthrough by recognizing that women's oppression in the family was not just based on psychological and ideological factors but is also firmly based on economic relations. Peggy Morton in "Women's Work is Never Done" extended this idea by showing how women's work is "the maintenance of and reproduction of labour power, i.e. that the structure of the family is determined by the needs of the economic system, at any given time, for a certain kind of labour power." (138) The rest of the articles reflect the development and elaboration of these ideas. Malos has selected a good range of material which

represents the various positions put forward on the domestic labour debate and on issues surrounding the strategic and tactical questions of wages for housework.

Missing from the collection is a discussion of two important issues in the politics of housework. None of the articles deals with the political activities of women who are housewives, nor do they consider the tension between housewives (many of whom are not feminists) and the women's movement. However, the important point this book does illustrate is that women's work is the home is *work*. Like any occupation it has a history, its workers are engaged in a changing labour process which affects their relations with other people and their political activities. In addition, unlike other occupations, women's work in the home is concerned with the reproduction of labour power and hence is a factor in all paid occupations. Therefore, when researchers conduct studies of other labour situations, they must begin to relate to their study the question of domestic labour, the role of women, and the sexual division of labour.

These articles are some of the best expressions of the feminist recognition that an analysis of class relations is inadequate by itself, that what is essential is an analysis of the interaction of sex and class relations. Unfortunately, as some of the debates at the 1981 COSS and CSAA meetings pointed out, the importance of this lesson has not yet penetrated very far into the realms of mainstream political economy or labour studies.<sup>1</sup> For those interested in becoming familiar with these issues, this book will be an excellent place to start.

Meg Luxton  
McMaster University

<sup>1</sup> The Committee of Socialist Scholars (COSS) and the Canadian Sociology and Anthropology Association (CSAA) both met in Halifax in May 1981. A number of the papers presented at these meetings addressed this question and one result of the discussion is that a main theme of the 1982 COSS meetings will be the relationship between sex and class.

John Fox and William Johnston, *Understanding Capital: A Guide to Volume I* (Toronto: Progress Books 1978) and Frank Cunningham, *Understanding Marxism: A Canadian Introduction* (Toronto: Progress Books 1978).

IT IS A CENTRAL paradox of our times that, during a period of its greatest apparent success, Marxism is in its deepest crisis. While about one-third of the world's population live in states which proclaim Marxism as official ideology, the versions adopted are not only different but often antagonistic. What appears as success, then, actually masks a crisis in theory and practice. At the same time, the spread of Marxism in western academic circles may be little more than institutionalized criticism, divorced from working-class movements. If there is a potential strength, however, it is in a diversity of views which attempt to confront the failures of twentieth-century socialism within a Marxist perspective.

The two books under review, which come out of the Toronto Marxist Institute, are important because they attempt to bridge the gap that exists between the theory of Marxism and its real constituents. Although they share a single origin and a common enterprise, these two works have to be evaluated quite differently.

Marx's *Capital* is a crucial starting point for a study of the capitalist mode of production, and *Understanding Capital* aims to provide the reader with some intellectual raw materials for this purpose. It deals with a single work, Volume I of *Capital*, "a difficult, century-old text," (ix) with the object of making what is written more comprehensible. The book provides short chapter by chapter summaries of the main arguments, divided into eleven sections of roughly equal length, corresponding to a "course" division of the material. A series of questions is presented for clarification or discussion. *Understanding Capital* is most useful for assisting a first reading of the text, especially as part of

collective study. The issues raised in *Capital* are controversial and an understanding of the capitalist mode of production must go beyond an uncritical reading of this single text. The explicit purpose of *Understanding Capital* is neither to defend the original text nor to discuss these issues of debate. However, it would have been useful if the contours of these current controversies had been sketched and references to other texts cited.

In contrast to this, *Understanding Marxism* does attempt to situate its exposition in the context of present-day debate. Marxism consists of numerous texts and, simplistically, it may be assumed that an introduction to the general principles would be largely a question of selection or emphasis. However, comprehending what is said in a text is a vastly different undertaking from understanding a social science. Obviously there is not only one translation or interpretation of a single text and subtle variations in emphasis, even distortion, are likely depending on the version of Marxism to which the commentator is predisposed. No complete distinction can be drawn between one text and many texts. Nevertheless a discourse about "marxism" as a single theoretical outlook is much more problematic.

While any explanatory concept is "theory-impregnated," there is an important difference between theoretical principles and social reality. For Marxist theory, in which an evaluation of practical activity within society is crucial for developing approximate knowledge, the link between general principles and less abstract political lines cannot be drawn absolutely. But there is still an important analytical distinction to be made. *Understanding Marxism* conflates these two aspects and thereby implies that the political lines offered (what is to be done) are equivalent to the "objective laws of development" presented in the text, rather than both these propositions and "laws" being controversial positions in contemporary debates within Marxism. In this fashion,

Marxism is presented as a holistic, unitary theory situated in the context of bourgeois criticism.

Without doubt, Marxist thought is so situated. The most useful aspects of this book are the responses made to the various critiques of socialism originating from outside Marxism, which continually recur and will not disappear on their own. The text grew out of practical efforts to explain and defend Marxism in the face of prejudice and misinformation. Given these facts, and its commendable style which makes it accessible to most interested readers, the book would have much to recommend it. But inevitably a review of this text cannot be separated from an evaluation of the political positions taken. From its inception Marxism has not developed in a linear direction. *Understanding Marxism* ignores most of the pressing theoretical issues which have been raised since the 1950s within Marxism itself. It accepts the appearance of political success in the world without probing its substance. Marxism is presented as a body of propositions and laws to be explained and understood rather than as a scientific approach with alternative conceptions and lines which need to be debated in class terms.

This is not only a problem of mission because it affects the interpretations presented. A few examples: while generally arguing a positivist version of Marxism as consisting of objective laws of historical development, leading the reader to look first for structural causes, the central problem is defined ideologically, as a matter of radical education to overcome the conscious media distortions of Marxism. The theory of crisis presented in the text is an inadequate underconsumption/break-down model. (24) Workers in Italy and France are seen as possibly "on the eve of transforming those countries to socialism" (29), but the nature of Eurocommunism is not discussed. Under socialism, we are told, there are not the same feelings of national antagonism (58), a position which hardly begins to explain wars between

nominally socialist states, an issue which is never raised. Much more needs to be said about racism and sexism if it is true that the transition to socialism does not automatically end these practices. (56) Theories which suggest that the economic conditions of a socialist state provide fertile ground for the political consolidation of a new exploitative class are simply labelled "anti-Marxist." (92-94)

These issues, as well as others, are as pertinent to an understanding of Marxism in the contemporary world as the bourgeois critiques so appropriately debunked. The fact that there is no consensus about these issues may be a more profound statement about the scientific status of Marxism than an outline of its laws of development.

Consistent with its title, *Understanding Marxism* is a "Canadian Introduction" because it uses some Canadian examples to illustrate its general laws and types of bourgeois ideologies. But Marxism in Canada must be more than the fitting of Canadian institutions into "universal" moulds. Consistent with its scientific nature, the further development of Marxist theory will dovetail with its concrete application in Canada and elsewhere. We need a Canadian Marxism but it must begin by recognizing the centrality and diversity of debate, with the emergence of general principles from the clash of political lines and practices. This would be a difficult and lengthy undertaking, evaluating the experience of the early socialists, the influence of the Third International, the new left, as well as contemporary social democracy and ultra-leftism. Within such a schema, *Understanding Marxism* describes one political perspective among many.

A great many changes have occurred in the world since World War II, not least in the socialist world. Marxist theory has yet to account satisfactorily for most of them. From this perspective, *Understanding Marxism* could have been written two decades ago. At the risk of a stereotype, this may be the most ironic basis for the claim



that the book is a "Canadian" introduction.

Anthony Thomson  
King's College, Cambridge

István Mészáros, *The Work of Sartre: Volume One: Search for Freedom* (Atlantic Highlands, N.J.: Humanities Press 1979).

THE CENTRAL TASK of Sartre studies today is to explain the apparent dichotomy in Sartre's work between the early existentialism and the later Marxism. The interpreter of Sartre must make the radical twists and turns as well as the self-criticisms which punctuate Sartre's career comprehensible within a general understanding of Sartre's work. István Mészáros established what he takes to be the basis for a general understanding of Sartre's work, the continued existence of an antinomy in the Sartrean problematic. In the latter part, he analyzes the early existentialism of Sartre concentrating, not surprisingly, on *Being and Nothingness*. In the forthcoming second volume, he promises an analysis of the post-war Sartre and his increasing involvement with history and Marxism. While one cannot at this time evaluate the success of Mészáros' overall project, one can see in the first volume the structure of his understanding of Sartre and comment on his ability to illuminate the movement of Sartre's career.

The central unity of Sartre's work lies in his fundamental project, the attempt to understand the individual through the specificity of the individual's praxis. Sartre's explorations of "the roads to freedom" of the individual, beginning with the self-oriented subjectivity of the early existentialism and moving to the problematic subjectivity of the serialized and exo-conditioned individual of the *Critique* and the universal singular explored in the final works, were always directed to the development of a general theory or method which would make the specific inter-

relations of contingent situations intelligible to other humans. For Mészáros, Sartre, from the very beginning of this quest, is trapped within "hopeless dichotomies" because he has always wanted to assert both unrestricted human freedom and the full weight of facticity (determinism). This antinomy is at the heart of all Sartre's work, according to Mészáros, and is the key to understanding and evaluating it.

In his preliminary discussion of Sartre and the methodological questions implicit in his project of understanding Sartre, Mészáros does little to further our understanding of the extremely elusive, yet fundamentally important, problem of Sartre's relation to this historical situation. Throughout the book, Sartre is treated as a timeless and socially unconditioned thinker who is trying to develop a theory of social conditioning and human freedom. Mészáros' failure to link Sartre and his work to the social and intellectual climate from which he and his work originated leaves us with an abstract view of the Sartrean project and the unity underlying its development. Because of this abstractness, Mészáros, in order to show the overall unity of Sartre's work, must read the earlier work in terms of the problematic of the later work. The result of this failure (which Mészáros shares with most writers on Sartre) is that this study provides us with little in the way of illuminating insights into Sartre and his project as situated in and responding to twentieth-century Europe.

A more fruitful approach, one followed by Aronson and Poster for example, is to investigate the dialectical relationship between his situation and the theoretical limitations of the various positions he has held. By beginning always with consciousness because it is perceived first (a fact Sartre himself explores in his autobiographical work, *Words*), Sartre is inevitably trapped within an ontological individualism which is continually haunted by the absence of the layers of

sociality and collectivity. The changes in Sartre's position are the result of his continuing attempts to overcome the particular manifestations of abstractness brought to his attention primarily through his interiorization of his situation. The development within Sartre's thought is intelligible only through this dialectical interplay of the Sartrean categorical structure and the Sartrean understanding of his experience.

Mészáros' discussion in the second half of this book is a valuable contribution to the analysis of the early work of Sartre even though it also focusses on the central antinomy he finds in Sartre's work between freedom and determinism. Around that central theme, Mészáros provides us with a great deal of insight into the conceptual network Sartre has developed in *Being and Nothingness* and illuminates the unfounded nature of Sartre's self-criticisms of his early work. The central argument, however, claims that the Sartrean use of metaphors, for which *Being and Nothingness* is so famous, is the result of the necessity of holding together the dualism in which Sartre is enmeshed.

The difficulty with this view of the early Sartre is that it is again the product of a reading of the early Sartre from the perspective of his later work and not a reading which emerges from a systematic study of the texts themselves. For the early Sartre, the irreducible freedom of the individual creates not only its own projects but also, through such projects, the nature and contours of its situation. The tension Mészáros finds is only apparent — the real problem of *Being and Nothingness* is the unmediated collapse of the world into the individual's freedom. It is in trying to deal with this problem, revealed to him through his attempt to think the experience of the war and the immediate post-war period using the categories of *Being and Nothingness*, that Sartre creates for himself the tension Mészáros places at the heart of all his work and, in particular, creates the

freedom-facticity dualism which is ultimately at the heart of his *Critique*. What Mészáros has done is to impose the framework of his own second volume on Sartre's early work rather than allow the dialectical development of Sartre's work to create that framework for him.

For the historian, the overall work of Sartre has enormous methodological implications although these become clear only in the later works. In opposition to the positivist human sciences, Sartre champions the freedom of the individual and hence places, at the heart of explanation, the subjective experience of the individual. While in the early work the question of history has no place within his view of radical freedom, later, the attempt to situate historical forces within his ontology of freedom leads him to pose the questions of the *Critique*: how, using subjective experience as a basis, can history be intelligible? How can there be only one history given the myriad of individual praxes?

Mészáros' first volume, even with its insights into the early work, seem primarily aimed at setting up the issues and the framework of the second volume. One hopes that the second volume, which is concerned with the period of Sartre's life during which he openly confronts the issue with which Mészáros is primarily concerned, will clarify not only Sartre's thinking but also the entire problem of understanding the situatedness of the individual within history. If it fulfills its promise, Mészáros will have contributed significantly not only to Sartre's studies, but to the philosophy of the human sciences as well.

J. Alan Lennon

State University of New York at Buffalo

Russell Jacoby, *Dialectic of Defeat: Contours of Western Marxism* (New York: Cambridge University Press 1981).

WHILE NOT QUITE YET a dime a dozen, histories of Marxism are not hard to come

by. From Vranicki to Zanardo or from Perry Anderson to Kolakowski, however, they tend to grind well-worn axes through relatively orthodox theoretical mills. Dubious constructions of ideal Marxisms are usually dragged through a 100 or so years of political tribulations either to a concocted happy ending of a fundamentally sound Marxist theory precipitating — or promising to precipitate — a proletarian revolution through the good offices of some mythological Leninist party (Anderson), or to the unavoidable succumbing to insurmountable internal difficulties resulting in precisely the opposite of what was initially promised (Kolakowski). What is missed by grand reconstructions of this type, which tend to overemphasize winners or otherwise "successful" exponents, is the very historical fabric whereby theory is translated into a new social reality through the sufferings and failures of tragic agents whose bitter fate consigns them to obscurity. The result is ideology. A fraudulent theory or a trajectory fabricated after the facts normally occlude a much richer historical terrain. Jacoby's new book is an attempt to remedy this state of affairs. As such, it performs a useful task. In focusing on "the defeated," it not only provides a much fuller than usual picture of what went on, but also explains why things here turned out the way they have.

*Dialectic of Defeat* does not mince words: the defeat of the dialectic is due first and foremost to precisely that orthodox Marxism whose success is drenched in the blood of countless victims and the subversion of every ideal or emancipatory hope the theory originally held. Unlike most standard reconstructions of Marxism which either thrive on an ambiguous attitude towards presently defective — but presumably redeemable — "really existing socialist regimes," or end up outrightly apologizing for them pending the cleansing of few remaining internal imperfections, Jacoby is frighteningly honest on the subject. The regime in the USSR and its sad caricatures around

its borders are criminal and the only emancipatory thing about them has to do with their elimination or transformation. Matching — if not surpassing — the Nazis in the extent of their crimes, these regimes are shown to be propped up by a scientistic Marxism whose absurdity and pretense cannot tolerate competitors. Hence, Western Marxism and its exponents have been its first victims.

Given this scenario, it is not surprising that the leading chapter on "Conformist Marxism" opens with a devastating critique of Althusserianism — a modern version of Stalinist apology whose internal incoherence betrays its ulterior motives. In a peculiar sense, however, this attack has more political relevance in England and some parts of Canada than in the United States, precisely because of the theoretical preventive spade-work that people such as Jacoby have been successfully engaged in for the past 10 to 15 years. As a result, with the possible exception of some marginal literary circles notorious for their intellectual looseness, Althusser and scientistic Marxism have never significantly penetrated American radical circles. The only surprises generated by *For Marx* and *Reading Capital* was the rapidity with which they were remaindered, while lesser dull epigones such as Poulantzas and the *New Left Review* Trotskyists have remained primarily continental and British oddities. This explains why Althusser is ridiculed and summarily dismissed in less than a dozen pages here. His inclusion is as the most outstanding example of that Conformist Marxism which, blinded by its success, recreates all the worst features of the capitalism it pretends to challenge. The most important task, however, is the explanation of the derivation of this scientistic Marxism and its complicity in destroying — often physically — its Western counterpart.

In so doing, Jacoby traces the theoretical red-thread of both Western and Soviet Marxism back to the subterranean Hegelian heritage, and their relation to Engels.

The split between Right and Left Hegelians that ensued after Hegels' death is seen by Jacoby as recreated within divergent Marxist traditions as, respectively, a scientific and an historical Hegelian tradition resulting, eventually, in Western and Soviet Marxism. In reconstructing the contours of their peculiar history, he provides a wealth of information about forgotten and/or repressed Marxist debates which makes the book a necessary reference source for anyone dealing with the periods in question. The main point, however, is fairly simple. The outcome of these debates was decided by the authority derived from the political success of the Bolsheviks rather than the respective merits of the various positions. The dialectic of defeat requires that the spell of success outranks any other consideration. Western Marxism in the 1920s was then forced out of center stage by a double-barrelled shotgun loaded with Nazi and Stalinist bullets.

Unlike Gouldner's parallel analysis in *The Two Marxisms* where "critical" and "scientific" Marxism are presented as Siamese twins — the critical side often being the conscience of its scientific counterpart — only to end up as ideologies of the "new class," in the final chapter appropriately entitled "Nightmare Marxism," Jacoby closes with the question of "Class Unconsciousness." More concerned with the emancipatory potential of Marxism than with Gouldner's emphasis on the social role of theory, Jacoby closes with a call to reconstitute radical theory qualitatively in order to salvage its emancipatory spirit. The vindication of Western Marxism thus leads to the ironic conclusion that its theoretical carcass be given a decent burial. The exposé of its suppression through well-known administrative measures does not result in the resuscitation of a viable theory pointing the way towards emancipation, but of an outlook which, independently of its bitter fate, was terminally ill nonetheless. The point of the *Dialectic of Defeat* is that the defeat

of the dialectic is an irreversible fact.

While careful scrutiny of particular accounts by specialists may turn up oversights — as in the reconstruction of Antonio Labriola's contribution to Marxism (42-9) where Jacoby misses the main thrust of the debate with Croce concerning the Marxist philosophy of history — the analyses are first-rate. They put an end, once and for all, to much claptrap about histories of Marxism seeking to buttress crypto-Stalinist attempts to present an uninterrupted continuity and a unified theoretical framework.

Paul Piccone

Memorial University of Newfoundland

Horace B. Davis, *Toward a Marxist Theory of Nationalism* (New York: Monthly Review Press 1978).

HORACE DAVIS'S new book, a sequel to his earlier *Nationalism and Socialism: Marxist and Labor Theories of Nationalism to 1917*, takes issue with the proposition that has dominated the mainstream of Marxist thought for a century, that nationalism is the by-path of folly, leading the forces of proletarian internationalism away from their historic mission of class conflict and socialist revolution. To Davis it is a self-evident truth of contemporary history that there exists no such thing as an international working class, while monopoly capitalism is subject to the deepest threats not from the highly developed and organized working classes of the imperial centres, but by socialist-oriented movements of national liberation in peripheral countries that frequently do not even possess a proletariat.

A recurrent theme in Davis's analysis flows from Sun Yat-sen's dictum that there exist three great movements in modern history: socialism, democracy, and nationalism. While Davis does not explore the question of a necessary link between the first two of these, he is emphatic that there exists no necessary link between, or chasm

separating, nationalism and either socialism or democracy. Nationalist movements have sometimes contributed to the development of socialism, but so have socialist movements sometimes led to the development of nationalism. Nationalism has sometimes furnished the indispensable driving force for socialism; sometimes, nationalism has lain at the basis of socialism's defeat. Equally, while socialism may hold the ultimate hope for true national liberation, socialist countries have upon occasion frustrated and defeated the drive of nationalities for independence and self-realization.

There are good nationalisms and bad ones. Good nationalisms are movements directed against the exploitation of whole peoples; bad ones are inherently exploitative of others. While the future of humanity does not stand or fall with the fortunes of any particular nation, and while the sole basis for moral law must rest not in the nation but in humanity as a whole, Marxist theory must view the nation as an authentic social category analogous to the concept of class. Davis cites as a valuable contribution to theory the notion developed by the Yugoslav communists that national oppression is a Marxist category related to class oppression but not identical with or subordinate to it.

Davis' definition of the nation, as perhaps all definitions of the term are in a socio-historical vacuum, is somewhat problematic. Nations, he says, share four determining characteristics: all nations have a specified territory, a certain minimum size, some integration (centralization, interdependence), and national self-consciousness. The first of these begs the question of a national diaspora, the best example, although by no means the only one, being that of the Jews. Davis sometimes seems to indicate that local Jewish communities, for example in Poland, have been "national," while elsewhere world Jewry is portrayed as being at least something very like a nation; more so, Davis suggests, following the creation of the

state of Israel than before. The second characteristic, a minimum size, does not appear to mean very much in the absence of criteria for deciding what that size might be: as it stands, it could presumably be used to deny the national claims of certain minority peoples who fit the other criteria. (Davis elsewhere distinguishes between nations and tribes by arguing that the former are based on territory, the latter on kinship.) The third criterion seems somewhat questionable in the circumstances of colonialism: what if the centralization and interdependence are imposed from outside, particularly in a way that destroys indigenous social organization? Finally, the emphasis on consciousness is clearly necessary to a definition of nationalist movements, but if nationalist movements can spur the development of nationalist consciousness, as Davis seems to argue, what is the nature of the social group before it attains that self-consciousness?

A substantial chapter reviews the Marxist theory of nationalism as developed by Lenin, Luxemburg, Stalin, and Trotsky, emphasizing the first three: Trotsky, Davis argues against recent adherents of the *Fourth International*, was no champion of nationalism. Classical Marxist theory is shown to have been seriously wanting in its analysis of nationalism, and in a deeply interesting chapter on the national question in the Soviet Union Davis provides grounds for his view that anti-Russian nationalism is on the upswing, that the Soviet communist party has not responded as thoroughly as it must to the claims of the nationalities, and that "a collision course is indicated." This is followed by a fairly detailed case-study of the Yugoslav situation, stressing the economic frictions that may translate into nationalist agitation in a largely bi-national federation. Much of what Davis has to say here sounds terribly familiar to anyone who has followed federal-provincial relations in Canada, notwithstanding that much of what is said seems to parallel more closely Alberta-Ottawa relations than

those of Quebec and the central government.

Next comes an exceedingly cursory review of the place of nationalism in the Chinese revolution, adding little to previous discussion of this topic. The chapter on Latin America is fuller. Davis argues that the Brazilian left's strategy of support for a national bourgeoisie was doomed to failure in the absence of a national bourgeoisie; that nationalism provided the pivotal motive force for the Cuban revolution; and that the explanation for the failure of Guevara's Bolivian campaign lies not in the hostility of Bolivian nationalists to Cuban leadership but in the presence of the CIA. Davis concludes his discussion of the bearing of the Latin American experience on the theory of nationalism by arguing that the progressive equation of nationalism with anti-imperialism there casts doubts on the validity of Lenin's notions about the class basis of nationalism. Nationalism may sometimes be a bourgeois movement, but it may equally sometimes be a movement of other classes against a bourgeoisie, and even a broad social movement that cuts across class lines.

The penultimate chapter views class and national formation in Africa through the prism of Fanon and Cabral. Davis points to the apparent paradox of revolutionary socialism and nationalism appearing where there is neither a revolutionary proletariat nor a progressive bourgeoisie. The African experience is also used to demonstrate again the non-existence of international labour solidarity.

There is much in this book that is informative and engaging. One reads it, however, with the feeling that on balance, Davis is engaged in the rather unrewarding task of trying to convert the vulgar Marxists of the sectarian left to a position the rest of us have long made our own: that nationalism is an independent force to be reckoned with in the modern world, capable of harnessing populations to seek freedom from exploitation and oppression, and

capable as well of exploitations and oppressions on its own account. And the book is all but silent on a number of matters one would particularly like to see addressed: there is no discussion of the bearing of nationalism in the developed capitalist countries beyond the equation of nationalism at the centre with imperialism, but this is hardly helpful when one considers the Basques, the Northern Irish both protestant and catholic, the Québécois and even (dare it be said?) the English-speaking Canadians. For all that it clarifies some questions, the book obscures others: for all that it attempts, "in the spirit of Lenin," to do, there remains much to be done.

Paul Craven  
York University

Gary P. Freeman, *Immigrant Labor and Racial Conflict in Industrial Societies* (Princeton: Princeton University Press 1979), and Sasha G. Lewis, *Slave Trade Today* (Boston: Beacon Press 1979).

**RACISM HAS NEVER** been very far below the surface in the white societies of Western Europe and North America. Nevertheless, the recent resurgence of open racism — the spread of the Ku Klux Klan to Ontario, the official Republican candidacy of a former Nazi Party and Klan member on a white supremacy platform in the 1980 Congressional elections, the introduction of a new, racist citizenship and immigration bill by the Conservative Thatcher government in Britain — is sobering evidence that the social liberalism of the civil rights era is giving way to a nativist conservatism in which racism has once again become respectable. The reason is not hard to find. The current economic crisis in the western world with its attendant unemployment and downward pressure on the real wages of workers has led to the search for scapegoats. How easy it is to find one in the darker skins of immigrants, legal and illegal — so visible, so obviously alien.

Yet there is a curious contradiction between this common reaction of so many workers and the needs of modern capitalism, needs which if not satisfied will undermine the very basis on which the relatively comfortable status of the majority of white workers rests — economic imperialism of the underdeveloped world and its peoples.

These are the central issues taken up, in very different ways, in these two recent books. Freeman's work, a rather dry, academic treatment as perhaps befits a published thesis, is a comparative analysis of French and British immigration policy in the post-war period. Lewis, on the other hand, is a *journalist writing with considerable passion* of the degradation of the trade in illegal immigrants, primarily Hispanic and Mexican, into the United States. Yet from both viewpoints and approaches, a similar theme emerges: low wage immigrants are a necessary ingredient in capitalist prosperity, but the social strains created by large scale "coloured" immigration threaten the very fabric of capitalist society. Employers use race to "divide and conquer" the working class but the racism engendered threatens the supply of immigrant labour.

Freeman establishes this theme in his introduction and suggests that the inherent contradiction in immigration "which makes it both a cause of and an answer to the crises of advanced societies makes a simple, straightforward analysis of policy impossible and explains, perhaps, the fumbling manner in which it has been handled by British and French officials." (10) The bulk of his study, however, is a narrative account of the evolution of British and French policy which, although in many ways similar in that both were constrained by economic goals, nevertheless differed, because of different perceptions of economic goals, because of different racist attitudes, and because of different political institutions.

The French, he argues, considered France underpopulated and facing a labour

shortage after World War II and therefore entered into controlled arrangements with less developed countries to import labour. Because immigration was part of the country's technical planning mechanism (and one might note, as a result of specific provisions to house and service immigrants who were not accorded citizen rights), the public was not drawn into policy discussion and the government was able to buy time "during which it extracted enormous profit from immigrants." (317)

Preference throughout the period was for "white" immigrants from southern Europe who could be culturally assimilated, but, as the common market expanded and prospered, increasing reliance had to be placed on African sources. As economic problems began to arise in the late 1960s and the visibility of unassimilated non-European immigrants rose, racial tensions also increased, ultimately leading to the suspension of most new immigration in 1974 and a substantial tax on employers who imported semi-skilled labour. At the same time programs were introduced to ameliorate social tensions.

In contrast, the British considered their country overpopulated and, as a legacy of their colonial policy, saw no way to control "commonwealth" (a euphemism for coloured) immigration, particularly since they had no manpower policy or economic planning mechanism. Rather, the role of the state was to mediate conflict which arose because immigrants were not separate, or of inferior status, and because of the racism inherent in the British belief, also a colonial legacy, in the "greatness of British national character." Also, unlike France, Britain lacked a Marxist party that would interpret the problems with immigration within a class context. Afraid of the violence of American race relations, the British resorted increasingly to attempts to legislate harmony and to restrict "commonwealth" immigration, culminating in the 1971 Immigration Act that placed "commonwealth" immigrants in the same

category as aliens, allowed to work only on temporary work permits.

Lewis, unlike Freeman, is not particularly concerned with the historical evolution of American immigration policy toward Hispanic America. Rather, she puts a human face on the immigrant experience, but it is a human face disfigured by pain, deception, exploitation, and death, exposing and documenting the inhuman reality of the trade in illegal immigrant labour and the extent to which all the participants, including the American Immigration and Naturalization Service, law enforcement agencies, employers, and those involved in the trade, have been degraded by the pursuit of profit. It is a story that does not compare particularly favourably even to the original African slave trade.

While most of the book concentrates on the realities of the trade perhaps the most interesting part of the study for the student of labour is her analysis of the economics of the contemporary "slave trade." Who gains and who loses? The obvious gainers, other than the traffickers themselves, are the employers who can pay very low wages, even below legal minimums, or who can avoid paying payroll taxes or other legislative benefits. After all, being illegal, the immigrants can hardly complain to authorities or form a union (as Lewis puts it, they take no civil rights with them to the job). Less obvious gainers, she argues, are the governments of Mexico and the other emigrant countries for whom the immigration is a safety valve against rebellion or revolution. But perhaps the biggest winner is the American consumer. Lewis quotes an investigatory committee appointed by President Ford: "The prices American consumers pay for labour-intensive products are kept down by the presence of exploited, unskilled illegal workers (and the more of them the lower the prices.)" (154) Canadians, being net importers of this type of agricultural produce also gain from this exploitation.

Lewis also argues that the illegal

immigrants are net contributors to the public sector in the United States, contributing more in taxes than they receive back in benefits. For the American (white) worker, he is ensured at least first chance at the better paying, more desirable jobs while relieved of filling the survival "jobs that require no skills but obedience: jobs that develop no skills." (161) Freeman makes the same observation, in a more general context, for Britain and France.

Who then are the losers? The would-be immigrants who die under the relentless desert sun or of suffocation in the trunk of a car as they try to cross the border: the ones who are apprehended after paying their life savings to a trafficker but who never make it; the ones who make it but who are turned over to the immigration service by their upright employer, the day *before* payday. Lewis points out how small a proportion of the illegal immigrants actually make significant gains from their perilous journey. Yet it is one ray of hope against the hopelessness of their poverty at home.

The other major losers in the United States are the lower strata of the legal labour force, particularly blacks and hispanics, who are displaced from those "dead-end, minimum-wage jobs that Americans take when they can get them and hold onto only until they think they can find something better," (161) but who are displaced nevertheless. The result is inter-racial tension and violence among the non-white community.

Lewis only begins to take the analysis one step further, to the role of American capitalist imperialism in perpetuating the poverty in the underdeveloped countries that provides the push of illegal immigration and in perpetuating the secondary labour market in the United States that provides the pull. Both are manifestations of the exploitation of unequal exchange. Under the reign of the multinational corporation, jobs that can be designed to use low-skilled workers and that can be exported to "safe" low wage countries — Hong Kong, Taiwan, Brazil, Chile, Singa-



pore, the Philippines — are exported. For those jobs that can not — restaurant help, fruit and vegetable pickers, hotel maids — low wage workers must be imported because American (and West European) workers refuse to work at onerous jobs for bare survival wages. There are few barriers to the export of multinational capital to the poor countries, but there are barriers to the free import of labour, particularly "coloured" labour. These are immigration laws, a response to the racial and social problems engendered by immigration and the resulting legitimate opposition from labour which sees immigration as feeding a permanent reserve army of unemployed, undermining and undercutting wage rates. The problem, as with the drug traffic, is when the exporting countries, the customers in the importing countries, and the traffickers, all "benefit," policing becomes not only extremely difficult, but avoidance also very profitable, to the degradation of the trade.

What is the answer? Freeman suggests that the European countries will resort to the German "guest worker" system which allows systematic and explicit exploitation of foreign workers whenever required by the state of the domestic economy. But as he notes, even this system is not without its problems. He hints that only a socialist transformation can provide any long term solution. Lewis is caught in a dilemma. She shows that the only hope to end the push of immigrants is social revolution at home. She also knows that the United States would never permit such revolutions, citing the case of Chile, (184) She also recognizes that the pull will remain so long as it is profitable for employers to exploit low wage workers, which means as long as capitalism remains dominant. As a result, her only answer is a plea to humanize the treatment of immigrants. Given the present employment crisis and rising racism, it is a plea that is likely to fall on deaf ears.

Paul Phillips  
University of Manitoba

Michael J. Piore. *Birds of Passage, Migrant Labour and Industrial Societies* (N. Y.: Cambridge University Press 1979).

BOOKS BY ACADEMIC economists are usually narrowly focussed, crammed with mathematical formulae, replete with statistical tables, and written in a style of English which might be called, gently, "modern scholastic." Michael J. Piore's most recent book is none of these things. Rather it is a wide-ranging and well-written narrative with only a dozen tables and not a single mathematical formula. Hopefully these defects will not prevent its attracting a wide readership, as it is the sort of book which many economists ought to read. Piore presents a suggestive analysis of a pressing social problem and his policy proposals are novel and far reaching in their implications. It is therefore to be hoped that those who cannot write or speculate will now address themselves to gathering the data which will prove whether or not he is right.

Piore starts from the viewpoint that the labour market of advanced capitalist nations can be divided into a segment which contains jobs with good pay, pleasant working conditions, prospects for advancement, fringe benefits, and formalized authority relationships, and a sector (the dual labour market) which offers the reverse — insecure, low-paying jobs with little prospect of advancement, poor working conditions, and informal, personalistic work relationships. The 10 per cent of the western European labour force which is composed of legal migrant workers and the unknown percentage of the U.S. labour force which is composed of illegal aliens are destined for the dual labour market. Piore argues that since spontaneous migration flows are relatively weak, the active agent generating migration flows is the evolution of developed countries. Individual employers and government recruiting agencies have a virtually inexhaustible supply of labour to tap from third world countries, labour which

can fill very specific needs in advanced economies. Piore sees the roots of the dualism in the job structure of advanced industrial nations as the "flux and uncertainty that inheres in all economic activity." Only part of the aggregate demand of advanced economies is stable and predictable and can generate stable, long term employment. In capitalist economies, cyclical fluctuations, changes in fashions and technology, seasonal variability, and a host of other influences generate an unavoidable instability in production and, therefore, in demand for labour. In a "pure" capitalistic system, all labour is "at risk" in this process, but Piore argues that advanced industrial nations are only "quasi-capitalist" in that native labour has, by and large, managed to insulate itself from this insecurity (through its struggles for the seniority system in the United States and redundancy pay and employment guarantees in Europe). Pools of malleable migrant labour therefore allow the system to cope with its inevitable instability and to fill its menial jobs while offering stable employment and relatively good jobs to most of its native sons.

Why do migrants accept their place in such a scheme of things? Piore argues convincingly that there is normally much more to work than money. Work roles define social roles and status rankings. They situate their occupants in society and they tie us to particular social groups. Piore argues that it is precisely because most migrants initially view their move as temporary that they can function as pure economic men and view work as a purely instrumental means to gain income. Income is desired so that wealth can be accumulated which migrants can take back to their home community in order to enhance their roles within *that* social structure. Migrants are therefore willing to do work that they would find demeaning, and to live under conditions that they would find intolerable, if they had to do so in their home communities. Frequent visits home enable them both to maintain a psychologi-

cal distance from the work which they do in the developed nations and to ride out temporary downturns in labour demand. The contradiction in this process is the growth of community among migrant workers in the advanced nations. The growth of social ties, assimilation, and long term residency create, over time, permanent inhabitants rather than temporary sojourners. Their descendants then aspire to upward social mobility *within* the advanced nations and become unwilling to accept the menial jobs of their parents.

Migrant workers are, however, not alone in their instrumental, short run attachment to the labour force. Peasant workers who work to supplement an agricultural income, housewives whose self-definition is in terms of traditional familial roles, and students, who are only in it for the summer, can maintain a similar psychological distance from dead-end jobs and tolerate (indeed desire) their instability. Migrant workers, however, present significant advantages to employers. They are adaptable, much more mobile geographically, and much more susceptible to manipulation and control. Piore argues "the economics of wages for migrant workers is by and large the economics of the minimum wage." (93) Piore argues that migrants are "target earners," meaning that they work only long enough to acquire a specific nestegg, and then intend to return home. The consequence of paying migrants more, therefore, is that they would work less, since they would need less time to attain their targets. This is one of the few points in this otherwise convincing narrative at which I, personally, pause in disbelief — Piore seems to have taken at face value the analyses of the employers of migrant labour and offers no other supporting evidence.

What is the impact of these migration flows on the home regions? Piore argues that migrants do not return with heightened skills and improved work habits — rather the opposite. Typically, their social distance from skilled native workers and the

sort of jobs which they fill prevent the acquisition of sophisticated work skills. Moreover they do not *want* to do the same sort of jobs at home that they were willing to endure abroad. Piore speaks of the strong "resistance to dependent status" among returning migrants. Because of this resistance to a wage-labour status they typically aspire to purchase land or to become small businessmen, usually in the service or commercial sector. In the former case, returning migrants may simply bid up the price of agricultural land. In the latter, they may establish the sort of small enterprise about whose importance in the development process far too little is known. Conceivably, the capital they bring to these businesses from the advanced nations and the management techniques which they have assimilated may hasten the development process. Whether this acceleration of development can outpace the heightened aspirations they also bring with them on their return is a moot question.

Piore is also ambiguous in his evaluation of the impact of migrant labour on advanced nations. When migrant workers are restricted to unstable dead-end jobs they are complementary, rather than competitive, to most adult male native labour. Temporary migrants pay taxes, unemployment insurance premiums, and social security contributions but make relatively few demands on social security systems — especially if they are "illegal." Their presence therefore confers significant benefits on both employers and many adult male native workers. There is thus little opposition to temporary sojourners, but much more opposition when their children begin to aspire to upward mobility. Piore seems to feel that a dual labour market is inevitable. Hence he argues that the aim of public policy should be to "maximize the temporary character of the migrants' stay in the urban industrial countries and capitalize upon the natural propensity of such populations to return home." (171) As a liberal, he is uneasy with a policy which finds its fullest expression in South Africa. To the

extent that migrant populations develop permanent settlements, Piore therefore argues that one should "encourage the second generation to acquire whatever traits (training, education, credentials, and the like) are requisite for upward mobility." (172)

In summary, Piore has written a stimulating and suggestive book. His discussion is wide-ranging and provocative. On the class composition of migration streams, on the relationship of the *de facto* U.S. immigration scheme to *de jure* mechanisms, on the internal migration flows of the U.S. and on much else he tells a very believable story. Hopefully, duller, more academic books will examine these issues in greater depth.

Lars Osberg  
Dalhousie University

Gordon Swanson and Jon Michaelson, eds., *Manpower Research and Labour Economics* (Beverly Hills: Sage Publications 1979); and Michael J. Piore, ed., *Unemployment and Inflation: Institutional and Structuralist Views* (New York: M.E. Sharpe 1979).

THE VOLUMES UNDER review both lack two elements of relevance and interest to students of Canadian labour history. They are neither Canadian, nor are they concerned primarily with history. They are, however, concerned with labour. They are edited books on labour economics, and are American in authorship and subject matter. Each was written to address certain theoretical, methodological, and policy issues within current labour economics literature, and as such may not be of great interest to labour historians. However, there is value in interdisciplinary cross-fertilization and literacy, and from this point of view both books are useful resources. They are written in non-technical language, easily understandable to a non-economist.

The Piore book addresses general

issues of aggregate unemployment and inflation using a theory of the labour market which differs radically from the mainstream economics theory underlying current national policy in both Canada and the United States. The Swanson/Michaelson book is a series of articles evaluating labour economics research funded by the Office of Manpower Research and Development (OMRD) of the U.S. Labour Department and is concerned primarily with the labour market problems of various groups, and with the relationship of research to manpower policy. Between the two books the lay reader is provided with a fair summary of the orthodox, the Institutional, and the dual labour market theories within labour economics. Recent Marxist work in labour economics is not discussed, although it may be more familiar to labour historians than is the work represented in these volumes.

The articles in the Swanson/Michaelson book are by prominent labour economists commissioned to "consider the state of knowledge in the field and its relationship to policy and to inquiry," (7) as background papers for a review of the effort of the Office of Manpower Research and Development since 1962. Each of the seven articles tries to provide a framework for assessing the research and in doing so gives a summary and viewpoint on theoretical debates and policy issues within labour economics. The authors range from the current U.S. Secretary of Labour, Ray Marshall, to Michael Piore, editor of the second review book, and a maverick in labour economics. Thus, the full spectrum of non-Marxist views is presented. The articles do not evaluate individual research products, but they do provide a sort of annotated bibliography of the work sponsored by OMRD.

The OMRD has been a major source of academic funding, with more than \$350 million invested in research over the 15 year period. As Piore points out in his article, there has been a remarkable balance, politically as well as topically, in the

research funded, and much of the emerging labour market segmentation theory, including Marxist analysis, was developed under OMRD funding. Piore suggests that the balance of academic, political, and program experience forces operating on the OMRD enabled greater academic freedom to the researchers than the discipline itself was affording. Furthermore, the research was unfettered by the ideological thrust of the manpower programs themselves. The assessors seem to agree that the program has been useful to the progress of academic labour economics, though not directly useful to the sponsoring agency or to the formulation of government policies. The discussion touches on most aspects of employment and wage theory, with specific emphasis on problems of poverty, discrimination and unemployment. The quality of the articles is uneven, and most read like hastily written, balanced overviews aiming to more or less please the agency funding the evaluation.

*Unemployment and Inflation* is a more focused, coherent set of articles written within one labour economics framework. This framework is the Institutional school, of which dual labour market theory is the modern outgrowth. The Institutional work of 1950s and 1960s rejected the rarefied neo-classical theory of wages and unemployment, known as marginal productivity theory. Their work emphasized non-market forces and institutions, and they focused on wage differentials, wage rigidities, nagging unemployment, and other issues which were antithetical to the neo-classical framework. Readers of *Labour* who are familiar with current work on segmented labour markets will know that the theory has a Marxist variant, represented by the work of David Gordon, Richard Edwards, and Andrew Friedman, and an institutional variant, represented by Piore and Peter Doeringer, among others. Piore, in the introduction, is careful to distinguish his book from both orthodox and Marxist theories, although he claims closer affinity to the Marxist camp. As with other

careful liberal work, there are insights and evidence of great interest to the left where a Marxist interpretation can be readily placed on the findings. The villain in the piece is mainstream economics, and the contribution of the book is to derive implications for the macro-economic issues of inflation and unemployment from the dual labour market theory of labour market structure. It follows from this theory that unemployment and inflation are two unrelated processes. Therefore, if labour markets are indeed segmented, then not only is neo-classical labour economics wrong, but the mainstream analysis of the macro-economy is also wrong. To a non-economist this may sound trivial, but mainstream economics has succeeded in keeping each attack isolated to one small corner of the edifice, refusing to accept the broader implications.

In developing the argument, the book presents a valuable collection of Institutional readings from the 1940s and 1950s. These are classic articles, yet many of the insights have been lost or buried and labour economists today are busy rediscovering the same truths. Further readings are by modern dual labour market authors, including Piore, Ben Harrison, Charles Sabel, and Paul Osterman. Piore's five essays range over the whole topic, weaving the other articles together, dropping insights, and flaunting neo-classical standards of scientific proof. The latter is left to his more pedestrian colleagues. To a Marxist he is a frustration, for he just misses a historical, class analysis. For example, his theory derives from the central role uncertainty and instability play in the economy, but the contradictions are never integrated in an explicit model of capital accumulation. In terms of particular topics, this volume contains useful articles on immigrant workers, the youth labour market, the role of minimum-wage, bargaining patterns, and the origins of the present inflationary spiral.

In conclusion, the two books provide useful summaries and samplings of the

state of the art in non-Marxist labour economics. They also inadvertently reveal the limitations of this field in terms of the questions asked and the illuminations provided. For overviews of the omitted radical labour economics, the reader is referred to works by Gordon, Edwards, Reich and Friedman.<sup>1</sup> From an economist's point of view it would be useful to have labour historians contribute their evidence to the radical/institutional/neo-classical debates in labour economics on the nature of the labour process over time. Literacy in each other's fields might enable such an alliance.

Martha MacDonald  
Dalhousie University

Klaus von Beyme, *Challenge to Power: Trade Unions and Industrial Relations in Capitalist Countries* (Beverly Hills: Sage Publications 1980); Tom R. Burns, Lars Erik Karlsson and Veljko Rus, eds., *Work and Power: The Liberation of Work and the Control of Political Power* (Beverly Hills: Sage Publications 1979); and J. Du Edelstein and Malcolm Warner, *Competitive Union Democracy: Organisation and Opposition in British and American Unions* (New Brunswick, N.J.: Transaction Books 1979).

TRADE UNIONS TODAY apparently bear little outward resemblance to the first combinations of workingmen formed to defend and promote the interests of workers in the labour-capital relationship. Changes in the composition and condition of the workers they represent, their own growth into

<sup>1</sup> Richard Edwards, Michael Reich, and David Gordon, eds., *Labor Market Segmentation* (Lexington: D.C. Heath 1975); Richard Edwards, *Contested Terrain* (New York: Basic Books 1979); David M. Gordon, *Theories of Poverty and Underemployment* (Lexington: D.C. Heath 1972); and Andrew Friedman, *Industry and Labour: Class Struggle at Work and Monopoly Capitalism* (London: Macmillan 1977).

organizations of sometimes considerable size and complexity, and the formalization of the relationship between trade unions, employers, political parties, and governments, all suggest a significant transformation in their character. Unions, however, remain the indigenous organizations of the working class and fundamental questions concerning their role and nature remain unresolved.

The books under review here have the implicit or acknowledged orientations which place them within the realm of Marxist debates concerning trade unions. Three main foci of these debates are: the extent to which trade unions compromise or promote the interests of the working class under capitalism; their potential as vehicles for the transition to socialism; and the role of unions in a socialist society. The ancillary questions to these debates are virtually endless in number. Nevertheless it is useful to note a few of them here: What factors determine the character of trade unions? Under what conditions does industrial militancy develop? What part does trade union activity play in the development of class consciousness amongst workers? How are unions used as organs of regulation of the working class and to what extent does this generate conflict itself? What demands by unions can be considered a challenge to, rather than accommodation with, the capitalist order?

It must be noted that changing conditions and events add new dimensions to these debates. In some countries the primacy of direct union-employer bargaining over the price of labour-power appears to be eclipsed at least partially by the increasing importance of forms of worker participation in management, share ownership, and profit sharing at the enterprise and industry level, and of union consultation and decision-sharing at the political level. Notwithstanding these developments, industrial militancy over wage demands remains the *enfant terrible* of advanced capitalism. While none of the three books address this most perplexing issue, they do

make contributions to our understanding of trade unions in contemporary conditions.

The problem of the conditions under which union leadership will be representative of and accountable to the rank and file is of increasing importance as unions become involved in the diverse forms of industrial democracy and national economic planning. For Edelstein and Warner this becomes the question of the extent to which a trade union constitutes a democratic system which they define as "a decision-making system in which the membership actively participates, directly and indirectly through its representatives, in the making and implementation of policy and in the selection of officials, for all levels, on the basis of political equality and majority rule. Furthermore, the system operates on the basis of accountability of officials, the legitimization of opposition, and a due process for the protection of the rights of individuals and minorities." (30) They choose as their indicator of union democracy the existence of successfully contested or close leadership elections which they suggest signifies the lack of oligarchic power and leadership and *ipso facto* the existence of the other elements of democracy.

*Comparative Union Democracy* is essentially a study of what constitutes a democratic electoral process and the organizational factors which facilitate this in unions. The authors present a model for effective electoral opposition in unions as consisting of close competition among full time officers of relatively equal rank, based on organizational subdivisions of relatively equal numbers, resolved through a voting system which does not favour the administration, and with the power of the administration checked by autonomous power centres and a judicial process outside the administration's control. Their empirical analysis of the relationship between structural and procedural organizational variables in 51 American and 31 British national trade unions is complemented by in-depth case studies of unions

with effective oppositional internal politics from both countries.

As far as their study of union elections is concerned, Edelstein and Warner have been thorough in their identification of the formal organizational characteristics which increase the likelihood of a contest for top office. One must ask, however, whether this in itself is sufficient to ensure that the leadership is responsive to and representative of the interests of the rank and file. One critic of their approach has charged that this is "too narrow because politics is about power, not simply elections: the preoccupation with elections rather than interests which was generally characteristic of American political sociology in the 1960s diverted attention from the fundamental issue of the bases of compliance. Evaluation of the distribution of power within unions, and the effects of recent changes upon that distribution, thus involves examining the interests of different groups within the union, and the outcome of varied attempts to secure compliance despite resistance, not simply the tabulation of election results."<sup>1</sup> Granted, *Comparative Union Democracy* is more than the tabulation of election results, but the distribution of power in their organizational structure which is pertinent to the electoral process cannot be seen as the only important aspect of power within unions. This is especially so, when one has, as Edelstein and Warner claim, a larger interest in the role which democratic unions might play "in the establishment of democratic socialism, through the struggle for industrial democracy." (viii)

This is precisely the question examined in *Work and Power*, a collection of 12 papers from the 1977 International Conference on the Liberation of Work and Politi-

cal Power held in Dubrovnick, Yugoslavia. Included in the papers are assessments of existing schemes of industrial democracy in Sweden, Denmark, Yugoslavia, and selected western European countries, and comments on the limitations on industrial and economic democracy in capitalist and socialist societies. As with most collections, the uneven quality of the papers is frustrating. That problem aside, the papers do raise a wide range of issues and present a variety of viewpoints.

The paper by Baumgartner, Burns, and Seville ("Work, Politics and Social Structuring Under Capitalism") both asks and attempts to answer many of the most pertinent questions surrounding the concept of industrial democracy. They are systematic in their approach: first situating industrial democracy reforms in the context of the failure of other attempts to regulate labour problems in advanced capitalism, then identifying the types of reforms which this has included and the interests which have promoted and benefited from them. Their critical assessment of these reforms points to their dual or contradictory consequences and their limitations as the means to a gradual transition to socialism. Compared to this paper, the others tend to be narrower in focus.

On the problems encountered with the system of self-management in Yugoslavia, Rus ("Limited Effects of Workers' Participation and Political Counter-Power") found that an informal status hierarchy within the enterprise gave greater power to management over the staff and workers' councils and created stratification between the skilled and unskilled workers. Baumgartner, Burns, and Sekulic ("Self-Management, Market, and Political Institutions in Conflict") identified the technical division of labour in the enterprise and market allocation rules of the economy as constraints on the system of self-management.

Concerning industrial democracy reforms in capitalist societies, Batstone ("Systems of Domination, Accommoda-

<sup>1</sup> Roderick Martin, "The Effects of Recent Changes in Industrial Conflict on the Internal Politics of Trade Unions: Britain and Germany," in Colin Crouch and Alessandro Pizzorno, eds., *The Resurgence of Class Conflict in Western Europe Since 1968* (London 1978), II, 103-104.

tion and Industrial Democracy") argues that organizational behaviour, both formal and informal, constrains the options for reform. Abell ("Hierarchy and Democratic Authority") presents a model for an "ideal type" of a rational democratic work organization where democratic principles, rather than Weberian rational-legal ones, govern decision-making. Gerry Hunnius of York University ("On the Nature of Capitalist-Initiated Innovations In the Workplace") argues that innovations in industrial democracy, such as job enrichment schemes, have not altered the system of class power and then poses the question of whether such innovations raise the potential that workers will demand more autonomy, control, and power; in short, will they raise workers' consciousness? Horvat ("Paths of Transition to Workers' Self-Management in the Developed Capitalist Countries") proposes a scenario for the transition to socialism through the gradual expropriation of capitalism through collective profit sharing and the participation by workers in a system of co-determination. Through these and the remaining papers in the collection, the reader is presented with ideas which are at least provocative, even if they leave one with more questions than conclusions.

To anyone dismayed by the facile generalizations concerning the functioning and role of trade unions in capitalist societies which plague much of the literature, Beyme's work will be a welcome respite. *Challenge to Power* is impressive in both its scope and depth. With an eye for gleaming what is both common and unique to unions in major western capitalist countries, Beyme identifies the "essential aspects" of their organizational characteristics, conflict behaviour, and participation in integrative schemes. In this he is thorough in both addressing the main established theories explaining these features of unions and in providing many empirical examples which either support or challenge the same. The richness of Beyme's analysis lies in the way in which

it draws together relationships between aspects of the various levels of analysis: from the micro-level of the union organization to the system of industrial relations, to the economic, political, and ideological macro-systems within which they operate.

Beyme identifies as his major thesis "that the current interpretation of the behavior of unions through organizational details . . . is as inadequate as is the confrontation of unions with their programmes and ideology to deduce either that they are making exaggerated power claims [conservative criticism] or 'betraying the workers' [neo-Marxist criticism]. Union behavior is only explicable in the context of labour relations as a whole and the political system." (6) His conceptualization of the system of labour or industrial relations goes beyond a legalistic focus on the rules governing collective bargaining. He includes both the rules and the ideologies which constitute the system, but unlike the consensus theorists does not assume a balance of power between capital and labour or the lack of challenge to the rules and ideologies by organized labour. Because the industrial relations systems function in social, economic, and political contexts, they, like the unions, are seen in dynamic rather than static terms. Hence, for Beyme the differences and similarities in the patterns of industrial conflict between countries are explicable only through the consideration of many factors.

Beyme recognizes the limitation of his approach: "A complex study like this, which aims to represent all the essential aspects of labour relations and identify the causal relations between individual factors in the macro-system instead of concentrating on one problem in detail, cannot offer a general theory of labour relations." (5) But this does not hinder him from arriving at some general conclusions. On the one hand we have the necessity of appreciating the specificity of the way in which unions in each country have developed, but on the other hand we have the existence of general trends in this development. As



Beyme's study progresses from an analysis of the internal structure of unions to their external relations, so does his identification of these trends:

1. the tendency for unions to unify ideologically and to integrate workers previously outside the traditional labour force;
2. the establishment of the precedence of industrial unionism and the centralization into larger units of organization;
3. a rivalry between unions and works councils and between union representatives and those from the rank and file where forms of industrial economic participation have been established;
4. a loosening of ties between unions and labour parties with the concomitant decline in focus on direct parliamentary representation and the new focus on institutionalized forms of extra-parliamentary functional representation;
5. an increase in the public functions of the unions and divisions between unions and labour parties;
6. a trend towards neo-corporatism, especially in northern Europe.

This takes us back to the underlying theme of Beyme's work: that in the power struggle of industrial relations organized labour is gaining strength. He explains that:

The extension of state intervention is altering the balance of power between the organized groups as the state takes over an increasing number of functions. As it wishes to regulate these but cannot administer them alone it gives some competencies and participation rights back to these associations. The mass organizations which can mobilize the greatest mass loyalty, like the unions, are bound to register a growth in political power over the longer term, whether there is a Labour government or not. The employers' associations are furthering this development by demanding a state guarantee for prices and profits developments and by attempting to control wage developments as well, which can only be done with the help of the state through the unions. (330)

Beyme is relatively unconcerned that

this may undermine the autonomy of unions and in fact sees it as a move towards "Socialist co-responsibility." He maintains that as long as the unions remain essentially democratic and do not give up their rights to negotiate collective agreements and strike (in other words maintain their essential functions), there will be no incompatibility of roles. He states that "no union can now embark on a cooperative course unless it maintains its militancy at the same time. This duality of role, when the unions are at once a regulative factor and a counter power, is a condition for the maintenance and achievements of social liberties." (339)

To be fair, Beyme does not suggest that this increase in union power will be unilinear and without setbacks. His projection for the future, however, highlights the essential weakness of his study: the absence of the notion of class interests and class conflict. The unions and employers are presented as interest groups in conflict over the distribution of the fruits of production, but the class relations which assure the accumulation and reproduction of capital are missing. Beyme is correct in identifying the increasing need for union cooperation to keep capitalist economies functioning profitably; he is unconvincing, however, in his argument that this implies a decrease in the employers' power rather than a new mode of accommodation in capitalist class relationships. Indeed, one could just as well argue that the increasing role of self-regulation by the unions only strengthens the economic and political bases of capitalism. Besides his optimism regarding the democratic potential of unions, Beyme provides us with little in his analysis to support the claim that unions can remain an effective counter power while engaged in this cooperative course. While he recognizes that the state is the important third partner in labour-employer relations, he has surprisingly little to say about the nature of this three party relationship. Despite these problems, Beyme's study remains useful for pointing out the

constellation of factors which are important influences on the behaviour of unions.

The unifying theme of these three books lies in their focus on trade unions and power: power within unions, the power which unions exercise at the workplace, and the power of unions at the political level. It is with the different conceptualizations of power that one finds most to disagree with. If the authors fail to resolve the many questions concerning trade unions today, they do evoke a lively response. And with that we can say the debates continue.

Heather McAllister  
University of Toronto

Cary L. Cooper and Enid Mumford, eds. *The Quality of Working Life in Western and Eastern Europe* (Westport, Ct.: Greenwood Press 1979) and Murray Yanowitch, *Soviet Work Attitudes: The Issue of Participation in Management* (White Plains, N.Y.: M.E. Sharpe 1979).

THE FIRST THING to strike the reader of these two volumes is the overriding primacy of economic factors in the introduction of work humanization, quality of working life (QWL), and related innovations. With the possible exception of Yugoslavia, productivity considerations have been the primary reason for the increasing popularity of such innovations, both in Eastern and Western Europe, as well as in Canada and the United States. The introductory comments by Cooper and Mumford, implying a greater concern with productivity issues in Eastern Europe, while Western Europe "appears to view participation and work design as contributing to an improved social situation," are misleading. A recent analysis of contemporary trade journals in the United States for instance illustrates a growing concern with declining productivity among employers. Two general explanations are offered: the inability of business to invest in profitable technological inno-

vations and the belief that workers are not producing enough. QWL, incentive programmes, and similar innovations are seen as *one* alternative to correct this problem — the other approach considered involves a new "war on labour."<sup>1</sup>

The composition of the Cooper/Mumford reader with one contribution from each of nine European countries (Section II & III) presents another problem. Many of these contributions deal with specific case studies or phases in the development of work humanization and participative innovations. The chapters on Norway and Sweden are particularly successful examples. The problem in this approach is that an overall evaluation of such changes cannot be undertaken successfully by viewing these experiments in isolation from general economic and political conditions. In Sweden and Norway, for example, QWL innovations and labour-management relations in general have historically been closely dependent not only on general economic conditions but also on the balance of power between labour and capital. The slow-down in such innovations in Sweden during the past few years is an illustration of this reality. In this connection, I would suggest that it is an oversimplification to suggest, as the editors do, that unions and management share a "common set of values on the kind of society that Scandinavian countries should be trying to achieve."

Since the Cooper/Mumford volume contains frequent references to the United States and Britain it is important to underline the different socio-economic framework of the United States, the United Kingdom, and Canada on the one hand, and many of the West European countries covered in the volume on the other. One way to illustrate this difference

<sup>1</sup> Paul Goldman and Donald R. Van Houten, "Uncertainty, Conflict, and Labor Relations in the Modern Firm: Productivity and Capitalism's 'Human Face'" (Part I & II), *Economic and Industrial Democracy*, 1, 1 & 2 (1980).

is by using the concept of liberal corporatism as a definitional category, particularly for the Scandinavian countries and the Federal Republic of Germany. The struggle between labour and capital in Sweden, for example, can be usefully analyzed in the context of a liberal corporatist model. The increased militancy of the labour movement in terms of new labour legislation, tougher bargaining positions on wages and work environment reforms, coupled with the fact that much of the new legislation has met with strong employer resistance and has yet to be operationalized in the private sector, would seem to indicate a stage in the development of relations between labour and capital which can best be described as "liberal corporatist."

A recent application of the corporatist model to West Germany fits the Swedish and Norwegian models very well. The author argues that while the integration and institutionalization of unions has served the interest of capital, this has been feasible only because it has simultaneously appeared, from the perspective of the labour movement, as a successful strategy representing particular interests of the unions and their membership.<sup>2</sup>

Liberal corporatism reflects the existing balance of power between labour and capital in a particular phase of capitalist development. As Panitch has pointed out, liberal corporatism cannot be reduced to class collaboration, while at the same time, it cannot come about without class collaboration. Geert Hofstede (Chapter 2 in the Cooper/Mumford volume) describes some of the same manifestations using the "power distance and uncertainty avoidance" model without, however, dealing with the changing function of the state in this phase of capitalist development.

Union cooperation/collaboration with

employers in QWL and related innovations in a liberal corporatist context must therefore be seen as being qualitatively different from cooperation in countries like Canada and the United States where the working-class movement is relatively weak in relation to capital. Partial support for this view, in the case of the United Kingdom, can be found in the contribution by Lupton, Tanner, and Schnelle (Chapter 4).

Two of the contributions from Western Europe are written by trade unionists. The radical historical analysis by Matteo Rollier sees union strategy in Italy not simply in the narrow terms of QWL, "but rather as an issue concerned with the overall use to which labour is put inside the factory." The organization of work is seen in the context of an overall union strategy of changing the entire system of capitalist economic development.

Jack Peel makes the important but frequently overlooked point that unions have been struggling for better working conditions long before employers or governments showed any interest in these issues. It is all the more surprising to see another author in the same volume (Geert Hofstede) state that workers have rarely demanded humanization of their jobs. Peel continues by outlining point by point why many unions today seem to be less than enthusiastic in co-operating in QWL innovations.

The chapter by A.T.M. Wilson, while lacking the historical approach taken by Rollier, is nevertheless a useful introduction to the subject. The author recognizes the economic imperatives surrounding QWL innovations. He points out that while most government support for such programmes in Western Europe was initiated during the economic boom of the early 1970s, in no case does it appear to have been cut back in later, more difficult years. This is an important point since earlier "waves" of work humanization and participative management in Europe as well as in Canada and the United States,

<sup>2</sup> Wolfgang Streeck, *Gewerkschaftliche Organisationsprobleme in der Sozialstaatlichen Demokratie*, Dissertation, Frankfurt am Main, 1980.

have often been terminated by employers once the initial economic or political reasons for these initiatives no longer existed.<sup>3</sup>

One partial explanation of this development might be that the reasons which initially compelled governments and employers to initiate or support such innovations have not disappeared. Traditional repressive measures are believed to be counterproductive in most West European nations as we move into the 1980s. A more complete answer must be sought in the context of the changing balance of power between labour and capital in many West European countries. The emerging liberal corporatist framework in many of these countries has given the working-class movement the power to resist attempts to terminate programmes perceived to be advantageous to organized labour.

Geert Hofstede is the only contributor who has no hesitation in describing the Work Humanization movement in terms of the "Third Industrial Revolution." He sees the Human Relations ideology, a forerunner of the present "Work Humanization Revolution," as having only tinkered with the symptoms of worker alienation and argues that an actual shift in power is more evident in the "Humanization of Work Revolution." The evidence for this bold statement is, however, slim.

His scenario for the "Humanization of Work Revolution" rests on four parties: the ruling élite (employers, top management and sometimes union leaders); the revolutionary élite of the "humanizers" (academics, management consultants, some politicians, professionals, junior managers, and some union leaders); the oppressed workers; and others (workers in non-alienating jobs). Hofstede argues that the "humanizers," who are rarely aware that they are a "revolutionary élite," will

need to gain the support of unions and workers if the revolution is to be more than a "palace revolution." Without prejudice to the final outcome of work humanization and related innovations, the above scenario suffers badly from a lack of historical perspective and even more so from a lack of awareness of the role of the state in contemporary capitalist society. In the American and Canadian context the notion of a "Work Humanization Revolution" along the lines described by Hofstede seems ludicrous. Whatever shifts of power between labour and capital have occurred in western Europe or in the United States and Canada, have been the result of successful working-class struggles and there is little evidence to suggest any change in this pattern.

Two of the chapters deal with the still inadequate tools and results of evaluating work reorganization programmes. Anthony Hopwood is concerned with the inadequate assessment of economic costs and benefits of work reorganization programmes and proposes a breakdown into three categories for the purpose of evaluation: the operational (enterprise level); the systemic (the impact of work organization on the long term capacity of the enterprise to adapt to its internal and external environment); and the societal (covering those costs and benefits which are borne or received by society as a whole).

James Taylor offers a useful critique of the present-day state of QWL evaluations. If evaluations imply measurement of the degree to which a given innovation results in the hoped for solutions, then, as Taylor points out, we are faced with the problem that in many instances the intended results have either not been published or not even been stated explicitly. In the American and Canadian context researchers may well face another political problem. Given the adversary relationship between union and employers and the suspicion with which many unions view QWL innovations, stated intentions of such innovations may prove to be mean-

<sup>3</sup> For a good account relating to the United Kingdom see Harvie Ramsay, "Cycles of Control: Worker Participation in Sociological and Historical Perspective," in *Sociology*, 11 (1977).

ingless as a yardstick for accurate measurement.

Section II of the Cooper/Mumford volume offers examples of QWL related innovations in six west European countries. We have already mentioned the contribution on Italy. What is of particular interest in other chapters are the reported case studies. In the case of Norway, Max Elden uses a case study of two banks to illustrate one phase of the Industrial Democracy Programme in Norway. This is a very satisfying phase in a national programme.<sup>4</sup> Similarly, the contribution of Sweden based on material compiled by the Swedish Central Organization of Salaried Employees (TCO) should be of particular interest to trade unions since it involves five case studies which were carried out using local resources and without expert participation.

The case studies as well as the introductory comments in the Dutch and Danish contributions illustrate management-initiated change efforts; in the case of the Dutch case study the innovations met with initial resistance from middle and lower management as well as staff specialists who feared that their tasks were at stake. The contribution from France is largely based on a survey conducted by the author in 1975/76. Initiatives in job redesign come exclusively from top management and are largely oriented to economic goals. Union opposition, particularly at the national level, is almost automatic.

The contribution from eastern Europe (Czechoslovakia, Hungary, and Yugoslavia) are, with the exception of the chapter on Hungary, disappointing. Bogdan Kavcic gives an accurate and useful introduction to the legal and institutional framework within which worker self-

management operates, and defines the terms used to describe the Yugoslav system. This, is however, not sufficient for an understanding of how the system operates in practice. We are left with too many questions as to the actual decision-making process, the distribution of power between the various organs of self-management as well as the role of management, experts, and the League of Communists.

The contribution from Czechoslovakia tells us little about worker participation in that country except to outline models and phases with little content. The impression one is left with, particularly in respect to a case study of an engineering enterprise, is that the approach is essentially top-down, that it was successful in increasing production, and that it involved a limited dose of job enlargement and job enrichment, but the essential details are missing.

The contribution on Hungary by Csaba Makó and L. Héthy is very revealing. The two authors differentiate between two principal philosophies of worker participation: job-oriented participation and the so-called power-oriented participation model. The former model focusses mainly on improvement of work efficiency and is thus of particular interest to management. The latter, while never clearly spelled out, implies a redistribution of power within the enterprise as well as fundamental changes in the role of the unions. The authors are fully aware that the conditions for successful participation are dependent not only on the power relations within an enterprise but also on those pertaining in the wider society. They give a critical account of the functions of trade unions in Hungary and the critical reactions of workers to management initiatives and union policies and actions. A case study of a management-initiated change effort in a large factory producing engines and rear axles for cars and railway coaches gives empirical evidence for some of the general observations made by the authors.

<sup>4</sup> The Industrial Democracy Programme should be seen in the context of more recent changes in work environment reform. For an up-to-date account see Bjorn Gustavsen and Gerry Hunnius, *New Patterns of Work Reform: The Case of Norway* (Oslo: Oslo University Press 1981).

In the second book under review here Murray Yanowitch has collected a fascinating series of essays about the emerging discussion on worker participation in management in the USSR. No actual innovations are analyzed in this volume.<sup>5</sup> The discussion emerged during the economic reform of 1965 and ended in the early 1970s only to re-emerge in 1977. The very fact that such a discussion took place is of considerable interest. We can only speculate about the reasons for its re-emergence in 1977, but the recurring problems with excessive labour turnover and disappointing productivity performance are likely to be among the main reasons. Job satisfaction studies in the Soviet Union differ to some extent from many American and Canadian studies by the inclusion of non-work related factors. A study of 10,720 Leningrad workers who had quit their jobs (reported by Iadov) revealed that 35.6 per cent gave dissatisfaction with working conditions as their reason, 27.5 per cent listed housing and related issues, and 21.4 per cent mentioned wages, while 15.5 per cent gave other reasons. While Iadov takes the social division of labour and narrow professionalization as given, Aitov tackles the design of technology when he argues that the design of new technology (in machine building in this instance) must take into account the socio-psychological needs of workers if a deterioration of attitudes towards work is to be avoided.

The issue of workers' participation in management is discussed in the contributions by Kapeliush, Alekseev, and Tikhonov. It should be noted here that this discussion essentially ignores the "official" channels of worker participation

such as production conferences, trade unions, and the enterprise's party organization. For a brief critique of the Hungarian equivalent of these "organs of participation" the reader should consult the Cooper/Mumford volume. What is under discussion here is foremost the idea that managers should be elected and be accountable to those who elected them. Ideological support for such a radical departure from contemporary Soviet practice is given by Kapeliush in a quotation from Lenin who stated that "Democracy, implemented to the fullest and most consistent degree conceivable, transforms bourgeois democracy into proletarian democracy."

The political difficulties in implementing elections of managers become evident when we look at the reported results of various opinion surveys reported by Kapeliush. Support for the election of managers decreases sharply as we move from rank and file workers (89 per cent in favour) to higher level managers (47.3 per cent in favour). The large percentage in favour of elections may come as a surprise to many readers. While several instances of such elections are mentioned by the authors in this volume, it is not clear how extensive this practice has become, nor is the actual process of these reported elections described in any detail.

It is customary to conclude a review with some general positive or negative comments. This is difficult in respect to readers. The Yanowitch volume is a must for anyone interested in labour-management related issues in the USSR. The Cooper/Mumford volume contains valuable contributions, particularly the case studies from western Europe and Hungary. Its shortcomings should be clear to readers of this review.

Gerry Hunnius  
York University

<sup>5</sup> For a description of one reported, but short-lived innovation in the democratization of management in a state farm, see Murray Yanowitch, "Pressures for More 'Participatory' Forms of Economic Organization in the Soviet Union," Paper prepared for the *First International Conference on the Economics of Workers' Management*, Dubrovnik, October 1978, published in *Economic Analysis and Workers' Management*, XII (1978).

*Pastoral Production and Society/ Production Pastorale et Société* (London and Paris: Cambridge University Press and Éditions de la Maison des Sciences de l'Homme 1979).

Pastoral nomadism is a form of social adaptation to a peculiar environment more or less concentrated along a world-wide frontier between the steppe and farmland. Pastoral nomadism is based on the exploitation of this environment through the "intermediary of gregarious migratory herbivorous flocks or herds under the management of a human group;" it differs from ranching because it is a precapitalist economic activity.

The recent devastating drought in Sahelian Africa has stimulated increased interest in pastoral nomadic societies. This interest has taken two general though not mutually exclusive forms: 1) studies directly or indirectly sponsored by international development agencies and concerned with ameliorating pastoral conditions and underwriting commercial exchanges; and 2) studies concerned with understanding the form and structure of nomadic groups.

*Pastoral Production and Society* fits neatly into the second category. It is the collection of papers presented at an international colloquium held in Paris in 1976 under the direction of the *Équipe écologie et anthropologie des sociétés pastorales*. The conference was organized with an explicit call for a discussion of theoretical, conceptual, and methodological issues, and a desire to avoid empirical detail. If there is coherence to the papers, that unity lies in a critical perspective on the two dominant themes in anthropological studies of nomadic pastoralists.

The contributors, although not uniformly and not always consciously, take issue with Evans-Pritchard's concept of segmentary lineage as the basis of nomadic political and social organization. According to Evans-Pritchard, nomadic groups were composed of lineages with relatively equal control over resources and manpower, which effectively limited centralization of power. Studies by Bonté and Gast on the Tuareg and by Digard and Legrand on the nomads of Asia demonstrate the historical dynamics of political centralization among different

groups of nomads. Further contributions by Hamès on the Maures and a general study by Burnham refine the processes of political centralization in terms of external and internal factors.

The second major thrust of this collection is a critique of ecological reductionism as an explanatory tool. By concentrating on the economic and social constraints of access to resources, contributors such as Lefébure on the Ayt Atta of Morocco argue that variables other than the natural and demographic must be used to account for the social differentiation found among social groups. This perspective of differential access to resources and the development of inequalities runs parallel to the critique of segmentary lineages.

Nomadic pastoralism was never a self-contained economic activity. Herders need to supplement their diets of milk, blood, and occasionally meat, with grain and vegetable products. This gave rise to the development of trade and the articulation of social relationships with sedentary farmers, which in turn had two implications. First, it established an exchange value on products of pastoral production and therefore the foundation for social inequality based on control over the products of trade. Second, the biological necessity of securing agricultural products created a dialectic which occasionally led to efforts by nomads to create political hegemony. The alternating claims to power (and therefore control over resources and surplus) by nomads and farmers has been a historical theme all along the desert fringe.

For those not directly interested in pastoral nomadism, this collection provides insights into the current debate raging within French marxist anthropology. Adherents of Maurice Godelier's concept of kinship acting as relations of production are pitted against adherents of Claude Meillassoux's argument that the articulation of kinship structure reflects the form of the economy. Talal Asad offers a short paper arguing that there are only two modes of production, a precapitalist one

and a capitalist one, and Lawrence Krader, compiler of Marx's *Ethnographic Notebooks*, has a piece on nomads and the state. Despite the unevenness of the contributions, *Pastoral Production and Society* reaffirms the value of historical materialism.

Richard Roberts  
Stanford University

Charles F. Delzell, ed., *The Future of History* (Nashville, Tennessee: Vanderbilt University Press 1977).

THIS COLLECTION of essays grew out of a symposium held at Vanderbilt University in 1975. Several established scholars were invited to offer evaluations of their particular fields and to assess new methodologies and interpretations. Although the topic of the book is timely, its tone is generally cautious and at times, downright superficial. Editor Charles F. Delzell begins by suggesting that the essays are united by the caveat that "the historian should keep his mind open to new methodologies and revisionisms but at the same time preserve a healthy skepticism of pseudosciences, new dogmatisms and the merely faddish." (x) Several of the authors perform admirably to the standards of this inauspicious and bland beginning.

Practitioners of the history of ideas will benefit little from Paul Conkin's definition of an intellectual historian as "one who concentrates, if not exclusively, then predominantly upon past concepts and beliefs." (113) Nor will their current crisis of identity be eased by his elaboration of intellectual history as the chronicle of great moments in western thought. On the topic of the history of the American south, C. Vann Woodward opens with a celebration of the proliferation of studies over the past 30 years and then abruptly launches into a witty but unnecessary attack on Fogel and Engerman's *Time on the Cross*. Stephan Thernstrom ranges over the development of urban history as a field

and closes with a call for innovation, accuracy, and a warning to avoid jargon. Richard Leopold invokes the standards of early giants in the history of American foreign policy and then predicts that the field will soon witness a "renewed dedication to basic research." (243)

Some of the essays in non-American topics are more promising. After a wide ranging review of developments in modern Japanese historiography, John Whitney Hall asks western historians to abandon their almost exclusive use of Europe as a model for comparative purposes and to consider Japan's social and economic history as an alternative. Lewis Spitz challenges the traditional division of European history into ancient, medieval, and modern periods. His attempt to relocate the Reformation in the broad sweep of European history is instructive to anyone who is tempted to relegate a particular epoch to "a time of transition." Gordon Wright ably defends the writing of contemporary history from critics who argue that students of post-World War I Europe seem to focus only on critical episodes at the expense of underlying structures of social change. His essay raises the question of the failure of the Annales school to deal effectively with the modern period.

Two of the essays should be singled out for special mention. Lawrence Stone outlines the changing relationship between history and the social sciences since 1870. Between 1870 and 1930, Stone argues, historians and social scientists conducted very little dialogue. With the development of the Annales school, the methods of social scientists were increasingly appropriated by historians. This happy integration forced historians to make explicit their hidden assumptions, to define their terms more precisely, to refine research strategies, and to support impressionistic judgments with quantitative evidence. Warning historians to be wary of injudicious quantification, psycho-history, and mechanistic determinism, Stone suggests methodological



diversity and ideological pluralism as correctives. Although readers may wish for a more adventurous solution, they will appreciate his sensitivity to the theoretical issues involved. Likewise Bernard Cohen's long essay on the evolution of the history of science as a sub-discipline deserves careful scrutiny. Rich footnotes and an engaging style help to make this enigmatic field easily accessible to the non-specialist.

Generally, however, the authors in this volume recommend a cautious and non-activist approach to the writing of history. Hall, for example distinguishes between ideological history (that is, Marxist) and methodological history, clearly expressing his preference for the latter. The organizers of the symposium, moreover, chose not to include papers on working-class history, women's history, or immigration and ethnic history. If the future of history bears any resemblance to this unexciting forecast, at least we will be able to say that they warned us in advance.

Bruce Tucker  
Dalhousie University

George Konrád and Ivan Szelényi, *The Intellectuals on the Road to Class Power: A Sociological Study of the Role of the Intelligentsia in Socialism*, translated by Andrew Arato and Richard E. Allen (New York: Harcourt Brace Jovanovich 1979).

THERE HAS BEEN a great deal of speculation in the West in recent years about the appearance of a "new class" of technocrats and intellectuals. One right-wing ideologue appearing in the *Financial Post* has even spoken of a "ruling class of bureaucrats and publicists" — proving, if nothing else, that when reactionaries adopt Marxist terminology they are apt to sound even more idiotic than usual. Unfortunately, much of the "new class" discussion has been not much more enlightening than this, from whatever ideological vantage point it has issued. Now comes a brilliant and exacting study of the "new class" in process of forma-

tion, not from the West but from the East.

George Konrád is a novelist, Ivan Szelényi a sociologist. They are both of the generation which came of age at the time of the Hungarian Revolt of 1956. Since that time they have observed the evolution of Hungary into the most "liberal" of Soviet bloc nations. In 1974 they committed their thoughts to paper in a manuscript which they felt constrained to bury each night. The police seized a copy and condemned it as a manuscript "which could serve as the programme of a counterrevolution" (high critical praise!). The authors were given the option of emigration. Konrád stayed, Szelényi ended up in Australia where he obtained the one copy of the manuscript which the police had missed, and which was finally published, in English, in 1979.

As it turns out the East European police are good critics, with a nose for subversive writing. Konrád and Szelényi are indeed subversive. *The Intellectuals on the Road to Class Power* is a very important book, along with Rudolf Bahro's *The Alternative in Eastern Europe* perhaps the most important to reach the West from the régimes of "actually existing socialism" in the 1970s. They have written, however, a very different book than Bahro's teleological Marxist vision: more empirical, more sociological, perhaps more cynical or more realistic, depending upon one's point of view.

They adopt a Marxist mode of class analysis, which to their surprise, after years of avoiding official orthodoxy, they suddenly discovered to be the most powerful at hand. But it is, in a sense, a Weberian analysis as well and to the extent that it is Marxist, it undermines some fundamental Marxist assumptions by turning them upon themselves. The core of their insight is that while class conflict is indeed the motor power of change in modern social formations, there is no reason to see the working class as a universal class which will end class oppression by overthrowing capital. The socialist societies of Eastern

Europe instead exemplify, in a more developed manner than the market societies of the West, a continuing dialectic of class conflict, this time around the emergence of the intellectuals as a new class force. Moreover, the very emergence and triumph of socialism can best be understood as the work of intellectuals striving for class consciousness and class power in the process of remaking the world. The working class is still left at the post and, one would assume from the authors' argument, are rather likely to always remain there.

It is the authors' thesis that intellectuals are in the process of forming a class, in contemporary Eastern European régimes, although nowhere else in the world. This development has roots in the specific historical evolution of the intelligentsia in Eastern Europe, which they argue was already distinct from the Western experience before the advent of socialist régimes (Perry Anderson argues this in a somewhat different manner in *Lineages of the Absolutist State*), but which derives its real impetus from the nature of states based on the legitimating principle of "rational redistribution" as opposed to market allocation. In this *teleological* context of societal choice the stage is set for the rise of the intellectuals who deal above all in *values*. They analyze the dialectical tension between the transcendence of values and the historically determined class interests of intellectuals imminent in their social position. In the latter case they investigate in detail the contradictory location of intellectuals between the ruling party élite and the working class. It is here that the fertile dynamic of transition from post-Stalinism to a third phase of socialism can be discerned.

The intelligentsia itself can be divided into a "technocracy" utilized by the state and the marginal intellectuals who rest in uneasy relationship with the ruling élite and its repressive apparatus. The marginals in turn divide into teleological critics

(usually Marxist) and empirical revisionists whose social science forces them to re-examine the official doctrines. Both categories are examined with unsparing candour, without a hint of romanticism: as marginal Eastern European intellectuals, the authors are extraordinarily self-critical, maintaining the courage of their methodological convictions. They realistically discuss the tortuous dialectical process by which, they argue forcefully, the intelligentsia under socialism will ultimately form a class, a class which will be the real beneficiary of socialist revolutions.

The argument is dense, rich, and suggestive. Given the circumstances of its composition it lacks the armoury of footnotes and references behind which social scientists normally hide, but as an essay in social and political criticism it stands on its own, unadorned and provocative. Konrád and Szelényi have the uncanny knack of disarming critical reactions in the reader by pre-empting possible cavils in advance. It is above all a book which should be read very carefully by Western Marxists. The authors' vision of the socialist future is unsentimental. Paradoxically, they undermine the Marxist *telos* by the critical application of Marxist method. This should generate a serious debate, which cannot be engaged here within the confines of a short review.

I completed my reading of this book at the moment of the dénouement of the Gdansk shipyard strike in August 1980. The apparent victory of the workers signalled in the granting of free trade unions and the right to strike is a remarkable symbol of the vitality of working-class struggle against a régime which claims to be a workers' state. Konrád and Szelényi in their realistic manner were not unaware of such possibilities when writing this book, nor were they unaware of the potential alliance between militant workers and the marginal, dissident intelligentsia which seems to have taken shape in Poland. Indeed, they predict it. But they ground

their analysis on the hard base of immanent class interests, not on romantic notions of revolutionary transcendence. And in this harsh light, it is the intelligentsia, not the working class which will inherit the leading role in the next phase of socialism. This phase, the authors suggest with mordant irony at the end of their book, will have arrived "when some Eastern European publisher accepts this essay for publication."

Reginald Whitaker  
Carleton University

David Bradby and John McCormick, *People's Theatre* (London: Rowman and Littlefield 1978).

PEOPLE'S THEATRE turns out to be a difficult concept, at least for Messrs. Bradby and McCormick. Is it, for instance, the theatre of a nation as a whole, rich and poor alike? Or, of the underprivileged and oppressed members of society? If the latter, is its purpose to enhance their political awareness? Or, to extend to them the cultural privileges hitherto the preserve of the wealthy and educated? Again, is theatre exclusively an instrument of education and propaganda? Or, a stimulus to community debate about social aims, achievements and failures?

Subscribers to each of these different aims are part of the panorama surveyed by our authors. Thus, they concede at the outset that only a negative definition is possible: "all have been united in their impatience with existing theatrical forms, audiences, buildings, techniques." However, since not everyone infected by this same impatience is included here, this definition is scarcely more helpful than a positive one would be.

Nevertheless, the book certainly has its uses and not least because it sheds light on unfamiliar areas in the history of the modern theatre. The authors are both lecturers in French, Bradby at the University of Kent, McCormick at Trinity College,

Dublin, and they are particularly good in their discussion of the ideas and practices of the French theatre since Romain Rolland's passionate espousal of *Le Théâtre du Peuple* in 1903. Rolland's pamphlet is fairly well known, but much less so, at least to English-speaking readers, are the work of Maurice Pottecher at Bussang in the Vosges, the syndicalist Théâtre Confédéral de la Grange-Aux-Belles, and Léon Moussinac's Théâtre d'Action International. On more familiar subjects, like Firmin Gémier and Jean Vilar, their discussions are equally worthy of attention.

In other parts of the territory, post-revolutionary Russia, Weimar Germany, and the Anglo-American world, their commentary is less original, to be sure, but it is useful to be asked to consider simultaneous responses to the ideas associated with people's theatre across a complex set of historical circumstances. Doing so implies another book than the one we have here, a more profound study in comparative theatre. But Bradby and McCormick point the interested enquirer in the right general direction.

Alan Andrews  
Dalhousie University

Paul J. McNulty, *The Origins and Development of Labor Economics*

MUCH OF CURRENT labour economics can be aptly characterized as embodying a lot of brains but very little memory and even less common sense. It is therefore highly refreshing to read a book such as this — a book which brings both brains and common sense to the task of restoring a memory to present day labour economics.

McNulty's point is that the current state of mainstream labour economics in North America is rather atypical. Currently, it is seen basically as a branch of applied microtheory and/or applied econometrics. Primary attention is devoted to models of individual maximizing behav-

bour, the efficiency properties of equilibrium market relationships, and the technical characteristics of statistical estimating techniques. Labour economics is thus now very close to mainstream neoclassical economics, but McNulty points out that it is not so very long ago that it maintained an interdisciplinary perspective which was in general very critical of laissez-faire economic theory. Indeed, he points out that over time the very conception of labour in the productive process has evolved in parallel with the actual social evolution of work relationships. In his book he presents the evolution of the study of labour from the writings of early economists to the emergence of labour economics as a distinct field of study.

In feudal societies, where impersonal markets for wage labour did not yet exist, "justice" was seen as the principle on which wage administration should be founded. When wages were predominantly set by princely fiat or guild regulation, there was no need for a theory of how wages were set, rather labour economics was seen as that branch of ethics which defined "what wages should be in order to ensure justice in social dealings." (19) Neither in feudal nor mercantilist times, however, did notions of "justice" imply any tendency to advocate high wages. McNulty notes the "paradox" that, although the labour of artisans was seen as the ultimate source of national wealth, "considerable agreement existed on the social utility of low wages." (31) Strangely, he argues: "Although many of the economic commentators during the age of mercantilism were themselves tradesmen, pleading their own causes, it would be cynical and shortsighted to dismiss the general approval of the idea of wage regulation as merely a self-serving effort to advance the interests of the trading and employing class." (25)

By contrast to previous writers, Adam Smith appeared in 1776 as a positive friend of labour. In his emphasis on labour as the ultimate source of economic

wealth and the determinant of value, in his view that the skill and dexterity of the population were the ultimate source of national economic strength, in his critique of wage regulation by the employing classes, and in his advocacy of a high wage, high productivity policy, Smith was writing more as a social critic than as an apologist for existing wages doctrines. McNulty points out that Smith attacked both slavery and the combinations of masters and employers to restrain wages. Smith saw both the alienation of labour under conditions of extensive division of labour and the inequality of bargaining power between employers and employees in labour markets — as McNulty points out, his successors were far narrower in their analyses.

It is in McNulty's discussion of classical economists that today's economists will begin to feel a sense of *déjà vu*. Although Ricardo saw distribution as "the principal problem of political economy" and offered a theory of value wherein labour played a crucial (but not exclusive) role, still it is the methodology of his analysis which had the more lasting impact. In constructing highly abstract models without institutional content, in emphasizing the behaviour of competitive markets, and in concentrating on equilibrium properties in the long run, Ricardo led several generations of English economists away from the study of concrete social reality. Classical economists tended to be anti-union (because unions impeded market forces), to eschew social responsibility for the poor (since they had brought it on themselves), and to argue that the level of real wages was set by a "wages fund" whose size was beyond any individual's control. But as McNulty puts it, "the greatest failure of the classical economists' study of labor may well lie in their inability to explain the existence of widespread poverty and unemployment on grounds other than those having to do with behaviour that was less than fully rational on the part of workers or other owners of

factors of production." (90) To which one can only add *plus ça change...*

In his discussion of Marxism, McNulty's book is perhaps weakest. Not only is there a relatively cursory treatment of Marx, *there is no attention, whatsoever*, to writers in Marx's tradition, who came after him. There is, for example, no mention made of such modern Marxist writers as Morishima who have largely resurrected Marx's labour theory of value. McNulty quotes Samuelson on Marx as a "minor post Ricardian" and Schumpeter on Marxism as a religion as well as a system of economics. Clearly McNulty is not a Marxist, as one might guess from the very organization of the book. In emphasizing the internal logic of the evolution of labour economics, McNulty de-emphasized its role as an ideology. Although he quotes authors such as Walker who asserted that belief in the antagonism between labour and capital would "blaspheme against the harmonies of Providence" and although he notes the urge for social reform present in the early institutional treatment of labour economics, still McNulty never attempts to draw these observations into a historical context. It is a strange omission, since many of the writers on labour economics have been explicit about their consciousness of the social role played by theories of the labour market. J. B. Clark, for example, in his original presentation of his "natural law" that wages are determined by labour's marginal productivity wrote that "the right of society to exist in its present form, and the probability that it will continue so to exist, are at stake" in the argument.

Nevertheless in his discussion of the origins of labour economics as an applied field in North America McNulty is at his best. Who today would recognize the American Economics Association as an organization whose draft prospectus held that "the doctrine of laissez-faire is unsafe in politics and unsound in morals and that it suggests an inadequate explanation of

the relations between the State and the citizens" (a declaration softened only to "we regard the state as an agency whose positive assistance is one of the indispensable conditions of human progress" in the adopted statement of principles). How many would recognize American labour economics as characterized by "the historical and comparative approach to the study of labor institutions and organizations, an inductive study of labor's actual economic position and the effects of labor's policies, an espousal of the ideal of an interventionist state in labor matters, and an emphasis on the actions of groups rather than of individuals." (139) McNulty notes the origin of these tendencies in the German heritage of many of the prominent early writers in labour economics, but emphasizes that the institutional school was a uniquely American approach to the study of labour issues. In emphasizing the roles played by Ely, Commons, Perlman, Adams, Sumner, and many others in the late 1800s and early 1900s, McNulty is reminding us of an interdisciplinary tradition which is now largely forgotten. Such failure of memory means that institutional and radical writers such as Doeringer and Piore and Bowles and Gintis in the 1970s must rediscover the work of Kerr, Reynolds, and Dunlop in the late 1940s/early 1950s, who were in their own turn lineal descendants of the institutional approach of the 1920s and 1930s.

Since the 1970s, of course, mainstream labour economics in North America has become increasingly anti-institutional and ahistorical. The victory of the human capital/neoclassical research tradition within labour economics has pushed interdisciplinary approaches into the twilight world of industrial relations and has concentrated economists' attention, once again, on individualistic, purely economic, non-institutional, explanations of labour market phenomena. The strengths of this approach include a much more rigorous attitude to explicit theoriz-

ing and a much greater attention to statistical verification of hypotheses. McNulty also emphasizes, however, the weaknesses involved in a rather extreme form of tunnel vision in much of current labour economics combined with excessive technical obscurantism. He concludes his book with the hope that the pendulum will swing back to a more balanced approach to the study of labour — to which sentiment one can only say "amen."

Lars Osberg  
Dalhousie University

Anne Humpherys, *Travels into the Poor Man's Country: The Work of Henry Mayhew* (Firle, Sussex: Caliban Books 1977); and Sheila M. Smith, *The Other Nation: The Poor in English Novels of the 1840s and 1850s* (Oxford: Clarendon Press 1980).

In [the ditch] float large masses of green rotting weed, and against the posts of the bridge are swollen carcasses of dead animals, almost bursting with the gases of putrefaction. . . . The striking peculiarity of Jacob's Island consists in the wooden galleries and sleeping rooms of the houses which overhang the dark flood. . . . Across some parts of the stream whole rooms have been built, so that house adjoins house; and here with the very stench of death rising through the boards, human beings sleep night after night, until the last sleep of all comes upon them years before their time. Scarce a house but yellow linen is hanging to dry over the balustrade of staves, or else run out on a long oar where the sulphur-coloured clothes hang over the waters, and you are almost wonderstruck to see their form and colour unreflected in the putrid ditch beneath. (Henry Mayhew, "A Visit to the Cholera Districts of Bermondsey," *Morning Chronicle*, 24 September 1849).

IN THE MIDDLE DECADES of the nineteenth century, comfortable Victorians, their attention caught by "the attraction of repulsion," and, in a period punctuated by devastating cholera epidemics, by a growing awareness of the

ease with which disease could spread from mean streets to modish ones, turned their attention to the physical and — in characteristic Victorian fashion — moral circumstances of the most profoundly uncomfortable of their fellow citizens. Part of this concern was evidenced by the reports of sanitary conditions by Edwin

Chadwich (*An Inquiry into the Sanitary Condition of the Labouring Population of Great Britain*, 1842) and Dr. Kay Shuttleworth (*The Moral and Physical Condition of the Working Classes Employed in the Cotton Manufacture of Manchester*, 1832), as well as by the famous revelations about the living and working conditions of the labouring people embodied in the spate of parliamentary investigations into the circumstances of workers, particularly children and women, in factories, mines, potteries, and agriculture. These studies are familiar fare for the modern social and economic historian, but for most Victorian readers the main source of information about the poor, Disraeli's "other nation," came from literary and journalistic renderings of this data. The description of Jacob's Island quoted above would have been well-known; Dickens had made the area infamous in *Oliver Twist*, probably the best part of Charles Kingsley's 1850 novel *Alton Locke* centred on the sweatshops and hovels of this rookery, and Rev. Thomas Beames quoted Mayhew's article extensively in his widely read *Rookeries of London* (1850). This slum attracted attention because both deadly disease and capitalist exploitation were here writ large. Here the first cholera deaths in the outbreaks of 1832 and 1840 occurred, here metropolitan slum clearances dramatically increased overcrowding, here landlords charged exorbitant rents for derelict accommodation, and here the evils of sweated labour were much in evidence. Reinforcing the sense of Jacob's Island as the archetypal symbol of the horror, as the menace and the tragedy of the "other nation," both

Kingsley and Dickens in their novels made this the scene of death — the accidental death of the sweated and starving Jemmy Downes and the violent death of the dastardly Bill Sikes. Jacob's Island was but one of many symbols used by authors concerned with the problems of poverty: palimpsests upon which the ecology of the English poor was described.

The authors of the books under review are concerned to situate the writers they examine within the framework of the era's social concerns and attitudes and to examine their differing uses of the same material. The two works are fruitfully considered together because of the manner in which their most telling interpretations mesh. Humphery's lucid examination of Henry Mayhew, the pioneering social investigator, elucidates Mayhew's objective of producing a scientific study of labour and the poor based on a collection of facts, with, in his words, "a view of arriving ultimately at the laws and circumstances affecting and controlling the operations and rewards of the labourer." (Interestingly, Humpherys attributes much of his scientific bent to his early interest in chemistry.) It was his aim, not to write an exposé, but a work of political economy. As his work advanced, this aim receded ever further into the distance. Where he was strikingly successful was in giving a face and voice to London's labouring people. His success was born of a passionate interest in the details of ordinary lives, which precluded the fixing of information into preconceived pigeonholes, and a nonjudgemental empathy — or, in E.P. Thompson's words, a freedom from "middle-class moral halitosis." His ability to get people to talk at length about themselves yields significant insights into working-class life; through the famous material on street-sellers we learn about working-class amusements, domestic decorations, and favourite foods, as well as something of working people's interests, sympathies, and concerns. But beyond this,

Humpherys argues, what makes Mayhew's work "a very important corollary to all the best social observations of the period" is his craftsmanship in presenting what he uncovered. Descriptive scenes are carefully constructed to combine a sense of scientific neutrality with strong emotional impact. The author backs up her interpretation by setting Mayhew amongst his literary contemporaries (particularly Kingsley and Dickens). Where other accounts of a trade or a locale are festooned with moralizing, irony or fulsome rhetoric, she shows Mayhew's version to have greater veracity — both as social science and as art.

In *The Other Nation*, Smith examines the re-creation of the lives and environment of the poor in a number of English novels (in particular Elizabeth Gaskell's *Mary Barton*, Kingsley's *Alton Locke* and *Yeast*, Dickens' *Hard Times*, and Disraeli's *Sybil*). She shows that, like Mayhew's writings, these authors' work mirrored a broader concern with poverty and were extensively based on fact: on parliamentary Blue Books, on investigative reportage, and personal knowledge. It is instructive to note how closely even such a wooden novel as *Sybil* often adheres to the evidence of the Blue Books. Disraeli, unlike Mayhew or even the parliamentary commissioners, does not let the poor speak for themselves; their real experiences are generalized so as to make them seem improbable, and words and sentiments of another class are made to issue from their lips. Despite their authors' frequent protestations of a documentary approach, these novels are, despite occasional glimpses which ring true, essentially reflections of middle class fears and opinions rather than faithful recreations of working-class reality. The need to spin an exciting and saleable yarn meant that any attempt at faithful rendering (to which Mrs. Gaskell comes 'closest) was distorted by constant crisis: extreme poverty, social unrest, violence, untimely death or moral dilemmas.

Smith's analysis plainly shows that for most authors the poor were problems rather than people, and as "problems" working people were not seen by these writers as capable of internally generating solutions to their own difficulties. Indeed, many authors seem to have had the dual purpose of urging their readers to acts of Christian charity — such as shipping large chunks of the "problem" off to the colonies — and of denouncing trade unionism, showing through their plots that unions inevitably resulted in violence and threatened the peace of the bourgeois world. The novelists, in Smith's view, despite the flaws in their works, were responding creatively to the "other nation;" they sought to go beyond frivolous entertainment to expose, to inform, and to teach. They counselled the middle class to social responsibility against the hands-off policy of *laissez-faire* economics, and to compassion against Utilitarian rigidities.

Smith's laudable intention to relate her analysis of the novels to both a wider artistic and literary context and a concrete historical setting has led to a book which is sometimes disjointed and often bloated. In one 90 page chapter designed to show how the novelists depicted the environment of the poor, the argument is diffused by frequent detailed analyses of descriptive passages, tedious for a reader who knows the novels and probably confusing for one who does not, and further weakened by many extraneous or marginally relevant asides. Some of the additional information might have been relegated to discursive footnotes, but here all the notes are incorporated in the body of the text, an irritating practice which further impedes the literary flow. As though prescient that only those assigned the task of reviewing the book will read all of the lengthy quotations (one from Ruskin takes up nearly a page), the author often repeats the salient points of the quoted material in her following remarks. Pertinent though it is to indicate that painters and photographers

shared some of the same concerns and perspectives as her subjects, the reproduction of some 47 plates and discussion of them in the text adds little to her argument. Surely this is not the place for inclusion of works by Courbet and Van Gogh, nor of photographs and prints — including some yawningly familiar ones by Doré — two or three decades later than the period with which the book is concerned. Fairly successful in her use of historical material elsewhere, Smith undermines confidence in the initial chapter by excessive reliance on the view of a single historian (whose name is mentioned four times on one page) and by her unfortunate citing of that author's most bizarre assertion — that "the working class . . . [came] into existence with the Parliamentary Reform movement from 1816 to 1819."

Both authors make solid use of recent research on the writers who are their subjects. Humpherys' survey of work on Mayhew is particularly perspicuous. Seeking to interpret the man as well as the work, Humpherys begins with a biography which helps to elucidate Mayhew's social perspective, formed in part in reaction against a tyrannical father who was steeped in bourgeois respectability, and nurtured in London's bohemian journalistic circles. Her account balances the well-known social investigations with Mayhew's little known and more conventionally preachy writings, and gives a fascinating account of the development of the surveys and the evolution of the interviewing techniques. Mayhew himself indicated that he deleted expletives, but what else was omitted or altered? To what extent did he make use of shorthand reporters or research assistants to record evidence, organize material, and work up statistics? There is also no consideration of the way in which these massive investigations were financed. While some questions remain unanswered, this book considerably advances our understanding of a writer whose work foreshadowed the later



social investigations of Booth and Rowntree.

Both Humpherys and Smith conclude that for most writers, however sincere their intentions, the working class remained the *other nation*: foreign, mysterious, and vaguely threatening. Only Mayhew saw them as individuals, found them interesting when not in crisis, and recognized with them a common humanity. Mayhew alone of the literary figures examined could have written: "Might not 'the finest gentleman in Europe' have been the greatest blackguard in Billingsgate, had he been born to carry a fish-basket on his head instead of a crown?" (Mayhew, *London Labour and the London Poor*, I, 230)

Patricia E. Malcolmson  
Queen's University

Theresa Malkiel, *Journal d'une gréviste*, traduit de l'américain par Marianne Sirgent et présenté par Françoise Basch (Paris: Payot 1980).

**MALGRÉ SON TITRE**, cet ouvrage offre plus que le simple récit fictif d'une ouvrière en lutte contre l'exploitation d'un capitalisme naissant. En première partie, la longue introduction au *Journal* de T. Malkiel par Françoise Basch, "Histoire d'une grève de femmes à New York en 1909," mérite à elle seule tout notre intérêt. Le travail de F. Basch vise dans un premier temps à resituer cet épisode dans son contexte historique et sociologique: main d'œuvre immigrée, à peine qualifiée, discriminée en tant que femmes à la fois par les ouvriers syndiqués et par les patrons. L'objectif de cette historienne féministe: donner une voix à des êtres humains qui cumulent plusieurs formes d'oppression se renforçant mutuellement: prolétaire, immigrante, femme.

Quant au *Journal* publié une première et dernière fois en 1910, il soulève un intérêt certain comme témoignage de "l'insurrection des 20,000" ouvrières du

corsage auxquelles F. Basch tend à restituer le titre de grévistes à part entière. Car, ce que l'auteur met en cause, c'est le "spontanéisme" ou l'"amateurisme" dont on a trop souvent qualifié cette grève de femmes. La publication du *Journal* présente donc un double intérêt car il pose simultanément le problème de la lutte des classes et de la lutte des femmes, ainsi que celui des liens entre l'une et l'autre.

Première grève massive de femmes, étape importante dans la maturation du mouvement ouvrier juif en Amérique, la date du 22 novembre 1909 marque un tournant dans l'histoire syndicale américaine. Le mouvement mobilise entre 20,000 et 30,000 travailleurs de l'industrie du vêtement, dont 80% de femmes, immigrantes juives âgées de 16 à 25 ans. La féminisation des métiers de l'aiguille (en 1913, 70% des travailleuses de la couture sont des femmes), s'accompagne d'une deuxième caractéristique: la majorité de ces femmes sont des immigrantes, 57% de Juives Russes et 35% d'Italiennes. 7% seulement sont Américaines.<sup>1</sup>

Quelles étaient les conditions de vie et de travail de ces ouvrières? Une militante de la première heure, Pauline Newman, apporte le témoignage suivant: "Je travaillais au neuvième étage avec d'autres enfants comme moi. Notre tâche n'était pas difficile: une fois terminées les coutures à la machine, c'était à nous les jeunes de couper les fils qui restaient avec de petits ciseaux. Et quand arrivaient les inspecteurs, savez-vous ce qui se passait? Les contremaîtres faisaient grimper tous les enfants dans des caisses... les recouvraient avec les corsages terminés... car nous étions trop jeunes pour travailler légalement à l'usine."

Une organisation du travail relativement anarchique règne dans les ateliers, encourageant une surexploitation des travailleuses. Bien souvent, les membres d'une même famille ou les amies, jusqu'à six ou sept dans certains cas, forment un

<sup>1</sup> Interview de P. Newman, *id.* p. 23.

groupe d'apprenties; toutefois, un seul nom apparaît dans les livres car elles se partagent le même salaire. Les semaines de travail sont de 56 heures, parfois de 70 en haute saison, les heures supplémentaires étant rémunérées au tarif normal.

Pour prévenir tout éveil de solidarité, on place par exemple une ouvrière juive à côté d'une Italienne: immigrantes récentes, parlant peu ou pas l'anglais, elles ne peuvent communiquer. L'exploitation atteint son comble quand... "On leur comptait l'électricité 20% plus cher que le tarif, ainsi que chaque aiguille cassée, chaque aiguille usée à 25% plus chère que son prix. On leur faisait payer la chaise... la patère pour le chapeau. On leur infligeait une amende pour un retard de cinq minutes le matin..."<sup>2</sup>

Enfin, après plus d'une demi-journée passée dans des ateliers malsains, les nouveaux arrivants ne peuvent que retrouver des conditions de vie et de logement tout aussi sordides: ils s'entassent dans les "bidonvilles du Nouveau Monde," échappant à toute norme de sécurité et d'hygiène. "On sortait de l'atelier, sombre et froid l'hiver, brûlant l'été, sale, ni balayé ni aéré, et on rentrait chez soi: mais quel chez soi?... je vivais dans deux pièces avec ma mère et mes deux sœurs; la chambre à coucher n'avait pas de fenêtre, les toilettes étaient dans la cour; mais c'était pareil à l'usine. L'été, les trottoirs, échelles d'incendie, toits se transformaient en chambres à coucher pour pouvoir respirer un peu. On portait des vêtements pas chers, on se logeait pas cher, on mangeait pas cher et on n'avait rien à espérer du lendemain."<sup>3</sup>

Les difficultés d'implantation d'une organisation syndicale chez les ouvriers juifs, arrivés de fraîche date en Amérique, relèvent de deux causes: d'une part, le type même d'immigration (main d'œuvre non spécialisée, ne possédant pas la langue du pays d'accueil, sans tradition

d'action collective, porteuse d'un héritage familial et culturel entravant parfois l'effort d'adaptation à une organisation plus combative); d'autre part, la complexité des rapports avec le mouvement ouvrier américain dont l'idéologie corporatiste et un certain chauvinisme s'opposent à la philosophie plus totalisante et d'inspiration messianique du mouvement ouvrier juif.

Quelle est la situation des femmes à l'intérieur du mouvement? En 1910, elles représentent environ 20% de la main d'œuvre, mais 3.3% seulement des travailleuses de la confection, des textiles et de la blanchisserie sont syndiquées.<sup>4</sup> Certaines causes expliquent le faible taux de syndicalisation chez les femmes: leur jeunesse (plusieurs en sont encore à "l'âge du jeu"); l'extrême mobilité de cette catégorie de travailleuses (le lieu de travail n'est pour elles qu'un lieu de passage avant le mariage). Cette insertion provisoire dans le monde du travail les écarte d'emblée du mouvement syndical américain préoccupé des seuls ouvriers spécialisés.

De plus, les réflexes sexistes ne font pas défaut. Ainsi, lors de la première grève qui suit la mise en place, en 1904, d'un syndicat dans une entreprise de chapeaux, seuls les hommes mariés reçoivent une allocation de subsistance. Dans le grand syndicat des métiers du vêtement, l'ILGWU fondé en 1900 et qui compte deux tiers de membres féminins, aucun nom de femme n'est inscrit dans les comptes rendus de congrès avant 1909 et aucune femme n'apparaît sur les photos officielles du bureau national. En 1917, à Philadelphie, le même syndicat décide de n'accorder aux femmes que la moitié des prestations auxquelles les hommes ont droit.

Les problèmes spécifiquement féminins ne sont pas abordés lors des réunions syndicales; les besoins propres des femmes ne sont pas reconnus. Il est vrai  
<sup>4</sup> R. Miller J., "The British and American Trade Union Leagues," *Ibid.* p.33.

<sup>2</sup> Souvenir History of the Shirtwaistmaker Strike, *id.* p. 26.

<sup>3</sup> Interview de P. Newman, *id.* p. 17-18.

que le militantisme féminin reste faible, surtout chez les immigrantes italiennes, davantage opprimées par la famille de l'Église; enfermées à la maison, peu instruites, ces femmes sont des victimes rêvées. Toutefois, ce sont les travailleuses américaines de la confection qui manifestent le plus de réticence à s'organiser. Elles seront souvent recrutées par les patrons comme "briseuses de grève." Conscientes des privilèges que leur confère la nationalité et la connaissance de la langue mais inconscientes de leur propre exploitation, au mieux se montreront-elles sympathiques au mouvement des immigrantes mais ne seront pas vraiment solidaires de leurs objectifs. Ce sont donc la diversité et l'hétérogénéité ethnique, culturelle et linguistique qui vont constituer l'obstacle majeur à tout effort d'organisation syndicale.

À un autre niveau, toutefois, l'appui du pays d'adoption doit être souligné. S'inscrivant dans le courant philanthropique et réformiste protestant d'une partie de la bourgeoisie américaine, le soutien, d'abord limité aux "bonnes oeuvres" en milieu défavorisé, se transforme rapidement vue l'ampleur des problèmes, en une organisation élargie et plus politique: la *National Women's Trade Union League*. Ses objectifs: "Aider les femmes dans le monde ouvrier à améliorer leur condition et ce, en les soutenant dans leur effort d'organisation." (42)

Dans un premier temps, la Ligue se consacre à une tâche d'information sur les conditions de travail des ouvrières et d'éducation auprès de ces dernières. Les militantes de la Ligue proclament une double appartenance idéologique: à la fois alliées des travailleurs et solidaires du mouvement des femmes. Cette double allégeance ne sera pas sans susciter des conflits, douloureusement ressentis par certaines. Car, même si "l'oppression pesait sur les femmes d'un bout à l'autre de l'échelle sociale... 'alliées' et travailleuses vécurent une collaboration tourmentée." (47) Le tiraillement entre

lutte de classes et lutte de sexes se manifeste particulièrement à l'occasion des campagnes pour le vote des femmes, selon que la priorité va à l'une ou à l'autre tendance.

À l'occasion de la grève des ouvrières du corsage de 1909, quelques mouvements suffragistes dont les effectifs se composent principalement de femmes à l'aise apportent leur soutien, surtout matériel, aux grévistes. Quelles est la signification politique de ce que certaines ont appelé la "brigade aux visons?" C'est la solidarité entre femmes qui les concerne avant tout; laissant dans l'ombre les autres objectifs politiques, elles se désolidarisent progressivement du mouvement ouvrier et finissent par adopter des positions nettement anti-socialistes. À la faveur d'un mouvement massif des ouvrières, les suffragistes ont donc tout simplement tenté de capitaliser sur sa dynamique pour renforcer leurs objectifs propres, sans aller toutefois jusqu'à mettre en cause l'ordre établi. Le suffrage des femmes n'a donc pu constituer à lui seul une base commune de lutte.

Par contre, l'appui des militantes du *Socialist Party of America* se situe à un tout autre niveau et s'oriente vers une perspective analytique également différente. Les premières fondaient leur action sur l'oppression des femmes de toutes les classes; les secondes mettent en relief l'exploitation de la classe ouvrière et privilégient la solidarité de classe. Il semble ainsi que l'oppression spécifique des femmes à l'intérieur d'une classe opprimée ne soit pas encore à l'ordre du jour. Que ce soit dans le cadre de la Ligue où leurs besoins particuliers en tant que femmes sont reconnus, mais où l'appartenance à une autre classe n'est pas acceptée sans restriction; que ce soit à l'intérieur du syndicat où leur condition de prolétaires est mise en valeur mais où celle de femmes est niée, les militantes ouvrières et socialistes des années 1900 ne trouvent pas à satisfaire les multiples facettes de leur identité. Finalement, seule la Ligue

reconnaitra le triple niveau d'oppression des travailleuses: prolétaires, immigrantes et femmes.

Qui est Theresa Malkiel, quelles sont ses préoccupations et pourquoi avoir adopté le journal comme forme d'expression littéraire et politique? L'auteur est juive, originaire d'Europe de l'Est; elle arrive à New York en 1891 et travaille dans un atelier de couture dès l'âge de dix-sept ans. Très tôt militante, favorable à une division des tâches entre parti et syndicat, elle se joint à d'autres socialistes pour fonder, en 1901, le *Socialist Party of America*.

Électoraliste et réformiste, le SPA n'endosse pas la conception d'une société de classes. T. Malkiel adhère à la même option idéologique; toutefois, sur la question des femmes, elle adopte des opinions plus tranchées. Elle participe activement à certains groupes autonomes de femmes ainsi qu'à la section femme du SPA et ce, malgré les positions orthodoxes du parti concernant la famille et le rôle de la femme. D'ailleurs, elle ne sera pas de celles qui vont quitter le parti en 1912 à la suite de divergences sur cette question.

Ses objectifs: briser le silence qui entoure l'exploitation dont sont victimes les immigrantes, surtout les Juives, peu ou pas représentées dans les cadres du SPA; travailler à l'obtention du vote des femmes, voie d'accès à un changement de société par lequel passe inévitablement la libération féminine. Cet engagement l'amène spontanément à s'impliquer dans la "grève des 20.000." Et c'est avant tout comme écrivain et journaliste qu'elle y participe.

Le *Journal d'une gréviste* ainsi que plusieurs brochures ou articles publiés par T. Malkiel reflètent toutefois une vision plutôt floue et peu réaliste du changement social: celui-ci constitue une condition nécessaire et suffisante à l'égalité des sexes. F. Basch fait état du divorce entre les positions officielles de l'auteur favorables au parti malgré son anti-féminisme et certains moments de lucidité où, s'adres-

sant aux ouvrières, elle dénonce la double journée de travail, au foyer et à l'usine. T. Malkiel constate également que les femmes doivent compter sur leurs propres forces: "... les femmes ne peuvent se tourner vers les hommes pour leur libération... ceux-ci n'étant pas, eux-mêmes libérés. La véritable libération des femmes ne se réalisera que par elles-mêmes..." (86) Par contre, elle ne favorise pas vraiment l'éclatement de l'organisation traditionnelle patriarcale et reconnaît même la prééminence morale de l'homme: "... les femmes continuent à considérer l'homme comme le guide de leur vie; c'est donc à lui qu'il incombe de guider ses pas..." (88)

Le *Journal d'une gréviste* reflète indéniablement ces contradictions. Comme le souligne F. Basch, la tonalité affective du récit évoque l'intensité de l'expérience vécue par l'auteur ainsi que la persistance de son attachement au milieu ouvrier qu'elle a quitté après son mariage avec l'avocat Léon Malkiel et qui l'a fait passer dans "l'establishment socialiste." Le *Journal* est donc le récit fictif de cette grève des ouvrières du corsage. Il met en scène une jeune Américaine, Mary, dont le père et le fiancé ne reconnaissent pas à une femme le droit de se mettre en grève. Le vécu de l'héroïne sert les fins didactique de l'auteur en mettant en relief une double dimension: d'un côté, la vie privée; de l'autre, une vie de labeur et de lutte, toutes deux s'interpénétrant dans un réseau de contradictions complexe mais dynamique. La première ouvre la porte au combat d'une femme opprimée en tant que femme; la seconde, au témoignage de la militante politique.

Le texte de T. Malkiel va à peine au-delà du féminisme modéré et ambigu qui l'a toujours caractérisée. Mais, contrairement aux critiques intransigeantes de F. Basch, cette ambiguïté et ce va-et-vient de l'héroïne au beau milieu de ses contradictions internes nous paraît révéler davantage la femme réelle, à l'écoute de ses besoins, mais également aux prises avec

sa volonté d'autonomie et les réticences de son entourage. L'auteur a échappé au piège de la femme mythique, débarassée des contraintes intériorisées.

Bien sûr, le récit par trop lyrique et pourtant réaliste quant à la simplicité d'expression, nous ennuie très vite; il a malgré tout valeur de témoignage. Témoignage de cette grève qui fut longue et dure (treize semaines et plus d'un million de dollars d'amendes); témoignage qui fait état d'une répression brutale et de mauvais traitements (plus de sept cents arrestations) et dont le bilan est "relativement" positif: les effectifs syndicaux augmentent considérablement; l'ouvrière n'a plus à payer ni fourniture ni amende; elle travaille 52 heures et non plus 60; enfin, son salaire est plus élevé de 20%. (68-9) Mais la non-reconnaissance syndicale, l'obligation de négocier individuellement avec chaque entreprise, l'absence de garantie

qui laisse la travailleur à la merci du patron, tous ces échecs vont accroître le mécontentement et semer les premiers germes d'agitation qui déboucheront sur la grève générale de 1911 dans l'industrie du manteau.

Symbole le plus dramatique de ce demi-échec, l'incendie de l'usine "Triangle" en mars 1911 où meurent calcinées 146 ouvrières, travaillant dans des conditions inacceptables au strict plan de la sécurité matérielle. Ces faits et bien d'autres, F. Basch nous les livre en vrac. Il convient encore une fois de souligner la valeur du travail de l'historienne qui éclaire et restitue toute sa dimension au texte de l'écrivain et de la militante.

Lucie Bullick

Institut d'Études du Développement  
Économique et Social (Paris)

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