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Jean-Pierre Hardy, *Le forgeron et le ferblantier* (Montréal: Les Éditions du Boréal Express 1978).

THIS SERIES, "Collection Histoire Populaire du Québec" as its name implies, is aimed at a general rather than an academic public and seeks to cash in on the current preoccupation of North Americans with their "roots." In this case, the material culture of previous generations is the focal point; previous works dealt with rockers, snowshoes, and fire arms used in the fur trade. This little book departs somewhat from the model in that it gives as much importance to the familiar figures who produced the articles as to the articles themselves. The abundant use of well chosen illustrations which complement the text is a very positive feature of this volume.

In a first chapter, the author gives a concise historical outline of the evolution of the technology of transforming ore into pig iron from primitive times to the eighteenth century. The Saint Maurice iron works which supplied *québécois* artisans with their raw material from the 1730s until the company went out of business in 1883, are dealt with in some detail. A second chapter deals with the artisans themselves. The European origins of the trades, the training apprentices received, the organization of the workshop, and the adaptation of techniques and products to meet Canadian needs, are all covered. The result, based on meticulous research carried out by the author, Thierry Ruddel, and other colleagues at the National Museum of Man, is a coherent picture of working conditions in pre-industrial trades. The two final chapters deal specifically with blacksmiths and tinsmiths. The necessary steps involved in transforming raw materials into finished products are outlined, the tools of each trade are described as are a wide variety of products which these men

created. Finally the author attempts to show how each trade adapted to industrial society: the smithy became a garage mechanic, the tinsmith a hardware dealer or plumber.

This book will go a long way in satisfying the curiosity of the general public, but it misses the mark as truly popular history and gets lost in a no man's land between this genre and academia. One hardly expects critical apparatus in a popular work yet there are copious footnotes. Academics might find the illustrations useful for undergraduate teaching, but will be disappointed by the lack of any explicit analytical framework. They might also cringe slightly at the rather romantic vision of the artisan putting his heart and soul into every piece he made and at the inference that hand made metal products are superior to their modern machine-made counterparts. The bibliography, although quite substantial, is an incomplete compromise with more than enough detail for a general reader, but not enough for an academic audience. For example, Cameron Nish's biography of Cugnet which contains considerable detail on the first years of the Saint Maurice foundry is not included. Relevant publications and theses on individual artisans such as Lyse Cyr and Yvan Chouinard's *Le forgeron Émile Asselin*, published by the Quebec Ministry of Cultural Affairs, should also have been included. Finally, for the general public and more specifically for school teachers wishing to present audio-visual materials, a list of National Film Board documentaries on artisans would be particularly useful in such a book.

This type of semi-popular history is still trying to establish itself on the Canadian book market. If it succeeds in breaking through and in kindling interest in the daily life of our ancestors, it will have served a useful purpose. Jean-Pierre Hardy's contribution to this field, despite

its minor shortcomings, will have set a high standard for others to follow.

John A. Dickinson
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En collaboration, *150 ans de luttes. Histoire du mouvement ouvrier au Québec (1825-1976)* (Montréal: Une co-édition CSN-CBQ 1979).

THE GROWING maturity — some would refer to it as institutionalization — of the Canadian labour movement has found expression in its recent desire to fathom its roots. Heeding the advice of Sidney Webb, organized labour appears finally to have realized that the capacity to proselytize is seriously restricted until the average worker understands and appreciates labour's past struggles. The result has been the publication of several histories over the past 15 years that are directly sponsored by labour organizations. It goes without saying that many of these undertakings can barely aspire to the label of popular history. On the other hand some are meaningful and even sophisticated historical contributions.¹

With the publication of *150 ans de luttes*, we have by far the most ambitious and satisfying of these projects to date. This is a book that belongs on the bookshelf of specialist, amateur, and worker alike. Behind every book there is a history. The story of *150 ans de luttes* itself reveals much about organized labour in Quebec and about the strengths and weaknesses of the book. The idea of the book first took hold in the mid-1970s following the second strike of the Common Front that saw the three major provincial organizations (Confédération des syndicats nationaux/Confederation of National Trade Unions; Fédération des travailleurs du Québec/Quebec Federation of Labour; and the Corporation des enseignants du Québec/Quebec Teachers Corporation) come together again to wrest a satisfactory contract from the provincial government to

cover 185,000 public service employees. This was also a time when spokesmen from the three organizations were also talking — not too loudly — about their hope for one large union to speak for all Quebec workers. The idea of a jointly prepared history hence was a very appealing and supposedly safe collective undertaking.

All agreed that it was time to have a comprehensive, readable, partisan, and accessible history prepared for the Quebec worker. To this end a team of researchers drawn from the educational services of the unions and from the universities² was put together. Louis Fournier, formerly labour journalist with *Québec-Presse* and *Le Jour* was given the imposing task of drawing it all together on paper. For the *imprimatur*, the unions set up their own internal reading committees.

Along the way and as a result of inter-union rivalry in the construction trades, the FTQ decided without much public fanfare to pull out of the venture which in part explains the "limited" coverage that the FTQ obtains in the post-1960 section of the book. (Rumour has it that the presidents of both the CSN and CBQ reserved for themselves *ex cathedra* editorial rights.) Entire sections were consequently re-written and then re-written again to satisfy all involved parties. What had been billed as a collective undertaking at writing history became an exercise in ideological gymnastics that said a great deal about where the labour movement in Quebec was coming from, the direction in which it was aiming, and the potential hurdles in its path. The fact that *150 ans de luttes* still emerged is in itself quite significant.

Whereas other union-backed histories have generally limited their scope to exploring the day to day tribulations and successes of their own organization, *150 ans de luttes* has further attempted, with much success, to relate labour developments to the overall provincial scene. The book is divided into seven chapters, each covering a chronological period. Chapters are then subdivided into a series of over-

views on the economy, on political life, on the organized labour movement, and on the role of labour in politics. At times this approach is a little deadening but the simple, engaging prose that characterizes the entire work saves the day. The text is also richly illustrated. Rarely a page goes by without a well-selected woodcut, engraving or photograph, many of which are being published for the first time.

The study makes no pretence at being a work of original research. It does, however, make full use of existing secondary sources and this explains the chronological imbalance and often spotty treatment that results. Nearly two-thirds of the text covers the period 1930-76 which is understandably well fleshed-in. For the nineteenth century and somewhat for the period 1900-30, however, the text moves rapidly from one event to another reflecting the limited state of current historical knowledge. This said, it must be remembered that we have here the first survey of the Quebec labour scene in all of its richness. The data presented are reliable and the book includes an eleven page chronological table that is of immense value.

Criticism can be leveled, however, on issues touching union ideology. As alluded to above, the coverage given to the FTQ is meagre. This is not to say that injustice or misrepresentation exists, but certainly the international unions in Quebec take a much larger portion of the total union picture than is presented here. If one can be charitable on this assessment, this is not so when it comes to the book's handling of the Centrale des syndicats démocratiques. Born of a schism within the CSN in 1972, the CSD today encompasses an estimated 50,000 members. It receives only two very amorphous paragraphs whereas two pages are devoted to the Communist Party which never had such an impact on Quebec workers.

A related concern touches the "radical à go-go" mentality that results in such travesties as devoting nearly a full page to

the I.W.W. (at best a footnote could have done in this circumstance), while allowing the same space for the pan-Canadian labour movement in Quebec. Granted that the volume was meant to inspire the troops. Yet troops do mutiny when they consider themselves ill-led or manipulated. The CSD lesson seems to have escaped those who should have learned.

Finally the book contains no footnotes and only a brief bibliography. This was willed — after much internal debate — since *150 ans de luttes* was intended as a manual for the ordinary worker, not as a reference work for the university student or professor. This is understandable considering the objectives of the book. It is unfortunate therefore that the authors did not follow through on their statement of making available a separate but more complete bibliography for those who saw the need (235). Perhaps it is not too late to do so.

In conclusion one must congratulate both the CSN and the CEQ for holding to the ideal of selling the book at rock bottom price. For \$5.00 the 235 page, 8 1/2 x 11 inch paperback is truly a bargain and the Quebec worker is responding enthusiastically. A first printing of 6000 sold out in two months. Perhaps the marketing experts of our university presses should take heed.

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¹ Of particular note is Paul Phillips, *No Power Greater, A Century of Labour in B.C.*, Vancouver, 1967.

² The names of Céline Saint-Pierre, Stanley-Bréhaut Ryerson, and Héliane David are well known to students of Quebec labour history.

Bryan Palmer, *A Culture in Conflict: Skilled Workers and Industrial Capitalism in Hamilton, Ontario, 1860-1914* (Montreal: McGill-Queens 1979).

BRYAN PALMER has written a substantial and important book in a field of study that

scarcely existed for Canadian historians 25 years ago. As I set down these words in review of *A Culture in Conflict, Skilled Workers and Industrial Capitalism in Hamilton, Ontario, 1860-1914*, the well-deserved tributes paid to an older and very different kind of history are still echoing in my ears: those offered during the memorial service in Convocation Hall at the University of Toronto for Donald G. Creighton. His was the history of the grand shaping patterns and the outstanding influential individuals of Canadian experience observed forming and performing in the crucible of politics. Although Creighton himself wrote about the "workingman" it was only as a footnote to the life of a pre-eminent hero in the national saga. My own amateur efforts as a graduate student in the 1950s to treat labour history and radical thought in the nineteenth century Creighton greeted with understandable skepticism, seeming to question not only the subject's interest to him but its viability: "You would have to know a great deal more about the period if you wanted to go on in this field." Bryan Palmer and others have been turning up that "great deal more" in abundance and with striking success in the past few years, helping to fill out the broad contours of Canadian political, constitutional, and economic history with the detailed, local experience of ordinary working-class people while at the same time showing that they too have their absorbing interest and their wider significance.

Bryan Palmer quotes Levi-Strauss approvingly on the essential dilemma of the historian: "In so far as history aspires to meaning, it is doomed to select regions, periods, groups of men and individuals in these groups and to make them stand out, as discontinuous figures, against a continuity barely good enough to be used as a backdrop." These words take us to the heart of Palmer's enterprise. Certainly he selects from the "chaos" of "total history," but on a different principle from historians with a constitutional, political or

economic focus, and certainly he does so to achieve meaning, though his approach is more dialectical than structuralist.

Drawing on the growing body of sociological and social history dealing with the workingman, industrial development, and the class system, Palmer attempts to adapt and apply essential theories of empirical Marxism to the way of life — or way of conflict — of the Hamilton skilled workers during the crucial years in which industrial capitalism established itself and became dominant. The "culture" Palmer sets out to examine is the whole array of "associational life" of the nineteenth-century workingman, which drew workers and their families together, gave them a sense of identity and relation, and fortified their will to recognize, protect, and advance mutual interests. Some of this "culture" is still familiar enough; some has almost slipped through the sieve of history and been lost from sight.

The culture of the nineteenth-century skilled workingman embraced a rich associational life, institutionalized in the friendly society, the mechanics' institute, sporting fraternities, fire companies, and workingmen's clubs. Complementing these formal relationships were the less structured but equally tangible ties of neighbourhood, workplace, or kin, manifesting themselves in the intimacy of the shared pail of beer, or the belligerence of the charivari party. Lingering on the edge of this culture, and illuminating its contours, stood events of importance to the community of the skilled: Confederation, marked by celebration and trade procession, self-proclaimed workingmen's holidays, later legitimized by government proclamation, and declared Labour Day; or less momentous happenings, such as the coming of a circus, or the visit of a minstrel troupe. By the early twentieth century, it is true, realms of this culture would be emasculated, if not destroyed. The mechanics' institute, poisoned by the condescension and contemptuous patronage of the city's elite, had withered and died, while professionalization siphoned off much of the cultural essence of the baseball teams and fire companies. Yet the passing of institutions or the sublimation of once specifically working-class

activities hardly signified the obliteration of a culture. Much lived on. . . (38)

Seemingly trivial or insignificant phenomena — funeral processions, fire-fighting, charivari parties, baseball games — take on fresh meaning in Palmer's persuasive argument for the vitality of a class consciousness with cohesiveness and continuity capable of fortifying the skilled workers in their resistance to the eroding effects of industrial capitalism.

Palmer stretches the concept of "culture" — perhaps to the breaking-point — when he goes on to focus on the central scene of conflict, the work-place, and to identify a "culture of control" through which workers tried to meet the employers' determination to dominate and discipline their employees. Nevertheless, it is illuminating to see specific features of worker/employer conflict, from wage and hours disputes to Saint Monday absenteeism, against the larger cultural background and in the context of evolving class consciousness.

The story which Palmer tells has to be seen as ending in a kind of defeat. The skilled worker of the 1860s had by 1914 become the victim of skill dilution, mechanization, and the increasing force of work-place disciplines imposed by expanding industrial capitalism. But on the other hand the continuities of working-class culture were, in Palmer's view, a social force able to cushion the dehumanizing impact of change, bolster resistance, and contribute to the growth and refinement of a revolutionary consciousness of the importance of work-place control. It would be truer to say, therefore, that the story is far from over. Not only does Palmer present a coherent and compelling argument, a fusion of suggestive theory and rich empirical detail, about a neglected aspect of Victorian Canadian social history, but he throws light in so doing on the cultural continuities and conflicts we have inherited and live with today. Although the parallels are more often implied than stated, no one will come away from Pal-

mer's book without an increased understanding of the realities of worker/employer relations in the 1970s and 1980s.

Any book as ambitious and original as this must raise more questions than it can answer. I mention only a few which interest me particularly. In the broad spectrum of cultural life Palmer explores, how important was "word culture," literature, ideas? Who actually read the labour press of the later nineteenth century or the radical press of the early twentieth century (which Palmer largely ignores)? Why is it that while the oppositional press becomes progressively more class conscious (beginning with the *Ontario Workman*, which rejected the notion of class war), Palmer gives the later phases less attention? Why does Palmer end the book with an anecdote or fable of a disenchanted worker motivated by Utopianism to leave Hamilton, yet he rarely mentions that ubiquitous form of idealism which might be thought of as countervailing to the effectiveness of class consciousness? Does Palmer perhaps wishfully over-reach the evidence in pointing forward to "revolutionary class consciousness and political organization" rather than, say, to conservative business unionism in the period beyond the end of his detailed discussion?

My final question is of a more rhetorical nature. One of the chief attractions of this fine book is Palmer's sympathetic, compassionate, perhaps even passionate involvement with the struggling lives of the class he is studying, and with particular life stories (none, of course, as illustrious as those of the Macdonalds, Browns and Blakes of the period) that enter his field of observation. Nevertheless, there is a paradoxical result. Why are we left with a somewhat melancholy impression, an awareness of distance, of loss and failure, of those faceless masses sliding away from the light of imagination and understanding into the shadows of the unredeemable past (whereas Creighton's Macdonald, as a unique individual, lives on vividly in our memories)? Is there something more for

the historian of the working class to achieve, or has Bryan Palmer brought us to the natural limits of this form of social history?

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Fernand Harvey, *Révolution industrielle et travailleurs; une enquête sur les rapports entre le capital et le travail au Québec à la fin du 19^e siècle* (Montréal: Boréal Express 1978).

LE VOLUME de Fernand Harvey, *Révolution industrielle et travailleurs*, récemment publié chez Boréal Express, dans le cadre de sa collection "histoires et sociétés," est d'un intérêt capital tant par la qualité de l'étude historique sur l'industrialisation au Québec que par l'analyse plus large du phénomène du développement du capitalisme industriel au Canada. À partir d'une source très riche, la Commission royale d'enquête sur les relations entre le capital et le travail de 1886, Harvey se livre à une analyse de "la question sociale" de l'époque, ainsi qu'à un examen de la commission en tant que réponse politique aux conditions sociales posées par l'industrialisation. L'importance de ce document historique a été déjà reconnue car il existe une sélection de témoignages publiés sous le titre *Canada Investigates Industrialism*, ainsi qu'un disque comprenant des témoignages fait au Québec.¹ À travers les cinq volumes de témoignages, réunis au cours du séjour de la commission dans tous les centres industriels importants de l'Ontario, du Québec, du Nouveau-Brunswick et de la Nouvelle-Écosse, ce sont effectivement tous les acteurs du monde du travail des années 1880 qui se décrivent: patrons, contremaîtres, notaires, avocats, travailleurs et travailleuses, artisans, enfants, etc. Pour la seule province de Québec, on a entendu un total de 649 témoins dont les propos sont réunis dans deux volumes.

Se limitant à la province de Québec, *Révolution industrielle et travailleurs* se divise en trois parties: la commission du travail, les témoignages et le diagnostic.

Dans la première partie, Harvey analyse les origines de la commission et la situe dans la conjoncture socio-économique. La deuxième section est consacrée, comme l'indique le titre, aux témoignages faits devant les commissaires lors de séances tenues à Montréal (17 jours), Québec (11 jours), Sherbrooke (3 jours), St-Hyacinthe (2 jours), Lévis, Hull et Capleton (1 journée chacune). La troisième partie traite des recommandations contenues dans les deux rapports soumis au gouvernement par les deux factions qui se sont formées au sein de la commission.

Le rôle partisan des commissions royales d'enquête dans la vie politique au Canada est depuis longtemps reconnu et il est peu étonnant que le travail de cette commission fut entaché de partisanerie politique. Du choix des premiers commissaires en décembre 1886 à la rédaction des rapports en 1889, les intérêts du Parti conservateur prenaient une place importante dans les affaires de la commission. Dans les notices biographiques sur les commissaires, Harvey démontre que "toutes les nominations s'expliquent par des motifs partisans, qu'il s'agisse de partisans conservateurs avoués ou d'individus sympathiques aux conservateurs et susceptibles de canaliser le vote ouvrier." (254) Malgré que le choix des commissaires faisait montre d'un effort réel d'inclure des représentants de la classe ouvrière, tous étaient plus ou moins sympathiques au Parti conservateur. Leur hommage à la Politique nationale de Macdonald dans le rapport "pro-syndical" est révélateur de leur attache partisane.

L'examen des témoignages que fait Harvey dans la deuxième partie de son ouvrage nous donne un aperçu de la société québécoise alors en pleine phase d'industrialisation. Les inquiétudes au sujet des changements technologiques, économiques et sociaux exprimés par des ouvriers, patrons, gérants, fonctionnaires et intellectuels constituent "une véritable enquête sociologique avant la lettre." L'auteur étudie les changements des techniques et de l'organisation du travail alors que le mode

de production passe de l'artisanat à la fabrique mécanisée. L'introduction de la mécanisation, la division des tâches, la disparition plus ou moins graduelle de l'apprentissage, l'exploitation d'un nouveau prolétariat, le travail des femmes et des enfants, les conditions du travail, etc. sont décrits à partir des témoignages qui rendent bien compte du développement du capitalisme industriel à l'époque de la commission d'enquête. Poursuivant son analyse, Harvey étudie finalement les relations de travail et les rapports entre les travailleurs et la société globale. L'industrialisation du Québec, conclue-t-il, a eu comme conséquence principale l'expropriation de la force du travail des ouvriers au profit de la bourgeoisie capitaliste. (16)

Malgré leur contenu fort intéressant, les témoignages présentent néanmoins quelques problèmes au chercheur. Du côté des statistiques par exemple, Harvey souligne leur nature contradictoire et confuse. Quant aux témoignages eux-mêmes qui se présentent le plus souvent sous forme de questions-réponses, on est déçu par l'attitude des commissaires qui, en dépit d'un intérêt évident envers les témoins ouvriers, réservaient une attention différente aux patrons et aux travailleurs. Par exemple, souvent les travailleurs témoignaient les premiers tandis que les cadres et les patrons se présentaient ensuite pour confirmer ou nier leurs allégations, ou encore la crainte de représailles empêchait certains ouvriers d'être d'une totale franchise. Plus frustrante pour eux fut la tendance à contraindre les ouvriers à se limiter "aux faits" et de ne pas permettre l'expression d'opinions en dehors de questions spécifiques. Par contre, les commissaires faisaient montre de plus de souplesse face aux patrons. "Les contremaîtres, les gérants et surtout les patrons bénéficient d'une oreille plus attentive. Les patrons, par exemple, sont souvent invités à choisir entre un témoignage sous forme d'exposé ou de questions-réponses." (67) Les témoignages recueillis par la commission de la part de travailleurs ne contiennent donc

pas de remarques sur le développement industriel capitaliste, ils se limitent aux seuls faits observables.

La division des commissaires en deux camps au cours de l'enquête et la rédaction de deux rapports (le premier, qualifié de "philanthropique" au lieu de "pro-capitaliste" par Harvey, déposé le 25 février 1889 [minoritaire], et le deuxième, "prosyndical," remis deux jours plus tard [majoritaire]) reflètent fidèlement les idées sociales du 19^e siècle et donne à l'historien l'occasion de scruter de près l'orientation idéologique des enquêteurs. À première vue, on serait tenté d'interpréter la division des commissaires comme un conflit de classe, ou, au dire de G. Kealey, "a particular case of growing working class identity."² Certes, entre les deux rapports, malgré leur maigreur (chacun n'ayant que dix pages), on peut discerner une insistance plus grande sur les bienfaits des syndicats dans le rapport majoritaire, fait peu surprenant car tous les commissaires venant du milieu syndical se sont rangés de ce côté. Mail il ne faut pas oublier que le manufacturier John Kelly a aussi signé le rapport prosyndical et que, parmi les minoritaires, on retrouve Jules Helbronner, journaliste reconnu pour sa défense de la classe ouvrière montréalaise. Il reçut l'appui unanime du Conseil des métiers et du travail de Montréal même après la remise du deuxième rapport.

La première constatation qui sort de la lecture des rapports est leur manque de vision d'ensemble, de cohérence idéologique. "Les deux groupes de commissaires," rapporte Harvey, "n'ont rédigé qu'une suite plus ou moins ordonnée de constatations et de recommandations portant sur divers problèmes soulevés au cours de l'enquête." (210) L'auteur est donc réduit à comparer deux listes d'observations sur les questions du travail, qui n'ont rien à voir avec un conflit idéologique sur les rapports entre le capital et le travail. Au sujet des organisations syndicales, Kealey releva une différence d'attitude entre les deux camps, qualifiant les commissaires

prosyndicaux de plus "positifs" que la minorité qui favorisa le développement des associations ouvrières "for fewer strikes, more moderation, and the general moral uplift of the working class."³ La partie prosyndicale a ainsi justifié l'existence de telles organisations:

Dans les pays étrangers l'existence des associations ouvrières a donc eu pour résultat de faire naître entre le travail et le capital des relations grâce auxquelles les grèves seront, avant peu, chose du passé et seront remplacées par des conseils d'arbitrage. Tel est le but des ouvriers canadiens. (230)

Les commissaires majoritaires déclarèrent ensuite les associations syndicales avaient lutté contre l'alcoolisme et prêché la tempérance: elles avaient promu l'instruction des ouvriers pour en faire de meilleurs "artisans" et obtenu de meilleures conditions du travail. Ils voulaient projeter l'image d'un syndicalisme conciliant et rassurant, "une force réformatrice au service de la classe ouvrière," (230-1) plutôt qu'une force révolutionnaire.

À propos des contrats de travail, la majorité dénonça l'utilisation du "contrat draconien" (*iron-clad contract*) qui interdisait toute affiliation syndicale à l'employé, pratique que les patrons justifiaient sous le prétexte que l'ouvrier était libre de signer un tel contrat ou de refuser l'emploi. Ce ne fut pas la question syndicale qui a amené la minorité des commissaires à s'interroger sur l'injustice faite aux ouvriers par ces contrats de travail, mais plutôt l'existence de la loi des maîtres et des serviteurs au Québec qui défavorisait les travailleurs. "Néanmoins," remarque Harvey, "chaque rapport illustre de façon différente l'injustice faite à l'ouvrier." (232) Il y a d'autres divergences du même genre entre les deux rapports, et parfois même, c'est la minorité, qualifiée à l'époque de "pro-capitaliste" par la presse syndicale, qui prône la position la plus radicale. Par exemple, sur la question des accidents de travail, apparemment sous l'influence de Helbronner, la minorité réclama avec la majorité un système d'indemnisation des victimes d'accidents et alla plus loin que la

partie prosyndicale en demandant également la reconnaissance du principe de la responsabilité patronale en cas d'accident.

Harvey explique le ton plutôt modéré du rapport prosyndical par son inspiration du programme des Chevaliers du travail "dont on ne sait trop s'il constitue un retour au passé ou une utopie coopérative." (248, 255) La similitude entre l'idéologie des Chevaliers sous la présidence de Terence Powderly et le courant philanthropique de l'époque peut être aussi un facteur d'explication du peu de différences fondamentales entre le rapport des syndicalistes et celui des "philanthropes."

La similitude des rapports porte Harvey à accorder une certaine crédibilité aux allégations de Helbronner qui accusa la majorité d'avoir utilisé le rapport de la minorité pour rédiger le sien. (210, 247) Seuls les annexes dénotent une différence dans les préoccupations des deux parties, la majorité penchant vers la défense des intérêts professionnels des travailleurs et leur opposition aux autres classes sociales tandis que la minorité se préoccupait davantage des questions sociales. Quant aux recommandations contenues dans les deux rapports, on y retrouve en effet sous des formulations à peine modifiées, les mêmes résolutions.

Fernand Harvey voit dans le déroulement de l'enquête et dans les rapports qui en sont sortis, une unité de perspective qui éclipse toute divergence visible entre les deux groupes de commissaires. Leur discours, selon lui, s'inscrit dans un rapport de classe: "la révolution industrielle étant dominée par la bourgeoisie capitaliste, c'est cette classe qui contrôlait non seulement le mode de production, mais également le discours sur l'industrialisation. Cette domination économique et culturelle était telle qu'elle a amené les acteurs sociaux de l'époque à considérer comme allant de soi une situation résultant d'un rapport de classes. Le modèle capitaliste d'industrialisation pouvait ainsi être perçu comme le modèle objectif d'industrialisation." (255) Tant chez les "philanthropes" que chez les prosyndicalistes, ce fut "au

même projet de société qu'on se réfère: la société industrielle capitaliste en voie d'élaboration." (259)

Une des contributions principales de cette étude, bien documentée et bien articulée, porte donc sur l'analyse de la division entre les commissaires, une scission interprétée auparavant comme une division de classe. Harvey fait voir le vrai visage du syndicalisme réformiste à un point décisif de son développement; en poussant plus loin son analyse des deux factions, il contribue à démystifier l'histoire ouvrière canadienne.

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¹ Greg Kealey, ed., *Canada Investigates Industrialism*, (Toronto: University of Toronto Press, 1973); *Commission royale d'enquête sur les relations entre le capital et le travail, 1888*, disque 33 1/3 t., (Québec: Université du Québec, 1976).

² Kealey, *Canada Investigates Industrialism*, xix.

³ *Ibid.*, xviii.

Jack Scott, *Trade Unions and Imperialism in America*, 2 volumes (Vancouver: New Star Books 1978). Volume 1: *Yankee Unions, Go Home! How the AFL Helped the U.S. Build an Empire in Latin America*. Volume 2: *Canadian Workers, American Unions: How the American Federation of Labour took over Canada's Unions*.

THE TITLES of these two volumes convey an accurate image of their scope and major theses. The author, a well-known writer on the Canadian left and a former member of the Communist party, argues that the AFL made an ideological commitment to capitalism at its founding, identified with the U.S. ruling classes, and proceeded over the next nine decades to support imperialist moves by American capitalists north of the 49th parallel and south of the Rio Grande. Scott attempts to straddle a tortuous line between conspiracy theories centering on the deviousness of American trade union officials, and a simplified Marxist analysis which assumes an *a priori* class structure of rulers and ruled. While

the books are meant to be historical, the author views his work "as a contribution to the fight for new policies," (13) thus betraying his overriding purpose and probably accounting for his serious abuse of historical methods. More of this anon.

Scott devotes 80 pages of the first volume to an analysis of the formation of the American "ruling class" in the eighteenth century and to its subsequent evolution over the next century. Rejecting the views of Aptheker and other Marxists who note democratic elements in the American Revolution, he writes only of the imperialist ambitions of eighteenth-century merchant capitalists and southern planters. Their lust for wealth was vented in westward expansion; English restraints upon this thrust provoked a war for independence. With the advent of industrialization stimulated by the War of 1812, capitalists displaced the merchant and slaveholding components of the ruling class, and a working class emerged out of the misery and shock of exploitation. Because of ethnic factors, working-class consciousness remained low in America until World War I; nevertheless in the 1880s a handful of workers debated the merits of craft versus reform unionism, and Scott praises the Knights of Labor for their political and "class" outlook. Unfortunately, skilled labour shaped overall trade union policy and AFL leaders abandoned political solutions for economic ones, forging a business unionism and setting the stage for decades of class collaboration with American capitalists. At the end of the nineteenth century, when the search for foreign markets and raw materials drove American businessmen into imperialist ventures, American unions followed meekly.

With this context in place, Scott turns in part two of his first volume to the role of the AFL in the acquisition of the U.S. empire in the 1890s. Departing from Philip Foner's views, Scott maintains that the AFL extended "loyal support to the ruling class and the policy of imperialist conquest" during the Spanish-American War "while they pleaded for themselves a measure of legitimacy and a role in American soci-

ety." (97) Gompers and his cohorts were racists opposed to Indians, Orientals, Filipinos, and Mexicans. They embraced the "new" imperialism which rejected colonies in favour of economic domination of foreign areas through U.S. corporate monopolies. Thus began what the author calls the "single most significant feature" of American union policy outside the United States — a consistent effort to better American conditions "at the expense of working people in other lands." (88) Five succeeding chapters outline the nefarious role of AFL leaders during U.S. penetration of Cuba, Puerto Rico, Hawaii, the Philippines, and later on behalf of dollar diplomacy in the 1920s and 1930s.

Part three of the first volume, "The Modern Era," carries the same story and judgments from World War II to the 1970s. Nelson Rockefeller used his position in FDR's administration to finance the extension of American trade-union activities in Latin America during the war and postwar years. Rockefeller and J. Peter Grace of the shipping family

desperately needed, and were most anxious to obtain, the services of American unionists to aid in the dragooning of workers and peasants into an acceptance of American capitalist values. And what the American capitalists wanted for Latin America, the American unionists also wanted. After all, had they not embraced and defended American capitalism for nearly a century in tandem with the ruling class? (225)

Later, American unions helped ITT and the CIA to unseat Chile's Allende, and supported the Vietnam War because it stimulated a recession-ridden U.S. economy.

After reading this first volume, most students of Canadian labour history could predict the outline of Scott's second tome. Setting out to "examine how the alliance between the United States unions and American imperialism functions in Canadian conditions," (11) the author establishes a context by citing "typical" imperialistic American thrusts into Canada in 1775 and 1812. In Canada unlike the Caribbean, the U.S. "settled for formal

recognition of the political independence" of Canadians, but still pressed for economic domination, motivated equally by a lust for Canada's resources and a desire to prevent cheap Canadian labour from depressing American workers' wages. With that stage set, chapters three through eight narrate the expansion of American business and labour into Canada at the turn of the century, explaining how the AFL at Berlin, Ontario, in 1902 (and elsewhere) managed to extend its dominance over Canadian workers. Then Gompers, ever the class collaborator, joined hands with the ruling classes to wage World War I and earn big profits, aided by state repression of radicals. An alliance "between international unions, employers and Government" clashed first with "militant industrial unionism and radical politics" (121) in Nova Scotia and British Columbia before the War and afterward in Winnipeg. The potential, he asserts, for revolution was present in the Manitoba capital. "The problem of the bourgeoisie was to stop the struggle short of the working class — as a class — accumulating the political knowledge and experience necessary for them to suppress the capitalists and rule the nation." (142) Making no reference to Bercuson's argument regarding the inherent weaknesses of the general strike as a tactical weapon, Scott places all the blame for the defeat on the strikers' enemies, lamenting only that the workers' class consciousness was "much lower" than that of their capitalist opponents.

Chapters ten and eleven skip briskly over the next 60 years of Canadian labour history in only 70-odd pages. His material on the CIO's activities in Canada is condensed from two chapters of Irving Abella's book. Following World War II, he continues, the AFL pursued the politics of anti-Communism: "dedicated as they are to the preservation of capitalism and United States domination of the world, the international unions have consistently opposed the radical economic and political programs advanced by socialists." (172) He details various conflicts between the

Canadian and American labour movements, quoting heavily (four pages) from a Trades Congress journal article published in March, 1949. He emphasizes the Great Lakes shipping crisis of the early 1960s, and concludes that "the whole history of the Canadian union movement in its Canadian-American relations proves that the fight for 'autonomy' leads nowhere. Only independence, the full and free right to set our own policies, will suffice." (213-4)

Throughout both volumes, Scott quotes extensively from a wide variety of primary and secondary sources, some of which are cited in footnotes abounding with references to trade union journals, proceedings, newspapers, theses, and a variety of books and articles. He concludes with the observation that "sufficient documentary evidence has been set before the reader so as to clearly define the international role of the United States unions in the service of American imperialism," (215) obviously expecting to persuade the reader that his analysis has been derived from extensive research in the sources. A few hours of checking both his text and footnotes has convinced me that there are major problems here as well.

Some of Scott's less egregious errors probably owe their origin to carelessness; others are deliberate. For instance, he attributes a mission by Charles Millard to Cuba in 1957 to an account by R.J. Alexander, referring to a previous citation which turns out to be *The Bolivian National Revolution* (I, 222, footnote 13). Not surprisingly, I could not find the episode either in the book's index or during a brief perusal. More seriously, I discovered that six of the twelve chapters in the second volume appear to be little more than a paraphrased and reorganized version of my thesis, "The A.F. of L. in Canada, 1896-1908: a Study in American Labor Imperialism," completed at Duke and copyrighted in 1970. Scott makes only one citation to the thesis (*sans* its sub-title); in the remainder of his footnotes for these

chapters, he cites my evidence directly without attribution. When he states, for instance, in his text that "further evidence . . . is found in the correspondence of AFL Secretary Morrison for June 1907," (72-3) quoting that portion of a letter which is reproduced on page 236, footnote 79, of my thesis, but without listing my thesis as his source, he clearly implies that he has used the original.

Not only does Scott quote from sources without proper attribution, he also paraphrases to an unacceptable degree. Paragraph by paragraph, Scott's third chapter, titled "Foundations of Empire: Territorial and Economic Expansionism," very closely follows my thesis chapter entitled "The Expansion of American Business and Labor After 1898." Here is an example of both his paraphrasing and his borrowed citations. Scott writes: "In this last decade of the nineteenth century United States exports in the field of manufactured goods more than doubled from \$158 million to \$339 million, and increased yet another \$100 million just after the turn of the century," citing Achille Viallate, *Economic Imperialism and International Relations During the Last Fifty Years*, New York, 1923. (26) In my thesis I cite the same source, with publisher and page reference added, and write: "From 1893 to 1899 the value of American exports of manufactured goods doubled from \$158 million to \$339 million, and shot up another \$100 million in 1900." (Note that 1900 was *not* "just after the turn of the century," the 20th century having begun on January 1, 1901). Again Scott, p. 31: "And as early as 1890, a Royal Commission reported that over one-half of the capital invested in mining operations in Ontario was American." My thesis, p. 74: "As early as 1890 a Royal Commission noted that over half the capital in Ontario's mining operations was American." Scott, p. 26: "Business journals in the United States began to discuss the economic advantages of expansionism prior to the Spanish-American War." My thesis, p. 64:

"Business journals began to talk of the advantages of expansionism in the years before the Spanish-American War." These are only samples of dozens that could be cited; if Scott were my student and submitted this work as his own, he would fail my course.

In some cases, Scott went back to the pages of a source I cited and excerpted more lengthy quotations, sometimes without identifying his (my) source. Brooks Adams, for example, is labeled only as "a leading political economist of the time" (which many would dispute), and not mentioned by name. (26) Honoré Mercier is not revealed by Scott to be the author of a letter to the *New York Times*, 18 September 1904, purporting to disclose "information regarding secret discussions on the subject of annexation between Canadian politicians and United States capitalists led by Andrew Carnegie." (29) I used this source to illustrate something Carnegie was *alleged to be thinking*; Scott *twists it around* in order to use Mercier's uncorroborated assertion as proven fact.

While borrowing my sources and considerable sections of my narrative, Scott rejects many of my conclusions in favour of his own. For instance, he declares on pages 47-8 that "AFL correspondence shows that [organizer John A. Flett] was being used to a greater extent in the U.S. than in Canada." My thesis clearly shows that Flett worked among Canadian workers between 1900 and 1914 except for brief forays into New York and Pennsylvania, and a longer period between 1913 and 1914 in Buffalo. My judgment is based upon reading weekly exchanges between Flett and AFL leaders over 14 years; Scott's conclusion is apparently based upon his desire to paint the AFL's record as black as possible. Even worse is Scott's clear inference that his conclusion stems from a study of AFL correspondence.

In his haste to condemn the AFL, Scott totally ignores my chapter outlining the contribution of Flett and his mentors to the organization of Canadian workers at the

turn of the century. Instead he chooses to write that "the organizing work in Canada was carried out by Canadian unionists — mostly unpaid volunteers." (53) No footnote follows this claim; on the other hand my thesis details the hundreds of locals organized by Flett. On the same page he asserts that AFL expenditures in Canada were "more than amply" covered by dues collected in the Dominion; the appendix to my thesis proves that his assertion is not true for the years from 1901 to 1907. In some places, Scott follows the same general trend of my argument, modified by his own condensation, simplification, or exaggeration. In short, much of the analysis in volume two is not based upon a diligent search of archival sources, but was arrived at by the author long before he began his "research." His quotations and footnotes are largely borrowed from secondary works and arranged to create the appearance of an argument firmly rooted in historical evidence.

Apart from all of this both books also suffer from imbalance, distortion, and datedness. Large and complex subjects such as the rise of industrial capitalism are given only the briefest consideration (4 pages), while the annexation of Hawaii receives three times as much space. Racist and WASPish attitudes common in the 1890s are ripped out of context; in a later period, both the Axis and Communists threats to the Western Hemisphere are treated as foolish myths. Important secondary works, such as Bercuson's on the Winnipeg strike or Harvey Levenstein's important study of AFL relations with Mexico, are ignored. Occasionally, the author's argument is riddled by an internal contradiction, as when he fails to explain how a union movement which never commanded the loyalty of more than 20 per cent of American workers (noted on page 12 of volume one) could wield so much power in the name of the working classes. His implied answer is reductionist; workers lacked sufficient political consciousness — an item, you will recall, which this

book is designed to supply. I'll bet it doesn't.

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Michael J. Piva, *The Condition of the Working Class in Toronto 1900-1921* (Ottawa: University of Ottawa Press 1979).

THE YEARS between the late 1890s and 1930 witnessed generally strong and sustained economic development in Canada. While British historians have recognized that rapid industrial expansion has often coincided with static or declining living standards for most workers, Canadians have largely assumed that the prosperity of this era was widely shared. Terry Copp's *The Anatomy of Poverty* indicated that Montreal workers did not benefit directly from economic growth. Michael Piva set out to test Copp's findings with respect to Toronto workers between 1900 and 1921. Like the Montreal study, he uses government reports and documents as well as census data to good advantage. His focus, Piva declares at the outset, is working-class living standards. Reform efforts are discussed only insofar as they directly affected labour's condition and dealt with general problems confronting working people. Above all, Piva stresses that he does not attempt to grapple with working-class cultural, social or political life. A study of the latter must first be grounded in "as complete an understanding as possible of the structure of the work-force and the material conditions of daily life which obtained during these years." (x) While Piva has succeeded in enlarging our understanding of this structural and material groundwork, there are a number of criticisms to be made of his analysis.

The core of the book is the lengthy chapter on real wages, devoted to proving the sad state of Toronto's working-class living standards between 1900 and 1921. Piva first determines that in 1921 the average annual earnings of blue-collar workers gave them only 63.5% of the income

required to elevate a family of five above the poverty line. He then measures the development of average real incomes of construction and manufacturing workers in the 1900-1921 period, when the cost of family budgets rose more than three times. Absolute increases in wage rates were usually offset by inflation and lengthy and frequent unemployment, which were particularly severe during 1906-08, 1912-14, and the post-war period. "In the vast majority of cases," he concludes, "real annual earnings were, with the exception of 1917-1918, lower in 1920 than at any time since 1901 . . . only one conclusion is possible: workers did not benefit from the wealth they toiled to produce." (58) Although Piva asserts elsewhere in his book that the standard of living "remained static" (27), he concludes his study of real wages by claiming that not only were workers "very poor," but "they became progressively poorer as the years passed." (58)

While his figures indicate the generally unimproved nature of real annual earnings in 1921 relative to 1900, one should be skeptical about the validity of the measurements used in the study: the standard of the male wage earner supporting a family of five is rather arbitrary, and the base and termination years of 1900 and 1921 are weighted in favour of Piva's argument, since 1900 was in an expansionary period for unionism and the economy, while 1921 was not. In this context, real wages can be seen as varying widely during these years, rather than simply unimproved; 1921 is an artificial terminal date. Moreover, the effect of a second (or third) wage earner on family income is not sufficiently acknowledged. Unimproved real wages do not necessarily mean unimproved living conditions. The data, as Piva cautions, are more comparative than "precise."

The dismal picture of labour's plight is not relieved when the account turns to living conditions. Unemployment was a common seasonal and cyclical experience for many workers, and a perennial experience for others. Relief systems were very

inadequate, and workers were as vulnerable to privation at the start of the 1920s as they had been 20 years earlier. Dreadful working conditions were ameliorated only in a very limited and gradual manner. Few employers improved factory surroundings, and those who did were interested merely in increased labour productivity. Poor public health and housing hampered labour advances. While some reformers aggressively campaigned to better these conditions, "their efforts produced only limited results" (141), since there was no frontal attack on poverty, the essential cause of miserable conditions. Moreover, reformers were motivated in large part by "welfare capitalism." Piva's description of working-class life and his analysis of reformers' motives are useful as far as they go. But his narrative is sometimes too episodic, a result perhaps of his virtual neglect of the daily press. Insufficient attention is paid to the disruption and activism stimulated by mechanization's deskilling of workers and their consequent loss of shop-floor control. Then, too, many readers will question whether Toronto's economic leaders did have such complete and thorough dominance over the labour market and living conditions. Working people had more leverage and more cards to play than Piva recognizes.

The final chapter on industrial relations refers to labour leaders as "militants without power." Beseet by vigorous employer opposition, organizational obsolescence, and unfavourable labour-market conditions, trade unions, according to Piva, were as a rule ineffectual and "powerless" (143), and "declined relative to population in size and effectiveness if not in militancy" after 1902. (170) While there is no denying the serious obstacles and setbacks faced by labour during this period, Piva's assessment of labour powerlessness is substantially exaggerated. Statistics on the number of "defeated" strikes relative to "successful" strikes are hardly conclusive evidence of union decline, given labour advances through peaceful settlements,

arbitration, or government intervention. The presence of both corporate repression and "welfarism" attests to the significance of the challenge posed by labour during this era. The weakest assertions concern the alleged political impotence of labour; Piva dismisses labour's political clout without providing virtually any evidence beyond the District Labor Council minutebooks, an inadequate source for studying Toronto labour politics. Labour was, despite internal divisions, consistently and strongly involved in a variety of political activities during the period, which occasionally paid dividends, not only through the election of unionists such as Simpson and Robbins, but also through less glamorous cooperation with sympathetic politicians outside union ranks. Piva's standards of political efficacy are apparently at once too exclusive and too one-dimensional.

The Condition of the Working Class in Toronto is valuable because it widens our awareness of various factors contributing to working-class economic hardships, and because it places the experience of poverty at the centre of early twentieth-century Canadian life. But the labour movement, political activity, working-class social institutions, and mutual aid were forces that shaped labour's living standards much more extensively than Piva grants. His failure to consider "the social and political implications of working-class life," and his denial of the existence during this time of a "subjective sense of class identification" (x) except in embryonic form, contribute to the book's rather fragmentary and lopsided character.

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Pat and Hugh Armstrong, *The Double Gheto: Canadian Women and Their Segregated Work* (Toronto: McClelland and Stewart 1978).

FOR FEMINISTS struggling to change a society where women serve as a special caste of exploited labour, *The Double Ghetto* offers little that is new and certainly provides no solution to the oppression of women. The book, however, does contain a useful survey of recent statistical and theoretical literature on women's work. If nothing else, it should lay to rest any false complacency about the "improved" status of women.

The Armstrongs show that, although there has been a remarkable growth in female labour force participation in Canada since World War II (from 20% in 1941 to 45% in 1976), women still occupy the low-paying, low-skilled, dead-end jobs. The segregation of women into secretarial, sales and service jobs permits employers to pay them at a uniformly low rate, makes a mockery of equal pay legislation and union membership, and explains why the gap between the average wages of men and women has increased since 1941.

The authors trace the roots of job segregation to the domestic sphere. The chapter on "Women's Work in the Home" is the best part of the book. There it is argued that women's role as producer of goods and services for the family has been gradually undermined over the last century by the effects of industrialization. This process has transformed the family and transferred much of women's traditional responsibility for domestic crafts and child-rearing to the factory and school. Unpaid, unspecialized and increasingly isolated with her "labour-saving" devices, the housewife became a new category of worker — a feudal anomaly in a world where men who did not own their means of production survived by selling their capacity to work. While housewives were denied the wages of proletarianization, they reaped all of its penalties: routinization, insecurity of job tenure, anger and alienation. Studies cited in the book show that middle class housewives who stay at home are more likely to beat their children, experience menopausal depression, and

suffer nervous disorders. There is a special hell, sanctioned by the very system on which their husbands thrive. Meanwhile, those women forced by economic necessity and isolation into the work force end up doing two jobs, one in the public and another in the domestic sphere, neither of which is satisfying. This, then, is the "double ghetto" that women increasingly experience.

Two chapters of the book are devoted to dispelling the biology vs. culture explanations for women's condition. Neither "inferior" biology nor "bad" ideas, the authors argue, are sufficient to explain the persistence of women's unequal status. This preoccupation with the "straw men" hoisted by misogynists is perhaps still required reading for those who have not been exposed to feminist thought over the past decade, but it unnecessarily diverts this study into arid channels that were charted and abandoned some time ago. Unfortunately, the authors do not explore fully the materialist explanations that they conclude are the most productive means of understanding women's oppression. It is not difficult to accept their hypothesis that women remain in their inferior position because it is in the interests of the prevailing system that there be a cheap and elastic labour supply. However, their brief discussion into women's consciousness and its relationship to what women do deserves more thorough consideration as do the various theories advanced by feminists on the nature of power (in the family and the workplace) and the role of women in the wider struggle against capitalist oppression. Further, although the authors dismiss the Report of the Royal Commission on the Status of Women and the C.D. Howe Institute's publication, *Opportunity for Choice* (1976) as idealist and reformist, their own conclusions are surprisingly thin. In one paragraph they suggest that structures designed to eliminate discrimination against women are indications that the contradictions in women's work are producing results; in the next paragraph

they warn that "reformist adjustments" designed to respond to the challenges of the women's movement may prevent significant changes in women's consciousness. While there is no contradiction in these conclusions, they do deserve more penetrating analysis. If "reforms" weaken the collective consciousness of women's oppression (surely a debatable point), why, then, is it necessary, as the authors contend, that "radical alterations" take place in structures and ideas? Of course, this touches on a debate that has been carried on in Marxist and feminist circles for some time now. While the authors draw upon the literature of this debate, they curiously avoiding confronting it directly.

In the final paragraph of the book, the Armstrongs conclude that more analysis from a materialist perspective is needed. Clearly this is so.

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Lorna R. Marsden and Edward B. Harvey,
Fragile Federation: Social Change in Canada (Toronto: McGraw-Hill 1979).

THE AUTHORS have written a textbook for undergraduates summarizing the theoretical and empirical literature on social change in Canada from a political economy viewpoint. They suggest two parameters which both limit and stimulate change: economic structures associated with staple extraction in Canadian historical development and the population profile of Canadians. On this basis, they examine institutionalized and non-institutionalized change. Institutionalized change is centered in elites (and to some extent class), regional interests, and ethnic and linguistic cleavages. Non-institutionalized change occurs in three types of social movements: the self-help approach of nineteenth century Canadian trade unions; the "cause" approach of the women's movement; and, the "revolutionary" movement of Quebec nationalism. Marsden and Harvey attempt to structure the analysis of change in the

entire book by evaluating three theoretical approaches to change: the structural-functionalist tradition associated with the American Talcott Parsons; the Marxist analysis of class and relations of production; and, the world-systems approach espoused by Daniel Chirot. The authors' loyalties quite clearly rest with the world systems approach, although not without certain reservations.

The strength of the book is its diversity. Change is viewed broadly, not narrowly. Economic parameters are considered through the works of Harold Innis and Gilles Pacquet; their social correlates are discussed through the writings of S.D. Clark, and the Quebec writers, notably Hubert Guindon and Phillippe Garigue. Canada's population is viewed in terms of immigration, internal migration, age, sex, births, deaths, ethnic and religious group composition, and changes in the labour force. Institutionalized change ranges from the economic bases of elites and class to the cultural heights of ethnicity and language. Social movements range from the 1872 printers' strike to the realignment of union forces underlying Quebec nationalism in the 1960s and 1970s, from the suffragettes in the early twentieth century to the varieties of feminism in the 1960s and 1970s, and from the conservatism of Levesque's sovereignty-association to the radicalness of the FLQ. But the diversity of the book is also its weakness. At times, it seems that everything is changing and everything is equally important in change. Although there is an attempt to trace a common thread throughout the chapters by evaluating the three major theoretical approaches to change, the substantive discussion in many chapters often bears little relation to any of the three approaches. The chapter on population is strong if left to stand by itself; equally strong in the same sense is the chapter on social movements. But the undergraduate student will have considerable difficulty in attempting to relate these two chapters to one another and to the other chapters.

As sociologists, Marsden and Harvey make some surprising statements about elite and class. Elites are viewed, much more than class, as dynamic sources of change, despite the conservatism of elites and the rich theoretical and historical literature (both international and Canadian) tracing social changes to class tensions between capital and labour. Along with a few other authors, they situate the economic elite within the capitalist class, despite the entirely different theoretical traditions from which elite and class emerge. A more forceful argument can be mounted that class and elite are theoretically irreconcilable. They also state that "class interests are expressed through economic and politic elites." (131) Although this may be true for the capitalist class, it is hardly true for the working class. They argue that John Porter, author of *The Vertical Mosaic*, "focuses on a revised Marxian interpretation of classes" (132), and then follow this with the statement that "the measurement of social class is modified by Porter to include socioeconomic strata as measured by the criterion of occupational membership." (132) Porter did not develop a Marxist approach to class (in any sense of this term), nor can strata be blended with Marxist classes without considering their important theoretical differences. Perhaps one reason for their de-emphasis of class struggle as an agent of change lies in their failure to discuss two of the most important Canadian Marxist analyses of change: H.C. Pentland's treatment of the transition from slave to feudal to capitalist labour markets, and Leo Johnson's portrayal of the transition from toiler to petty commodity to industrial capitalist modes of production. A much more careful perusal of the rich studies being completed on Canadian labour history might have led Marsden and Harvey to give more credence to the tensions between employers and employees.

Marsden and Harvey make a number of statements and assumptions which some readers might question. They argue that

"Canada needs workers" and go on to list the types of skilled workers needed for different projects. (92) They further argue that "new technologies... required the importation of skilled workers...." (93) Might some readers not want to seek out the bourgeois class interests lying behind this need for a labour supply displayed apparently by nation and technology? Has not Canadian labour been much more concerned with restricting the immigration of workers to lessen competition on the labour market and to maintain an "adequate" standard of living for themselves? The authors also argue that "... multinational corporations are very important in the economy of Canada." (93) An executive of Exxon Corporation might argue this way; would a Canadian nationalist, or a worker who has just lost his job by the shutdown of an American branch-plant, argue this way? Marsden and Harvey state that North American societies are "less rigidly stratified" than European societies. (93) Although this makes good ideology, it does not make very good social science. Much research contradicts this assumption.

The authors also suggest that "the union movement in Canada was much strengthened... by affiliation between Canadian and American union groups...." (181) What about the thesis by Pentland and others that such affiliation helped to weaken the solidarity of Canadian labour by driving a wedge between craft and industrial unionism? In reference to India, the authors argue that "the people most deeply involved in bringing industrialization and economic development to the area are outside the political process and may exert their influence in more direct ways on the political elites." (136) The facts are contrary: the Indian state maintains control over the leading sectors of the economy, with a resultant weakening of the indigenous bourgeoisie.

A much more theoretically integrated approach to social change in Canada, with closer scrutiny of historical materials on

change, would have served students and instructors better than the present compendium of a mosaic of changes. They do argue that they are trying to take us closer to a theory of change, and in this they place considerable emphasis on Canada's dependence on the United States. But the concrete richness of changes in Canada appears to overwhelm their capacity to make tough theoretical decisions on a systematic approach to social change.

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John Crispo, *Industrial Democracy in Western Europe: A North American Perspective* (Toronto: McGraw-Hill Ryerson 1978) and *The Canadian Industrial Relations System* (Toronto: McGraw-Hill Ryerson 1978).

THE PUBLICATION of John Crispo's two most recent books gives us cause to reflect on the current state of industrial relations in Canada. Despite recent developments such as the Ontario government's new Quality of Working Life Centre and the Oil, Chemical and Atomic Workers — Shell Oil participative management experiment, Canada lags behind western Europe in the area of industrial democracy. The debate over industrial democracy is mounting on this side of the Atlantic, yet comparative research is only beginning. *Industrial Democracy in Western Europe: A North American Perspective* is a step in this direction. At the general level of teaching and research in the area of industrial relations we are also at a relatively nascent stage. *Industrial relations* as a specialized focus of inquiry came into its own with the Prime Minister's Task Force on Labour Relations in the late 1960s. Marking the evolution of industrial relations since then is Crispo's *The Canadian Industrial Relations System*. As the first comprehensive text in the area, this book fills a large gap in industrial relations teaching at Canadian universities.

Let us turn first to Crispo's study of European industrial democracy. The author tells North Americans that "there are a wide variety of hitherto unexplored channels through which unions and workers can participate in a meaningful way in decision making at all levels in the socio-economic-political system." The central message of the book, then, is that we can learn much from the European experience in this respect. The author criticizes the North American obsession with collective bargaining as the "ultimate in industrial democracy." The survival of unions and collective bargaining depends, in Crispo's view, on reforms in the direction of greater employee participation in work-related decisions and more consensual relations between labour and management.

Readers anticipating a scholarly review of research may be disappointed. Instead, Crispo presents a readable, impressionistic essay based largely on interviews he conducted with labour, management, government representatives, and other observers of industrial relations in ten western European nations. The book begins with a discussion of approaches to collective bargaining, indicating how European approaches differ from ours. Details are then provided on collective bargaining in each of the countries covered in the study. Emphasis is placed on the growing role of labour in national policy formulation, worker-management administration of public welfare and pension programs, works councils and worker participation on corporate boards. The book ends with chapters considering the problems associated with these plans and the general implications of European-style industrial democracy for North America.

Crispo notes that industrial democracy can mean anything from worker involvement in managerial decision making within the firm to the extension of political democracy at all levels of society. The book focuses on the former. While "economic" democracy — where workers share in the ownership of a firm — is con-

sidered, Crispo's central interest is in schemes which involve unions in decision making. North American unions should be aware of the "micro-macro trade-off" made by some of their European counterparts. Restrained demands at the firm or industry level have been traded off for a stronger union voice in national socioeconomic policy formulation. Examples include Austria's corporatist labour-management chamber, West Germany's central manpower and social security programs, and Sweden's National Labour Market Board.

Important developments have also occurred at the company level. For example, the 1967 Swedish Democracy at Work Act indicates that unions are becoming more responsive to rank-and-file demands. Crispo asserts that "recent Swedish experience would seem to suggest that an expansion of information rights and an enlargement of the scope of collective bargaining, together with a narrowing and shrinking of management prerogatives, can provide a potent means of extending the influence of organized workers in enterprise decision making." Yet the future of this trend is far from certain. Worker participation on corporate boards, to take one application, is a highly contentious issue. Unions embarking on such plans run major risks: co-optation, creating a new elite of worker representatives, and compromising their interests by yielding to demands for greater responsibility which accompany decision making power.

This book is useful because it summarizes European developments in industrial democracy for a general readership. Academics may take issue with the text on several counts: the absence of a systematic review of relevant research; the often superficial treatment of the applicability of European plans on this side of the Atlantic; the exclusion of Yugoslavia; an overly brief assessment of how industrial democracy actually operates in the work place; and little analysis of semi-autonomous work groups. In sum, if the recent appoint-

ment of UAW president Douglas Fraser to the Chrysler board is any indication of the direction of change in North American collective bargaining during the 1980s, then anyone likely to be affected by this process will benefit from reading Crispo's book.

Now to consider Crispo's other book, *The Canadian Industrial Relations System*. A textbook designed for university industrial relations courses, it combines elements of both a reader and a standard text. Crispo has organized a vast range of material — much of it culled from non-academic sources such as government, union and management publications, and newspapers — linking it together with his own brief commentaries. The book's seventeen chapters are arranged into six sections: an introductory statement outlining a systems approach to industrial relations; environmental factors shaping industrial relations; the major parties involved in the system; the processes of interaction between the parties; the results of the system; and current issues and future prospects.

Crispo defines industrial relations as everything from individual employee-employer relations through to all forms of collective bargaining, the union movement and its activities. The book's coverage of the area is comprehensive to say the least. Thus only a few of the important issues dealt with can be mentioned here.

The chapter on the constitutional and legal system is a clear, concise summary of the complex tangle of labour law which, as Crispo argues, has resulted in a highly fragmented approach to industrial relations. Crispo goes to some lengths in the various sections on the labour movement to contrast the differences between developments in Quebec and the rest of Canada. It can be fairly said, though, that the analysis of the "radical cast" of the QFL and the question of why "class war" in Canada seems to be concentrated in Quebec could be more probing. The history of the trade-union movement is interesting if only because much of the

material is taken from official union publications. Labour's political role is also examined, with Crispo suggesting that solid support of the NDP would be a "shrewd investment" for unions. The topic of arbitration is handled well. Crispo traces the roots of third party intervention back to W.L.M. King, quoting from *Industry and Humanity*. Compulsory arbitration is treated in a lively fashion, with the juxtapositioning of opposing positions setting up a debate. (This technique is effectively used elsewhere in the volume.) Finally, another contentious question discussed is that of international unions and greater Canadian autonomy.

Given the mass of material and the range of topics covered, there are bound to be weaknesses. While not necessarily detracting from the usefulness of the book as a teaching resource, examples of these are nonetheless worth pointing out. To begin with, the historical patterns of industrial conflict are not explored as a means of shedding light on contemporary labour problems. The Winnipeg General Strike, to take one example, is only mentioned in passing. Second, the discussion of legislation could be augmented by an examination of actual practice, including management's use of injunctions, the role of the state in strikebreaking, and whether legislation inhibits or facilitates organizing. (The assumption implicit in the book is that the state is an impartial arbiter and protector of the "public interest.") Third, much of the discussion of white-collar unionization focuses on the CLC's abortive and now virtually defunct Association of Commercial and Technical Employees. An analysis of ACTE's failure and a comparison with successful white-collar campaigns in the public sector would be more useful.

There are further problems which deserve attention. The book's organization and its general theoretical orientation is derived from the systems model of industrial relations presented in the final report of the Prime Minister's Task Force on

Industrial Relations, *Canadian Industrial Relations*. Industrial relations is thus defined mainly in terms of the institutions and processes of collective bargaining, to the neglect of the daily struggle for control over the labour process on the job. Furthermore, the book's conception of economic development is borrowed from modernization literature. As such, it does not convey the organic historical links between capitalist development, forms of work organization, the labour market, and the rise of a particular type of trade union movement. Both of the above criticisms are tied to the relatively descriptive use of social class as an explanatory variable. The point is that a stronger, overarching theoretical framework would alleviate some of the present difficulties and better integrate the text's materials. Also helpful in this latter respect would be the use of dominant themes interwoven throughout the text, more detailed transitional statements linking the readings and summaries at the end of each chapter.

In short, both books are timely contributions to Canadian industrial relations literature. By way of conclusion, it is interesting to note the diagnosis of current industrial relations problems and the forecast for the future which they contain. In both volumes Crispo asserts his commitment to capitalism, unions and collective bargaining. However, he quickly points out that the entire system requires reform. Capitalism needs to be humanized, unions must be held more accountable for their actions, and union-management relations must develop along more consensual lines. Crispo proposes a more rationalized system of checks and balances to correct present abuses in the system and guard the public interest. While ultimately concluding that "more of the same" will characterize industrial relations in the future, Crispo nonetheless detects a move in the direction of corporatism. This trend is discernible in some of the European industrial democracy schemes. Moreover, Crispo envisions greater state regulation of indus-

trial relations in Canada. This, he observes, will inevitably strengthen the CLC and employer groups. That pluralist ideology can lead to corporatist practice is evident in Crispo's proposal for a national economic and social consultative body comprised of representatives from labour, management, and government as well as from consumer, farmer, and professional organizations. This trend may well be one of the great challenges facing organized labour in the coming decade.

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Roger O'Toole, *The Precipitous Path, Studies in Political Sects* (Toronto: Peter Martin, 1978) and Richard Fidler, *RCMP, The Real Subversives* (Toronto: Vanguard Publications, 1978).

DURING THE period of the late 1960s, two types of sleuths were snooping around Marxist organizations like the League for Socialist Action (LSA). Roger O'Toole was posing as a former Trotskyist from Britain and a potential recruit to the LSA, when he was in fact preparing a University of Toronto doctoral dissertation under the direction of Lewis Feuer. His findings on the LSA and two other organizations have now been published as *The Precipitous Path*, and are presented as the results of guileless "participant observation." The other sleuths, under the direction of the RCMP, may or may not have conducted "participant observation" studies; to date it can only be proven that they stole information from OHIP records to spread gossip about members, blackmailed a homosexual into informing on the organization, and were involved in far-right Western Guard vandalism against the LSA headquarters. The RCMP's scholarly findings have not as yet been published in a form available to the public; however, successors to the LSA have published an exposé on the RCMP, *The Real Subversives*.

O'Toole's observations on the three groups he studies — the Internationalists,

the Socialist Labour Party (SLP) and the LSA — are mostly informative, interesting and shrewd. To this extent, the book is valuable as a manual along the lines of the *Joy of Sects*. Nevertheless the book as a whole is dishonest, superficial and wrong-headed and does not even begin to approach what every labour historian needs to know about sects.

O'Toole studied his three revolutionary organizations of late-1960s Toronto not as examples of Canadian social criticism, not as illustrations of minority tendencies within the labour movement, not as expressions of the youth radicalization of the time, but "essentially as examples of the left-wing political sectarianism." If everything cannot be measured according to this narrow, ahistorical schema — what O'Toole calls a measuring rod — we are reminded in a most sociologically scientific way that the "value of the ideal type [such as "sect"] as a methodological device is its distance from empirical reality." We are left to imagine how O'Toole would classify a sectarian socialist who claimed that "the value of Marxism as a methodological device is its distance from empirical reality."

In his introduction, O'Toole refers to two major schools of thought on political sects — the Marxist school, in which he includes such normally overlooked practitioners as Lewis Feuer and Daniel Bell but excludes such classic writers as Lenin; and the collective behaviour tradition, which is based on the writings of Robert Park and other members of the "Chicago school." O'Toole claims to view both traditions as equally insightful and claims to embark on his case studies eager to test and apply the insights of both. In fact, he draws almost entirely from the writings of Park and Lewis Coser, the latter of whom narrowly missed being defined as a Marxist. These writers define sects as "intransigent and irreconcilable" rather than "accommodating and compromising," and look for such trademarks as persecution complexes, superiority complexes, and sup-

pression of individuals' personalities. As is readily apparent, these terms are codewords for Marxism, at least as it is viewed by cold war liberals and professional anti-Marxists like Feuer, Bell and Coser. Marxists, after all, believe that upholders of capitalism are hostile to them, that their analysis of society is superior, and that accommodation with capitalism runs counter to the historic interests of the working class. Since O'Toole never clarifies a distinction between Marxism and sectarianism, and never assesses sectarian politics as a deviation from Marxism, we can only assume that sectarianism is O'Toole's catch-all phrase for Marxism. Those familiar with studies of the CCF-NDP, which classify confrontations between grass-roots socialists and parliamentary reformists as evidence of the tension between "movement" and "party," will recognize the technique of attributing political differences to worldly versus otherworldly social psychologies, and thereby reducing political differences to political deviance.

Apart from the author's political bias which arbitrarily equates Marxism with sectarianism, the book does not advance precise political criteria for the study of sectarianism. There is virtually no discussion of the three groups' programmes and strategies. Yet surely, a group's sectarianism, as distinct from its Marxism, can only be defined by reference to its strategic and programmatic misapplication of Marxism, which Engels long ago defined as a guide to action rather than a credo. Political strategy, not social psychology, was fundamental to both the born-again Maoism of Hardial Bains' Internationalists and the splendid isolation of those who repeated the SLP catechism. The SLP is politically opposed to reforms, to unions, and to labour parties like the NDP. The SLP thereby deprived itself of agitational issues and audiences available to Marxists, and it is for this reason that their political literature was both abstract and aimlessly distributed at shopping plazas rather than

union meetings. The Internationalists likewise showed "hardly any brains" in their analysis of the Canadian political situation. Their make-believe mass organizations, bi-monthly *Daily People*, and self-flagellating personality style rested on a political analysis common to Maoist organizations around the world, and analysis akin to Stalinist policies in the 1928-35 (third) period. Revolution was on the horizon, and was dependent on the proclamations and will of revolutionaries rather than the objective level of the class struggle. (This political analysis had sectarian attributes, but could be classified more usefully as "ultraleft," an over-estimation of the level of working-class radicalism which Lenin defined as an infantile disorder. O'Toole, of course, is not concerned with such distinctions.)

In the absence of political criteria, O'Toole becomes obsessed with the self-conceptions and internal patterns of social interaction in the groups whose members he studies. To his evident satisfaction, he analyzes normal and even trivial features of political organization as peculiarly sectarian. Thus, if radicals think they are correct, they are defining themselves as part of a "cognitive and moral elite." If all members agree on a basic political outlook, membership is "exclusive." If members try to situate their organization historically, they are concerned with "apostolic succession." If the group involves itself in ongoing political struggles, "esprit de corps is cemented by conflict with perceived enemies." Whatever the activity or belief, O'Toole can encompass it in his "sect-22" schema. If groups attempt to win new adherents, they proselytize; if they do not, they are inward-turning.

Although all three groups which O'Toole studied meet his standard for sects, he is most uncomfortable in designating the LSA as a sect, suggesting that this group had some potential to become a party. O'Toole appreciates that this group conducted itself in such a way as to minimize radicals' cultural differences

from the working-class milieu, so that it could focus more effectively on political work. Oddly enough, given O'Toole's fascination with such secondary social features of a socialist organization, he overlooks some of the most obvious LSA efforts to rid itself of "offbeat" characteristics. Despite bohemian and countercultural tendencies common to the radical milieu of the time, LSA members were readily recognizable at radical gatherings by their short hair, "straight" dress and rejection of "dope."

There are political reasons for these characteristics, however. The LSA was politically oriented to the working class rather than to some new left intellectual-lumpenproletarian alliance, and it was attempting to implement a "transitional strategy" which would allow it to intervene in "mainstream" reformist labour and social movements. Within the anti-Vietnam war movement, it fought for a "single issue" approach, as against "sectarians" who insisted that opposition to the war had to be coupled with other "exclusive" radical beliefs. It recognized and supported the NDP as the vehicle for independent working class political action in Canada. Moreover, as its name implied, the *League for Socialist Action* was quite consciously not a party but a league, an educational organization which constituted one tendency within the larger labour movement. In this fundamental aspect, the LSA was not a sect, despite its small size and minority political beliefs. It conformed more closely to what O'Toole described as a "faction," which "considers itself only a part of a wider movement." Given this political self-definition, the social characteristics of the LSA were inevitable, but obviously not primary.

O'Toole's schematic use of the "sect" ideal-type is not only apolitical, it is ahistorical. Marx, according to O'Toole, underestimated the trans-historical character of the sect phenomenon when he projected the disappearance of sects once a mass radical labour movement developed.

Certainly the experience of the 1960s youth radicalization, the contours of which O'Toole does not even refer to, does not refute Marx. It was precisely the low level of working-class radicalism, so low that even the NDP leadership usually stood in the vanguard of working-class opinion, that accounted for the frustration of student radicals, many of whom acquired a revolutionary perspective on fundamental questions of social organizations and international politics. It was in this context that radical groups became politically isolated and suffered the first pre-condition of sectarianism. It is, of course, easy to be inward-turning when no one outside is listening. Steven Langdon, an important student leader of the time, has captured this and other features of the student radicalization:

Many young, middle-class students were eager and anxious for rapid, immediate and far-reaching change. There was a taste for the apocalyptic in their belief that 'the revolution' could take place now, ignited by the spark of a student uprising. Partly it reflected their time-scale as university students; partly it came from lack of commitments in the existing society.

All left groups of the 1960s, from the Waffle to the Maoists, suffered from political isolation arising from the unevenness of the radicalization in the Canadian population at large. The touchstone of "mainstream" working-class radicalism was the NDP, and only an organization with a firm grasp of history or a solid cadre of working class radicals could overcome the impatience of its members with lacklustre and long-term work in the NDP. By 1971, the LSA had also adapted to the radical student milieu and repudiated its 1960s positions on such key questions as the NDP. The NDP became, in the words of the Revolutionary Workers League document appended to *The Real Subversives* (the RWL is a successor to the LSA and Revolutionary Marxist Group), the "principal obstacle" to socialist revolution. Nevertheless, these revolutionaries were prepared to engage in "united fronts" with the NDP. If this is not

politically sectarian, it at least reveals an inflated sense of self-importance somewhat akin to the proverbial ant climbing up the elephant's leg with intent to rape. This sectarianism, however, is a product of the uneven development of the radicalization in various strata of the population, the ability of many radicals to live within a self-contained and self-consuming sub-culture, and the isolation of revolutionaries from the working-class movement. It is not an inherent product of the group's "intransigence," persecution complex, or proselytizing zeal. It is, in short, the result of social, not psychological, processes.

So much for the application of one "ideal type" — sectarian — in the study of revolutionary socialist organizations. The other ideal type — subversive — is slanderous as well as misleading. O'Toole himself bandies this, and other terms such as violent, quite loosely. This equation of revolutionary with subversive, of violent with militant, testifies to both O'Toole's bias and the success of the organization which has done so much to define certain political beliefs as intrinsically criminal — the RCMP. As the RWL's Richard Fidler complains: "We have been accused, tried and convicted by the government — not for any criminal actions, but solely on account of our political opinions."

The book itself is a compilation of RWL news articles and documents presented to the Macdonald Commission, together with a useful chronology of RCMP activities prepared by the Quebec Human Rights League. Largely an exposé based on recent news developments, the book insists that revelations of RCMP crimes point "to the existence of a massive conspiracy against the democratic rights of Canadians and Quebecers, formulated at the top levels of government." Subversion has become an ideological crime, above and beyond specific acts of illegality, and the scope of the charge is broad enough to encompass all dissent. Despite the fact that the LSA was a legal organization, known for its political opposition to terrorism, it was subject to

extensive surveillance. Although the LSA suffered particular victimization, the book argues that the real targets of the RCMP were the Quebec nationalist and pan-Canadian labour movement: "It is precisely because the ruling class confronts a mass movement, and not a tiny group of would-be conspirators isolated from the masses, that it has had to create an immense arsenal of repressive agencies and techniques."

As a review of RCMP "dirty tricks," and an attempt to define the RCMP as "the real subversives" of Canadian democracy, the book is persuasive enough. As an analysis, however, the book has several shortcomings of particular importance to labour historians. The RCMP, the book argues in a formulation that suggests more familiarity with C.I.A. activities than Canada's real world status, is "rooted . . . in the evolution of the Canadian state into a modern imperialist power." Indeed, apart from RWL make-believe about Canada's place in the world imperialist system, the Watergate "ideal type" of exposé underlies the whole book and accounts for the persistent "muckraking" effort to lay responsibility for RCMP misdeeds on high-ranking government ministers. But Watergate is not an appropriate model for the study of RCMP revelations, if only because RCMP crimes are considerably more profound. Watergate was a mere caper compared to the simultaneous Agence Presse Libre de Québec break-in, which involved co-ordination of hundreds of officials, yet remained a secret for four years. No accused RCMP officers have been brought to trial and no criminals have been held up to public scorn, even as scapegoats.

The RCMP, unlike Nixon's plumbers, is part of the state apparatus, the Canadian state on horseback, and exists independently of responsible or non-responsible ministers. It has powers and autonomy unequalled by any police force in the "free" world. It regards all Canadians, including top cabinet ministers, as poten-

tial subversives. These are unique features of the RCMP which need to be analyzed, yet to the extent that this book finds anything unique in Canada which explains the extraordinary level of police repression and reprisal, the author borrows a phrase from the Russian Revolution and defines Canada as a "prison house of nations," a term referring to the national oppression of Quebecois and Native Canadians.

This, perhaps, leans too heavily on a straight reversal of the myth about the RCMP's role in settling the west by keeping order in the midst of whisky traders and wild Indians. It is true that in the period prior to mass settlement of the west the RNMWP performed as the "moral equivalent of the CPR," establishing the power of central Canada in the west. By the time of World War I, however, the RCMP acquired its modern specialized character as a state instrument against labour radicalism. Its undemocratic patterns of organization and responsibility, which evolved in the context of western settlers' lack of provincial powers, were retained as an antidote to the semi-colonial, staple-based growth of working-class communities in the North and West. The RCMP was the one force able to "maintain the right" in such isolated communities where strikers and their supporters often formed the majority. In this sense, the RCMP was a product of Canada's twentieth century, not its nineteenth century, frontiers. It is best understood in the context of working-class militancy on the staple frontiers, rather than in the context of nationally oppressed peoples or worldwide imperialist holdings.

"Sectarian" and "subversive" — these are two charges that describe most Canadian's opinions of Marxists. The concept of subversion, that is a minority conspiracy to overthrow established authority, is itself contrary to Marxism. Even when the RCMP points to the undeniable Russian Connection of the Communist party, we must remind ourselves that individual Communists, like Roman Catholics, have the right voluntarily to subject their lives to

a foreign pope, and that the RCMP spends none of its time thwarting the subservience of Canadian branch plant managers to the dictates of expansionist U.S. multinationals. The charge of sectarianism, however, is often true. Moreover both charges are widely believed to be true which sets apart the 1948-79 period as a distinctive one in the relations between Marxists and the labour movement in Canada. This provides labour historians with an important problem to explain.

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James C. Foster, *The Union Politic, The CIO Political Action Committee* (Columbia: University of Missouri Press 1975) and Ruth L. Horowitz, *Political Ideologies of Organized Labor* (New Brunswick, N.J.: Transaction Books 1978).

A RECENT REMARK by Lane Kirkland, AFL-CIO secretary-treasurer and heir-apparent to George Meany, underscores the timeliness of a re-evaluation of labour's historic relation to politics, government, and legislation. Kirkland wondered out loud whether the North American labour movement might not be better off returning to the "jungle" of confrontation with employers without the "protection" of national labour legislation. Spurred by a net stagnation in organizing amidst the flight of manufacturing, decline in workers' real income, and new political anti-union initiatives, the suggestion, however serious, reveals a forced recognition of the costs accompanying the "rise" of the labour movement in the twentieth century. The impact of labour's post-Depression marriage to the Democratic Party and the reliance on government legislation and administration for securing, extending, and protecting worker advances are specifically called into question. Both books under review offer help in reappraising the value and assumptions of labour's shifting

political strategy, but the reader will have to travel far beyond the authors' own announced perspectives and purposes to ferret out this larger message.

Ruth L. Horowitz's *Political Ideologies of Organized Labor* essentially traces the rhetoric of union leaders from the erection of the doctrine of voluntarism to its ultimate supersession by New Deal-inspired commitment to labour-government cooperation. Her exposition of voluntarism, often simply equated with conservative business unionism in the Gompers era, turns up some intriguing connections to nineteenth-century radical thought. She notes, for example, that

Gompers' elementary starting point was his hostility to classical economists who, he felt, had popularized the notion that society's economic order was governed by impersonal "natural laws" and that rational social behavior consisted of conforming to them. . . . "Wages [according to Gompers' view of the market-as-jungle] are . . . paid on the trial and balance principle, fixing them as low as the workman will stand and not according to any rational, well-formulated theory." . . . Thus voluntarism was based on an initial assumption that human will must make economic activity submit to human needs, a task labor must take the initiative in carrying out.

In Gompers' ideal view, the benefits to the workers of a capitalist economy could only be secured through active recognition of permanently conflicting class interests to be thrashed out on the economic plane.

Gompers and AFL policy in general carried over this truncated syndicalism in their approach to politics and the state, especially at the federal level. Except for absolute protection of labour's right to organize and strike, (Sherman Act, Clayton Act) and the search for limits to the competitiveness of the market-place (child labour, prison labour, immigrant restriction laws), the AFL cast a cold eye on government social initiatives. Horowitz identifies one source of the opposition as a moral-political fear on the AFL's part of the loss of popular will to fight: "Whether as a result of laziness or incompetence there is a

steadily growing disposition to shift responsibility for personal progress and welfare to outside agencies. . . . Many of the things for which many are now deludedly demanding legislative regulation should and must be worked out by those concerned." Advocates of "easy" legislative remedies also were forgetting or deluding themselves, according to this view, regarding the neutral, even beneficent nature of the state:

Some say that the state is an agency through which the people obtain results — that it exists for their service. But the state is not some impersonal thing. It has no existence outside the people who compose it. . . . What is legislation but class legislation or the formulation by one group . . . what they deem in their interests?

Suspicion of the impact of legislative reform and mistrust of the major parties (and scorn for the impotence of third parties) carried over into the New Deal Era. The federation, of course, applauded the Norris-LaGuardia Anti-Injunction Act. And it jubilantly greeted the NRA:

The hosts of mankind now are stirring,
in freedom from fear's blighting sway;
Equator to pole sounds the whirring,
Of azure-hued wings, NRA!

For the latter, in the AFL's view, stopped short of government administration-adjudication of industrial relations, while facilitating and authorizing collective bargaining and labour-management cooperation. But on unemployment insurance (1932), Social Security (1935), the NLRB machinery of the Wagner Act (1935), and the wage and hour regulations of the Fair Labor Standards Act (1935), the AFL waffled in ambivalence, opportunism (i.e. supported only when inevitable or to steal thunder of the CIO), or internal division.

These politically "backward" positions have generally been assimilated into a picture of the AFL's narrow-minded defensiveness and strategic somnolence.

And, no doubt, they did make for an ostrich-like response to the Depression. But the relatively self-sustaining strength of the conservative craft unions — their very distance from the dynamic of 1930s' politics and labour organizing — also placed them in a position to offer some prophetic insights about the nature of the bandwagon running along without them. "I want to tell you this, men," said John Frey of the AFL's Metal Trades Department, "that if you feed lions cooked meat they are not going to roar. . . . It is only through the fighting strength of economic organization that you are going to get higher wages and shorter hours."

The logic of voluntarism, from the beginning, depended on a thoroughly organized working class. This neo-syndicalist faith, for a host of political and economic reasons, of course, was not only compromised but thoroughly contradicted by the AFL in practice. What appeared, in principle, a call to militancy became, in practice, a justification for do-nothingism. Still, it provided a more rational critique of what would become dominant assumptions of the labour movement than it has perhaps been given credit for. The subject, in short, raises many questions about the sources of and conflicts within labour political ideology and political practice.

Unfortunately, Horowitz lets drop the opportunity for a revisionist look at her material. While early AFL practice clearly had many ties to its nineteenth-century predecessors — compare, for example, voluntarism to nineteenth-century cooperation, associationalism, and syndicalism — the author clings to a rigid Grobian division between pre-AFL utopian political unionism and the "modern" movement. Similarly, she is quite willing to accept an uncomplicated "progressive" view of the ultimate eclipse of outmoded voluntarism through ultimate awareness "that a new era of enlarging industry, enlarging government, and an enlarging population of unprotected workingmen was developing, and that the liberties of workers could now

be safeguarded only by active extension and politicization of the union movement."

The nature of that politicization is the subject of Foster's history of PAC, the CIO's Political Action Committee, 1944-1954. In a plodding, dissertation-style narrative, Foster dispels the image, projected both by contemporary paranoia and romanticization, of the CIO as a powerful and persuasive political juggernaut. While activists and well-wishers like Len DeCaux and Harold Laski would hail the founding of PAC as a kind of proto-labour party ("as important an event in American history as the creation of the Populist alliance of the 1890s . . . to America what the Co-operative Commonwealth Federation was to Canada"), such tidings proved both premature and inexact. "In the twelve years of its existence; the CIO-PAC lost many many more elections than it won." And far from setting an ideological and programmatic standard for the adopted Democratic Party, PAC, according to its long-time director Jack Kroll, ended up as one of several Party clients, bargaining with the Party "much as it would with an employer." Nor could the PAC hold its ranks to a distinctly left-labour Democratic bloc. Foster concludes regarding the apparent "PAC victory" of 1944, for example, that

Although the CIO hierarchy had handed down a program calling for everything from reconversion to minority rights, its efforts had no easily measured effect on Democratic fortunes. Roosevelt carried CIO districts with stunning ease, but CIO-sponsored liberals all-too-often found his costails quite difficult to ride. The hoped-for liberal labor constituency seemed as elusive as ever.

If AFL-era political ideology represented a pale reflection of an earlier syndicalist radicalism, then the CIO's political weakness may be traced to the shrunken terrain of political citizenship by the mid-twentieth century. In no sense did labour seek control of the government; indeed few workers or union members were ever put

up for office. To suggest, however, the limits of CIO-era political accomplishments — that is its distance from a distinctly labour-centered politics and its susceptibility to outside manipulation — is not necessarily to criticize the CIO-PAC political strategy. For far from empowering the working millions it organized, the CIO had before it the basic task of political enfranchisement. Any such reassessment must begin with an extended look at contemporary labour politics in specific communities; here a vacuum of scholarship remains unbreached. Indeed, Foster, like Horowitz, tries to take the easy way out of the difficult question his investigation opens up. For Foster, the idealism and left-liberal principles of the PAC were largely ineffectual posturings. They would have fared better he suggests, had they stuck to a less ideological course of hard bargaining within the "realities" of power politics: "The 1930s-thinking of a number of CIO leaders led them to champion the wrong causes in front of the wrong audiences at the wrong times. It was poor judgment, not bravery, that placed the PAC on the wrong side of the Communist issue in the 1946 elections."

It is interesting, in this light, that the only election in which the PAC, according to Foster, played a successful and sophisticated role, the 1954 Congressional races, also represented the first serious outreach to women voters. Foster chalks up the effectiveness of this appeal to "Democratic households" (rather than simply to CIO members, since women were largely back in the homes then) to a concentration on "sound economic motives." But another lesson surely lay in the attempt to extend the meaning of unionism to a larger audience. If so, it may be that a distinct vision of community and citizenship, as well as supplements to the bargaining table, lie at the heart of a winnable labour politics, as well as one worth winning.

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Tom Clarke and Laurie Clements, eds.
Trade Unions Under Capitalism (Atlantic
Highland, N.J.: Humanities Press 1978).

THIS BOOK provides an interesting and useful collection of articles covering a wide range of subjects. It begins with a section called "Classical Analysis," containing selections from Marx, Engels, Lenin, and Trotsky. This is followed by a very interesting section, "The Revolutionary Tradition," which in fact deals with the intense shop-floor organizations and struggles in Britain during World War I. "Sociological Analysis" presents critiques of pluralism and trade-union bureaucracy, largely within the framework of academic sociology. The only section in which rank-and-file, working-class struggles are discussed and documented is called "Workers, Unions and Job Control." There are two excellent selections from Carter L. Goodrich and Huw Beynon but a very poor one from Richard Herding. It is unfortunate that Herding's piece on "Job Control and Union Structure" is the only one that deals substantively with the North American experience.

Herding begins with an outrageous economic analysis: "Since in a private enterprise economy labour as a class inevitably has to pay for whatever gains a specific labour organization may enjoy..." (260) After having thus single-handedly restored the iron law of wages, it becomes a simple matter to interpret rank-and-file struggles in the worst light, since all victories are won at the expense of other workers. The concluding sentence completely contradicts everything he tried to demonstrate in the body of the article: "The time when it was fashionable to write off American organized labour wholesale, as a conservative, stabilizing force in society, is past..." It is the kind of lip-service to revolutionary rhetoric that has given so much of Marxist analysis a bad name.

The book concludes with a section on "Unions and Contemporary Capitalism"

designed to present a contemporary theoretical analysis. It is most useful in presenting a range of left thinking on trade unionism. On one extreme is a selection from Richard Hyman who seems to be the only writer around today who is aware that *What Is To Be Done* was not Lenin's last word on unions and working-class consciousness. As a result, what he writes tends to relate more meaningfully to changes in historical reality. At the other extreme is Perry Anderson, who has managed to enshrine some phrases from *What Is To Be Done* as eternal verities and has taken them to ludicrous extremes. Lenin, who was always willing to learn from experience, modified some of his statements as a result of the experience of the 1905 revolution. Anderson, on the other hand, refuses to let experience interfere with his theory. His selection was originally published in 1967. One year before the French Revolt he managed to declare that "The occupation of factories is in practice no more than a dramatic form of picketing." (339) He succeeds in doing to Marx (and Lenin) what Marx did to Hegel — stand him on his head, and transform a materialist point of view into an idealist, Utopian point of view. "A revolutionary party, . . ." he says, "embraces more than the working class; it includes intellectual and middle class elements which are bound by no inevitable ties to the socialist movement at all. Their allegiance is created, *against the grain of the social structure*, by the work of the revolutionary party itself. Thus the political party alone can incarnate a true negation of existing society and a project to overthrow it." (335) That is to say, the working class, contrary to Marx, is not inherently revolutionary because its struggles arise necessarily from the social structure. Only struggles arising out of a voluntary commitment to ideas are revolutionary.

Although the selections in the book reflect much of the range of thought about trade unions on the left, they also tend to reflect some of the weaknesses and limita-

tions of that thought. For example, selections from Marx, Lenin, etc. are usually viewed as political positions, rigid and eternal, rather than changing responses to a changing reality. As a result, what is crucial to the "classics," namely the methodology, is lost. For example, here is a selection from a speech by Marx in 1869 (during the life of the First International): ". . . If they wish to accomplish their task, trade unions ought never to be attached to a political association or place themselves under its tutelage; to do so would be to deal themselves a mortal blow. Trade unions are the schools of socialism. It is in trade unions that workers educate themselves and become socialists, because under their very eyes and every day the struggle with capital is taking place. Any political party, whatever its nature and without exception, can only hold the enthusiasm of the masses for a short time, momentarily; . . ." This is not presented to contradict some other quotation from Marx or Lenin but rather to indicate the richness of what they wrote and the need to examine the sources of their views. To do otherwise is to distort their views, as the editors do. (227) Talking about *workers*, the editors say that "Marx declared, 'They fail generally from limiting themselves to a guerrilla war against the effects of the existing system, instead of simultaneously trying to change it.'" The original quotation (55), clearly indicates Marx was talking about *unions*. So that a statement expressing the limitation of union struggles is distorted to mean that workers' shop floor struggles are limited.

Moreover for many of the contributors (including the editors) there is a confusion between "unions" and "rank and file workers." Where there is the need to show bureaucratization of unions, the distinction is clear enough. Elsewhere it is blurred, resulting in considerable lack of precision in arguments and in distortion of reality. To cite one example, among many: Michael Mann notes that "typically workers will gain some shopfloor control infor-

mally, and indeed surreptitiously, and then formally sign it away in union-management negotiations." (296-7) That is to say, *workers* will violate the contract and ignore the union in winning rights in direct struggles. The *union* then signs these rights away. Conclusion: see how limited workers' struggles are! The problem generally is that the writers see the limitations of unions as essentially the limitations of direct working-class struggles. One wonders where these authors think that soviets and workers' councils are formed — in some party committee? Workers have often transcended the limitations of unionism in struggles on the factory floor.

In addition there is a general inability to deal with Roberto Michels' "iron law of oligarchy" as it relates to unions. It is a continuing theme in many of the selections. The writers agree that unions are bureaucratic but that they *need not* be. That is not a serious analysis. If Michels' conclusions have been confirmed over a period of over fifty years, wishful thinking is not an answer. The real problem rests with the reasons Michels presents, not with his conclusions. Beynon points out (247) that "controlling the membership is part of the steward's job." It is in that objective reality of union functioning, the enforcing of contracts and agreements, and the disciplining of workers, that the sources for "oligarchy" can be found.

Finally, much of radical analysis suffers from the limitations of bourgeois social science. The tendency, most pronounced among structuralists but evident elsewhere, to fragment social reality is inherently conservative because it conceals the revolutionary potential. The absolute separation of economics from politics, of factory from society, etc. tends to confirm the prejudices of the middle class observer who then finds it easy to get evidence of the backwardness of the workers. Laurie Clements' contribution, "Reference Groups and Trade Union Consciousness," has the distinction of suffer-

ing from most of these weaknesses, augmented by an insistence on using an obscurantist jargon.

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L.M. Cullen and T.C. Smout, *Comparative Aspects of Scottish and Irish Economic and Social History, 1600-1900*. (Edinburgh: John Donald Publishers Ltd. 1978). Distributed in Canada by P.D. Meany Co. Ltd., Port Credit, Ontario.

THE INTELLECTUAL economy of the British Isles often has followed the lead of the political economy. During the 1970s the economic renaissance of Scotland and the quantum jumps in the Irish economy have been paralleled by a burgeoning "production" in academic fields related to the two national cultures, not only in Scotland and Ireland, but in North America as well. That relatively little work is being done by English scholars tells a great deal about their parochialism, but little else. This collection of 17 essays by 15 historians testifies to the vitality of academic scholarship in the nations too long dismissed by English historians as merely the Celtic fringe. Generally, collections of essays are as difficult to describe as are the contents of the combination-plate at a Chinese restaurant, but this one is different: the editors have focused their contributors' work tightly upon a single theme. Although one cannot read this volume in the same way one reads a monograph, its entire contents do bear directly upon the central theme as announced in the title.

Among the contributions four essays stand out. "Landlord and Tenant Relations in Ireland Between the Famine and the Land War, 1850-1878," by W.E. Vaughan of Trinity College, Dublin is the most lucid summary yet available of the major revisionist school of work concerning the land question. This school is led by Vaughan himself, by James Donnelly of the University of Wisconsin, and by S.D.

Clark of the University of Western Ontario. These, and associated writers, question the standard notion that the Irish landlords of the nineteenth century were capricious overlords, that the state of Irish agriculture was backward because of the insecurity of the tenants and the failure of the landlords to improve their properties, and that the country was rife with agrarian violence caused by the landlords' callousness. Such notions are now part of the mythology of nationalist Ireland, and they do more than a little mischief as one strand of the tradition which justifies random violence as a tool against alleged oppression. Actually, there is a large and convincing body of recent scholarship which shows that the tenantry in the era of the land wars were not rack-rented, were not pressed into poverty, and were, in fact, raising their incomes and their profits much faster than the landlords raised the rents. The argument and documentation on these and related points is too rich to summarize here, but Vaughan's concluding points bear noting: that we no longer can take a simple oppressors-and-victims view of Irish landlord-tenant relations, because historians of Ireland have increasingly come to realize that discontent and agitation are not always founded on actual oppression, and because we realize that positive law (in the sense, here, of rigorous land legislation) has little influence on actual economic behaviour.

"The influence of the Landlord in Eighteenth-Century Ulster," by W.H. Crawford is misleadingly titled. It actually is about one of the most intriguing, and seemingly intractable historical puzzles of Irish life: from whence came the "Ulster Custom?" The Custom is crucial in Irish social history, for it was one of the distinguishing characteristics of the mainly-Protestant counties of the north-east. Unlike the tenants in the rest of the country, the Ulstermen had virtual lifetime security of possession as long as they paid their rents and the right to sell the capitalized value of their improvements to

the next tenant if they left the holdings. (The details of the Ulster custom, being based on customary practice rather than statute law, varied area-by-area and over time as well, but the basic principle was the same.) Crawford, Assistant Keeper of the Public Records of Northern Ireland, knows more than does anyone about the Custom in the late seventeenth and eighteenth century, an era for which the records are scarce and for which those that are available are difficult to interpret. In a brilliant paper, so compressed as to be terse, he argues that the rise of the Custom is explained by the strong democratic traditions imported from Scotland by the insuring Presbyterians of the seventeenth century, by the colonial nature of the society which gave the Protestant settlers great leverage with their landlords, and by the prosperity of the domestic linen industry which in the eighteenth century made the cotton-weavers relatively invulnerable to the caprice of individual landlords.

R.B. Weir's "The Patent Still Distillers and the Role of Competition," chronicles the emergence of the near-monopoly on spirit production by a cartel of Scotch whiskey producers, and, incidentally, explains why most Scotch has no more character than the grain spirits sold in Canada under the misnomer "rye." In 1850 a retired Irish exciseman, Aeneas Coffey, patented a still that worked continuously. In the large whiskey firms this replaced the traditional pot still, because it did not require successive redistillation of the spirits and because it worked with inexpensive raw grain, rather than with malted grains as previously required. The resulting whiskey was thinner-flavoured, but was cheaper to make. The large distillers invested heavily and brought a relatively inexpensive drink to the Scottish lowland and the English market, replacing gin as the workingman's spirit. However, the technology was expensive, and beginning in 1865 a series of attempts was made to restrict competition. Essentially, the cartel

wished to fix sales and prices in the United Kingdom, while competing only for export sales. Their success is indicated in the fact that, by 1922, a cartel controlled all but one patent still distillery in Scotland, and all save one in Ireland and two in England.

Of direct interest to historians of Canada is Ian Robertson's "Highlanders, Irishmen, and the Land Question in Nineteenth-Century Prince Edward Island." Unlike most parts of North America, Prince Edward Island did not have a freehold system of land tenure, but instead adopted the leasehold system dominant in the British Isles. Under a complex, and fascinating array of forces — Scottish and Irish immigrants, and descendants of the U.E. Loyalists — the "neo-feudal" system eventually was broken, but not until surprisingly late in the nineteenth century. The agitation, though, was not straightforward, because Orange-Green strife, and Scottish-Irish rivalries within the Catholic church made the land agitators' alliances ever unstable. Robertson emphasizes that on this issue, as in so much of Canadian history, one cannot understand the participants' actions without a knowledge of the cultural and political reflexes they brought with them from the old world.

The editors of this volume, L.M. Cullen and T.C. Smout, provide a useful introductory essay in which they weave together most of the papers in the book and try to explain why, though starting from a roughly equivalent base, Scotland did so much better economically than did Ireland. Their discussion is encapsulated in the formula: "The big factors in the differing development of the two countries, then, seem to us not political but geological (coal and ore), agrarian and, in so far as adaptability is concerned, cultural."

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James Hunter, *The Making of the Crofting Community* (Edinburgh: John Donald 1976).

THERE WERE few movements in the history of capitalist production, noted Marx in *Capital*, more significant than the expropriation of the agricultural producers, a process "written in the annals of mankind in letters of blood and fire," and one that stood as the "starting point" of capitalist accumulation, conquering "the field for capitalistic agriculture, [making] the soil part and parcel of capital, and creat[ing] for the town industries the necessary supply of a 'free' and outlawed proletariat." This beginning of the history of capital, or primitive accumulation, as Marx and others have noted, assumed its classic form in the English enclosures. But Marx himself argued that what the "clearing of estates" properly signified was revealed most starkly "in the promised land of modern romance, the Highlands of Scotland." There, the eighteenth century witnessed the clan chiefs' efforts to turn their titular rights to the land into a right of private property. Then followed expropriation and resettlement, as the Gaelic peoples were forced off of their holdings, banished to wastelands by the sea while their previous acreage was converted, first, to sheep farms, and then, later in the nineteenth century, to deer preserves. "One after one," recounted Marx, "the liberties of the people have been cloven down. . . . And the oppressions are daily on the increase." It was in the midst of just such developments that 600 Scots tenants emigrated to Canada from Locheil in 1803-04, hoping "to be better off to be out of reach of such unnatural tyranny."

It is the long-term consequence of these clearances and deportations that James Hunter explores in this richly detailed, finely textured, and sensitively conceived examination of the painful and protracted experience culminating in the consolidation of the crofting community. The book is an exemplary study, its sym-

pathies for the small, landholding crofters and landless cottars reverberating throughout every page, each paragraph consciously constructed to counter the many studies of economic historians who have probed land records and the papers of estate managers or factors to illuminate the inevitability, and impersonality, of the "modernization" of the Highland economy. Much more than a populist restatement of the crofters' cause, however, *The Making of the Crofting Community* goes beyond the nineteenth-century radical critique of Highland landlordism to probe the historical evolution of the crofting community, and its intricate relationship to the landed lords. Placing the crofters at the centre of this history, making them subjects rather than objects, Hunter nevertheless is seldom far from the MacLeods, MacNeills, MacDonalDs, and MacDonnells of North and South Uist, Mull, Skye, Harris, or Bernera. Such men brought crofting into existence, orchestrating clearings, evictions, deportations, and legislative protection for their interests. And as Hunter demonstrates, it was against such individuals that crofters eventually came to feel a deeply embedded antagonism. The crofting community was forged in the crucible of conflict.

The birth pangs of the crofting community lay in the eighteenth century. Structured around the military needs of the clan, the genealogically-defined society of the Highlands of Scotland rested on kinship ties cemented by a series of land tenures and subtenures reflecting the hierarchical, caste-like divisions of the society. Below the chiefs were the tacksmen, lieutenants of warfare, for whom courage and prowess in battle were the ultimate virtues. These men held most of the land, and paid their lords nominal rents for their farms; their principal role, however, was to provide the chief with skilled soldiers, and their interest in farming was slight. Inordinately conscious of their status, the tacksmen subtlet the greater portion of their land to the cottars and mailers, the lower orders of the

clan, and seldom troubled themselves with the mundane details of agriculture. Such a system, while unrivalled as an instrument of war, was grossly deficient as an economic undertaking, the mutual obligations of kinship undermining any recourse to the impersonalities of commercialized agriculture or the hard dealings of maximizing rents. As the eighteenth century progressed, and the chiefs found themselves rubbing shoulders with commercial men from Glasgow, London, and Paris, their world, and their needs, expanded. Commercial interchange between the Highlands and the rest of the British Isles accelerated, the trade in black cattle being a major component. The sweep of market relations caught even the Highland lords and, ever-conscious of their aristocratic place, the chiefs felt obliged to keep up with their social rivals, the nobles, merchants, and courtiers with whom they mingled. Highland chiefs thus seemed caught between two roles, two expectations, indeed two worlds. Traveling in the urbane circles of the eighteenth-century commercial and industrial revolutions, the chiefs needed money to prove their worth, which, day by day, came to be increasingly defined by the cash nexus. In the Highlands, however, where patriarchy persisted, the tribal leader could do little to translate effectively his own rights and powers (which always entailed reciprocal duties and responsibilities) into hard cash or profitable property.

The internal strains within Highland society were thus considerable, and were exacerbated by the British government's determined efforts to suppress Highland Jacobitism, expression of the Highlanders' devotion to the cause of the exiled Stuart monarchy, which threatened London's imperial might. In 1745, for instance, a small Highland army conquered Scotland and marched to within 127 miles of London. The British government moved forcefully to destroy the traditional society of the Highlands, and in the post-1746 years the clansmen, Jacobite and non-Jacobite,

were disarmed, physically and culturally. Those lords that remained, their traditional rights and powers vanquished by a larger authority, were finally freed to pursue the thorough-going commercialization of their estates.

By the end of the eighteenth century, then, the Highland chief had become a landlord, and while the people might cling to certain notions of the customary ties of the clan, the lord pursued profit with what was to become a relentless passion. The first to feel the sting of this dissolution of the clan was the tacksman, whom the lord struggled to transform into a businesslike farmer. Such efforts generally proved unsuccessful, however, and the tacksmen, usually men of some means, emigrated to America in large numbers. For their subtenants this avenue of escape was blocked, and they remained behind in large numbers to face landlords' improvements, evictions, and calculations of profit and loss, nurturing, all the while, a profound sense of betrayal. They were to be the human substance of the crofting community.

These subtenants would probably have been driven off the estates mercilessly to make way for sheep, a more lucrative species, were it not for the fact that in the north-west Highlands and Hebrides the kelp industry proved even more profitable than sheep farming, securing landlords immense profits in the opening decades of the nineteenth century. Kelp, an alkaline seaweed extract used in the manufacture of soap and glass, had been harvested in Scotland since the 1720s, although the chiefs remained unaware of its possible value to themselves, a few enterprising tacksmen and Irishmen pocketing most of the industry's proceeds. But as the price of kelp rose from 2 pounds a ton to over 10 pounds a ton the landlords took over the industry, going so far as to establish their legal rights over the weeds of the sea. And to harvest the kelp crop they needed a labour force. To secure workers many proprietors reorganized their estates, displacing their tenants to marginal farms where they could

subsist for most of the year on their own, unpaid, labours. Rents on these newly-established crofts were raised to sufficiently high levels to insure that the small tenants would be forced into kelping in order to earn enough to pay their landlord, who was also their employer in the kelping industry. Kelp and crofts thus marched hand-in-hand in the Outer Hebrides in the years 1800-20, and on the mainland or in those parts of Mull and Skye where kelping was not well established, the fishing industry played the same role. Massive migrations followed in the wake of these developments and to retain their captive labour force the landlords secured the passage of an 1803 Act regulating the number of immigrants that could be transported on any vessel, and ensuring the provision of adequate supplies of food, water, and medicine. Depicted as a humanitarian measure, the act was in fact an effort to limit the number of crofters fleeing the abominable conditions of the Highlands, and by raising the costs of emigration from 3 pounds to 10 pounds it succeeded beyond its designers' expectations.

Crofting, while by no means a bountiful existence, was no doubt tolerable in these early years, but the whole structure rested on the precarious foundation of kelping. As glass and soap manufacturers perfected the manufacture of alkali from salt, and as the wartime tariffs maintaining artificially high duties on foreign alkali collapsed in the post-1820 years of peace, the bottom fell out of the kelp industry. The agricultural depression which also followed the ending of the Napoleonic wars merely made things worse, depressing the price of cattle, the mainstay of the crofters' existence. For the landlords the solution seemed inevitable, and evictions of crofters proceeded almost from the beginning of the kelp industry's collapse. The crofters, once in great demand, had become a "redundant population": Highland proprietors who had, with much humanitarian concern, petitioned the government to secure the Passenger Vessels Act of 1803,

greeted the 1827 abolition of emigration restrictions with unrestrained glee. By the 1840s crofting life had deteriorated to an abysmal state. Rents remained excessive, but the means of meeting them had disappeared as kelping succumbed to new conditions and processes, and the fisheries failed miserably to take up the slack. The gulf between crofting income and rent made saving impossible, and stifled all initiative and enterprise. Threatened by the omnipresent retaliation of eviction, the crofter was reduced to total dependence and subservience. Economically, crofting life turned on one crop, the potato. And when famine struck, in the years 1845-50, even that lowly root became a victim, infected by a fungus. This development only served to intensify the Highland proprietors' hostility to the crofting system, and many crofters were driven from their lands by those determined to turn a profit on their estates. "Redundancy of population" became a "prominent topic of lamentation" in landowning circles. With the transatlantic depression of the 1850s even the safety valve of escape through immigration was blocked, for things appeared little better in the Americas.

By 1857, however, the crofting community had faced the worst. To be sure, whole townships had been obliterated, and crofters' lands taken from them with much violence: poverty, insecurity, hunger, and deprivation seemed the inevitable lot of the crofter. Throughout the Highlands and Hebrides crofting was at best a marginal undertaking, of little economic significance, and the small tenant was regarded as something of a nuisance by his landlord. But in spite of everything, the crofter persisted, and retained his attachment to, and in a reduced way, his hold on, the land. For the first time in the memory of at least a generation prices were rising, and the force of the potato blight was spent. In the post-1857 years a crofting community, conscious of its own needs and aspirations, would emerge, and Hunter argues that it is in these latter decades of the nineteenth

century that crofters begin to do more than react to the landlords' initiatives.

Much is often made of the crofters' lack of resistance to the wave of evictions that dispossessed the crofting community in the years 1800-60 and the passivity with which the tenants replied to spiralling rents which mired them in poverty. As Hunter is well aware, and as Marx also stressed, this acquiescence should not be exaggerated: when the Duchess of Sutherland proceeded to clear her estates of 3000 families, destroying and burning villages to turn their fields into sheep pasturage, one old woman was consumed in the flames of a hut she refused to abandon. And there were countless other instances of resistance, many of an aggressive, violent character. But, on the whole, the crofting community mounted no large offensives, and it is significant that the first stirrings of collective resistance were visible, not in the economic sphere, but in the cultural, specifically religious, realm. An evangelical movement swept through the crofting community in the opening decades of the nineteenth century culminating in the disruption of the Church of Scotland in 1843. Led by a group of radical lay-preachers, and sustained by a series of revivals, this religious movement embraced a fervent anti-landlordism, and in its opposition to the Established Church it posed the first serious division within Highland society, illuminating the conflicting interests of the proprietors and the crofters. With the establishment of the Free Church, the crofters broke decisively from the ranks of the clan for the first time, acknowledging their separation from the landed lords. On the first Sunday following the split of 1843 the crofters announced their attachment to the Free Church, and their refusal to accept the Established Church's authority with appropriate symbolism. In Dumess the church bell was muffled with an old sock, while in Farr a dead dog was hung over the pulpit.

In the years following the disruption of 1843 the force of religious experience

spent itself, and it would play only a marginal role in the crofting community in the later nineteenth century. The years 1858-80 were ones of recovery and consolidation for the crofters, and the clearances that had so disrupted life on the crofts in earlier years eased. John Murdoch emerged in these years as the voice of the crofting community, urging the destruction of the vicious land system that kept the crofters in a perpetual state of dependency. But it was in the 1880s that the crofting community was finally made, as crofters stood together to resist a new wave of evictions, seized lands in defiance of their landlords, the police, and the courts, and attracted the sympathy of many southern supporters, who founded the Highland Land Law Reform Association, loosely modelled on the Irish Land League. All of this played a not inconsiderable role in stirring the authorities to action, and a royal commission under the chairmanship of Lord Napier was established in 1883 to "inquire into the condition of the Crofters and Cottars in the Highlands and Islands of Scotland." The result was the Crofters' Act of 1886, guaranteeing security of tenure to crofters and giving them the right to claim compensation from their landlords for improvements made to their holdings by themselves or by their family predecessors, should they relinquish their claim to the land. Rents were to be reviewed by a land court, the Crofters' Commission, which retained power to fix fair rents and administer crofting legislation. But the Act did not address the crofting community's major concern, the demand that land be made available to crofters subsisting on meagre holdings, and to cottars, who held no land. Rather than tranquility, the Act was followed by further hostilities, the landlords regarding the 1886 legislation as a revolutionary incursion on their prerogatives, the crofters violent in their denunciation of the shortcomings of the 1886 act. In the years to come the crofting community waged a relentless war for the land which its forefathers had occupied. Lands were

taken by force, dykes destroyed, and buildings burned. On the Isle of Skye, Glendale crofters resolved to take possession of some lands their fathers and grandfathers had been evicted from "in the days when sheep were more thought of than men." In other parts of the Highlands, crofters and cottars demanded restoration of their former rights on farms "in the hands of strangers while . . . the rightful owners were huddled together on rocks and moss not fit for cultivation." Meetings were regularly held in every township and the "wildest resolutions" were passed, urging the possession of "land presently occupied by sheep farms or deer forests." The military often had to intervene to placate landlords' fears and secure some measure of an uneasy peace.

Land raids continued well into the twentieth century, necessitating further government action, and it was clear to all that the crofters' and cottars' claims to the land, rooted so strongly in history and tradition, would not be abandoned. In 1911 the Small Landholders (Scotland) Act attempted to redress some of the crofting community's grievances by providing for the resettlement of crofters and cottars on consolidated holdings. But compensation claims were processed in a slow and complex manner, and the Board established to effect the provisions of the 1911 act was notably generous to landowning and sheep farming objectors to its plans. By 1913 there was "ample evidence of a coming revolt." More land seizures followed, and against this background of virtual guerrilla warfare in the Highlands, the coalition government of 1919 passed the Land Settlement (Scotland) Bill. The Board of Agriculture was granted a large sum to facilitate settlement, was empowered to purchase land to settle cottars and crofters, and was authorised to make loans to the settlers. While going a long way towards settling the discontent within the crofting community, the 1919 Act, like most legislation, was cumbersome and unwieldy, and results were slow in coming. The croft-

ing community reacted with its by now predictable response of extra-legal action, but by the mid-to-late 1920s this no longer seemed necessary. Problems remained but by the end of the decade the struggle for land had run its course. The crofting community had won a notable victory.

This, in broad outline, is the narrative Hunter unfolds before us in the pages of *The Making of the Crofting Community*. Although things have definitely improved for the crofters, Hunter concludes that, to this day, their lot is not a satisfactory one, for until the people own and control their land, he argues, they cannot be masters of their destiny. It is a sensible and sensitive depiction of almost two centuries of an important slice of Scots history, and one that provides us with a detailed look at much of the background to the emigrations to Canada in the pre-Confederation period. We have heard much about the Scottish tradition of late, and one might suspect, from all the talk about the professions and the merchants that that is all that is worth talking about. But Hunter reminds us that the depopulation of the Highlands and the Hebrides, the violent clearances that drove thousands to British America, involved people whose experience was not coloured by their intimate knowledge of Glasgow's merchant houses or the intricacies of book-keeping or banking. Many of the Scots who emigrated in these years suffered greatly, and Catherine MacPhee of lochdar, South Uist, preserved something of their trauma in her recollections: "Many a thing have I seen in my own day and generation," she wailed. "Many a thing, O Mary Mother of the black sorrow! I have seen the townships swept, and the big holdings being made of them, the people being driven out of the countryside to the streets of Glasgow and to the wilds of Canada, such of them as did not die of hunger and plague and smallpox while going across the ocean." When Gordon of Cluny, a proprietor "notorious . . . for his indifference to the feelings and interests of those connected with him," transported one contin-

gent of crofters from his estate to Canada, the medical officer at Grosse Isle noted that he had never seen "a body of emigrants so destitute of clothing and bedding; many children of nine and ten years old had not a rag to cover them." Even the Irish had presumably fared better than the Scots crofters. This was not the human material out of which merchant princes and hard-nosed financiers were fashioned. But it was one chapter in that internationally significant process of primitive accumulation, that lever for the capitalist class in the course of its formation, one of many "moments when great masses of men [were] suddenly and forcibly torn from their means of subsistence, and hurled as free and 'unattached' proletarians on the labour market."

Hunter has done much to enlighten us about one aspect of this long and protracted history of primitive accumulation, the evictions and clearings of the Highlands, and the crofters' struggle to resettle themselves on their forefathers' lands, a struggle culminating in the land wars of the 1880s and the ultimate making of the crofting community. There may well be flaws in his discussion. Perhaps, for instance, he has underestimated the tenacity of certain forms of paternalism, and the bond between lord and crofter/cottar in the nineteenth century may well have been more resilient than he cares to admit. We might well profit by a closer look at the crofters' spokesmen, agitators like John Murdoch, or at the political movement that culminated in the election of crofters' members of parliament. And in the sweep over the Highlands there are times when a finely-honed discussion of a specific locale — its demographic structure and economic, social, cultural, and political life — would open the door to a stimulating analysis. Such undertakings may be possible, or they may not: evidence may not be available. Certainly the argument about the role of religion, especially its declining importance, could be more fully developed, and kinship could be discussed in greater detail, more being made of its sig-

nificance in the crofters' tenacious attachment to the land. The old woman who burned in her hut rather than be moved to the seashore by the Duchess of Sutherland had her reasons, and they were not likely the many years of poverty she had undoubtedly endured in her cottage; she could well have buried a husband on that land, not to mention a mother and a father, brothers and sisters, and countless cousins. To raise these, and other minor points of criticism, is in no way to denigrate this study, which goes so far towards

penetrating the history of a group too often perceived as an anachronistic problem, rather than as a community embracing a distinctly human set of values and needs. Pointing to the history of those values and needs, and chronicling the barriers and opposition they have struggled to overcome, is Hunter's considerable achievement, and this is a very fine book indeed.

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Ian MacDougall, ed., *Essays in Scottish Labour History: A Tribute to W.H. Marwick* (Edinburgh: John Donald 1979).

THIS BOOK is a *festschrift* for W.H. Marwick, a pioneer in the field of Scottish labour history and, until his retirement in 1964, a long-serving member of the staff of Edinburgh University. In a career spanning six decades Marwick published 16 books or pamphlets, and more than four dozen articles. His interests ranged from the history of trade unionism to the economic, religious, and educational history of Scotland. Ian MacDougall has provided a brief biographical note which is of interest to anyone who does not have the privilege of personal acquaintance with W.H. Marwick. A Christian socialist background; an association with the Society of Friends, the Fabian Society and the Labour party; his imprisonment in World War I as a conscientious objector; and his work in both university and adult education, all testify to Marwick's lifetime of political activism. We must appreciate this in the context of a productive scholarly career as evidenced by the complete bibliography of his works published in this volume.

Four of the nine contributors to this collection are senior officers of the Scottish Labour History Society. Two others have worked with the presses of the Communist party and the United Socialist Movement. The editor confesses that the contributors have differing political beliefs but that they are united by a desire "to pay our tribute to William Marwick, a historian of labour in Scotland, a kind and gentle man."

The introductory essay by W. Hamish Fraser demonstrates that trades councils played a greater role in Scotland than they did in England in the nineteenth century. Scottish trades councils were more important because trade unions in that country packed a strong centralized structure. Trades councils, uniting a number of different trades in a region, filled the gap by supporting strike activities of their mem-

bers, organizing the unskilled, working to improve the legal position of organized labour and agitating for the eight-hour day. By the end of the century they were engaged in the struggle to establish independent labour representation in Parliament.

Gordon Wilson analyses "The Strike Policy of the Miners of the West of Scotland, 1842-74" in a well-researched study. He finds that the overwhelming majority of strikes were caused by employer refusal to give in to wage demands or miner refusal to submit to wage cuts. A minority of strikes were caused by such things as disputes over the weighing system, restrictions on the miners' rights to organize unions and hostility to the employment of Roman Catholics. However, in my view, it would not be wise to place too much emphasis on these distinctions. It might be argued that the minority causes of strikes were intimately related to the central question of wages.

Why were some regions more prone to strike than others? Not surprisingly Wilson finds that the strikes were more frequent in the areas of stronger unionization. He then goes on to make a very interesting case that the frequency of these strikes is evidence for class-consciousness among the miners. It is also interesting to note that the growth of an effective strike technique in the western Scottish coalfields co-incided with a decline in the use of violence in labour disputes. Strikes were a "regular feature" of the Scottish mining industry of the period with the employers having the better of the struggle in the major disputes and the miners winning the greater number of victories in the more numerous localized conflicts.

In an essay entitled "Irish Immigrants and Scottish Radicalism, 1886-1906," Ian Wood steers a course between those who have dismissed the Irish in industrial Scotland as a "festering sore" of nationalism and those who viewed the Irish as an integral part of a solid proletariat. He argues that the late nineteenth century was an

important transitional period in which nationalistic forces were in an uneasy tension with the more secular working-class political impulses.

Barbara Robertson discusses those factors which militated against trade unionism and class consciousness in Scottish agriculture in a study of "The Scottish Farm Servant and His Union." She argues that Edward Thompson's definition of class consciousness (which is developed in *The Making of the English Working Class*) cannot be applied to the Scottish agricultural situation. Instead she introduces a sociological concept of "citizenship" (which is presented as "a much bigger thing than class-consciousness") to explain the fuller participation of agricultural workers in the life of twentieth-century Scotland. At best this study introduces a confusion into the study of class-consciousness. Certainly it appears to be irrelevant to Thompson's study of an earlier working-class culture in another country.

James Treble fills in a blank area on the map of labour history with his very solid study of "The Market for Unskilled Male Labour in Glasgow, 1891-1914." He defines this section of the labour force in terms of occupations, area of recruitment, and earnings. The predicament of the unskilled worker is portrayed in stark terms. He lived close to the poverty line and was often unemployed. His wife and children swelled the ranks of the casual labour force to supplement the family income. Yet he was less mobile than the skilled worker because he could not afford to spend money to travel to work. Also he was tied to his place of residence by virtue of his dependence on the earnings of other family members. These problems and the general surplus of unskilled labour in Glasgow produced "a peculiar air of hopelessness" among these workers.

John Butt's study of "Working Class Housing in Glasgow, 1900-39" effectively argues that the forces of private enterprise and the free market were hopelessly

inadequate in dealing with the city's chronic housing problem. Even the systems of subsidies for slum clearances and rebuilding provided by Labour administrations met with only limited success. Indeed the author concludes that the overall failure to deal with the housing problem in Glasgow can only have led to greater class antagonism.

The editor's contribution on "Some Aspects of the 1926 General Strike in Scotland" tells a familiar tale from a regional perspective. MacDougall shows that although rank-and-file support for the strike was solid, from an organizational point of view it was an abysmal failure. Some workers were sent to picket the transportation systems kept in operation by strikebreakers. Other trade unionists were told to go off and play football. By contrast the authorities were well-prepared; they held overwhelming military force in reserve, and they could afford to turn away thousands of volunteer strikebreakers (including large numbers of Scottish university students).

William Thompson writes on "The New Left in Scotland" and describes those events which paralleled the emergence of left-wing critique in England after 1956 which rejected both Labour and Communist parties. The movement was an intellectual one in that it was largely confined to teachers, writers, and university lecturers (such as the labour historians John Saville and E.P. Thompson). The same picture held true for Scotland except for the Fife Socialist League which was more broadly-based than most New Left organizations.

In the endpiece John Caldwell offers a preview of his forthcoming biography of Guy Aldred. Caldwell was a close personal associate of Aldred for thirty years and his sketch has all the merits of an intense personal attachment to his subject. He shows us Guy Aldred — the Antiparliamentarian — whose long career bridges the whole era from the Social Democratic Federation of the turn of the century to the anti-nuclear

protests of the 1960s. In between Guy Aldred wrote hundreds of books, articles and pamphlets on politics from his socialist-anarchist perspective. He was imprisoned for refusal to serve in World War I; for political agitation in 1909, 1919 and 1921; and for publishing a birth control pamphlet in 1922. He ran for Parliament on several occasions, usually polling a few hundred votes, and all the while declaring his firm opposition to parliamentary government. He was a stubborn atheist who insisted on going about Glasgow in Victorian Knickerbockers, who did not believe in marriage, and who denounced most of the labour struggles or campaigns in Scotland for various reasons. The author's admiration for and friendship with Guy Aldred is both an advantage and a disadvantage. He does not explain why we should not dismiss Guy Aldred as a fringe radical who is in fact outside the labour movement. Perhaps we must suspend judgement on Aldred until further studies are published.

A minor point of general criticism concerns the use of a variety of terms such as "the masses," "the working classes," and "the working class." It is not clear whether these are used as vague and interchangeable expressions or whether they represent the different ideological perspectives of the contributors. Some editorial controls in this regard would have been helpful. In spite of these inevitable imbalances in a collection of essays, this book is to be highly recommended. It provides evidence of the healthy state of Scottish labour history and will be of interest to both British history specialists and labour historians seeking comparative perspectives on American and Canadian questions.

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Peter Burke, *Popular Culture in Early Modern Europe* (London: Temple Smith 1978).

PETER Burke has long been an industrious and, above all, enthusiastic proponent of comparative historical studies. Characteristically, he deliberately sets the limits of his current book as broadly as possible. Burke argues forcefully that popular culture was a unifying factor in European life and that its variants right across Europe need to be brought within the scope of a single study. It is hard to imagine anyone else having either the energy or linguistic skills to pursue this subject from Scandinavia to Italy and from Britain and the Netherlands to eastern Europe and Russia. His chronological framework is also ambitious. In Burke's view, the entire span from 1500 to 1800 forms a single intelligible unit in the history of popular culture, a judgment that carries with it the advantage of allowing the historian to draw upon the relatively rich documentation of the eighteenth century to fill out the patterns of earlier times.

Burke describes his enterprise as "a set of nine linked essays on major themes." We cannot expect, then, a single unified and comprehensive treatment. The opening and closing sections, however, create a rather neat frame for the diversity of materials that make up the body of the book. In his first essay, Burke describes at some length the growth of interest in folk materials and popular culture among Europe's educated classes in the late eighteenth and early nineteenth century. At first sight this may seem an odd introduction to his subject and its full relevance is not apparent until we come to his closing chapters, entitled "The withdrawal of the upper classes" and "From withdrawal to discovery." Burke's contention is that this new interest in popular culture is the sign of the gulf that grew up between the life of the people and that of the elite. "As the gap between the two cultures gradually widened," he writes, "so some educated men began to

see popular songs, beliefs, and festivals as exotic, quaint, fascinating, worthy of collection and record." The reason for this withdrawal was that "learned culture changed so rapidly between 1500 and 1800. . . . European popular culture was far from static during these three hundred years, but it did not, indeed could not, change so fast."

The second and third essays are devoted to methodological discussion. Reviewing a variety of models, Burke argues that in early modern Europe popular culture was the common property of all classes. The elite had its own "great tradition" from which the masses were barred by lack of literacy and education, but the upper classes were equal participants in the "little tradition" of the majority. Between these two groups there was considerable traffic and Burke identifies a number of mediators, while resisting the idea that influences travelled in one direction only. Popular culture was a common property in another sense too. Burke is sensitive to the importance of occupational and regional cultures, but the main thrust of his study is an emphasis on the unifying pattern. "It is," he writes, "one of the central arguments of this book — and its only justification — that the regional level is not the only level at which popular culture should be studied."

Direct evidence about the details of popular culture is notoriously difficult to come by, and Burke advocates a mixture of oblique approaches. These are put to work in the next section of the book, in which some specific themes and contexts (e.g. Carnival, popular performances) are discussed. Finally, the remaining essays take up the "reform of popular culture," first by the clergy and later by the laity. As already noted, this attempt to discipline and purify popular life — carried out initially under the auspices of the Reformation, both Catholic and Protestant, and subsequently influenced by secularist and scientific impulses — created the split between

popular and elite cultures that was Burke's point of departure.

This brief summary does little justice to the many virtues of this book: the range and richness of its sources, the author's breadth of curiosity, and his sensible and often sensitive observation. These qualities mark out Burke's study as work of a high order and an excellent survey of a difficult topic. Nonetheless, some reservations remain in my own mind, and these concern certain of the basic assumptions behind the work.

Burke sees the relationship between the cultures of the elite and the mass as remarkably free from conflict. It is true that he devotes valuable chapters to the reform of popular culture from above, but the emphasis here is on propriety and godliness, not the conflict of vital interests. Material circumstances, in fact, enter very little into his definition of popular culture, and with the exception of groups like shepherds and sailors whose distinctive lifestyles set them apart, Burke gives surprisingly little attention to the daily work or material needs of the ordinary peasant or townsman.

The assumption that popular culture was the common property of the elites and the masses also acts to reduce the importance of social conflict. Burke accepts, for instance, though with some qualifications, the view that rituals of disorder like the charivari ultimately confirmed and strengthened the social order. It may well be so, but one would also like to know more about the ways in which the masses organized rituals and institutions to protect themselves from the social order. This question, however, is difficult to approach from the assumption that popular culture belonged to elites and masses alike. By the same token, Burke's identification of elite culture with the "great tradition" raises the equivalent problem: did the elites not have exclusive folkways of their own having little to do with education or literacy?

Finally, a word about the comparative

method itself. The following passage is not, I believe, atypical:

Dances for groups seem to have been dominant in this period, especially the round-dance and the weapon dance. The Dalmatian *kolo* or 'wheel' was vividly described by an Italian visitor to the region in the late eighteenth century: [description omitted]. The *kolo* (Bulgarian *horo*, Rumanian *hora*) had many varieties and was well known in the Balkans. Round dances, whether or not they were danced so wildly, were common in western Europe too; the Catalans had their *sardana*, the French their *brandle*, or, in the 1790s, the *carmagnole*, danced round the tree of liberty or the guillotine. The *farandoulo* of Provence, in which the participants hold hands and dance in a line, may be seen as an adaptation of the round to the long narrow streets of a traditional urban culture. (116-117)

Surely if the comparative method is to be anything other than a dictionary game, we need to be told either considerably more or considerably less. As it stands, once we recover from its linguistic dazzle, such a passage leaves us very much where we started.

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Patricia Branca, *Women in Europe since 1750* (London: Croom Helm 1978).

In *Women in Europe since 1750* Patricia Branca constructs a "conceptual frame" for women's history. Building on *Silent Sisterhood. Middle Class Women in the Victorian Home* (1975), she adds material on working class women and stretches her theories to include the Continent. It is a worthy project, ambitious and necessarily flawed, deserving a careful reading.

Branca's unifying argument is that women in the nineteenth and twentieth centuries successfully modernized their work, family, and public roles. By modernization she means a creative response to economic transformation, a response which involved rational choices, marriages based on sentimental not economic

grounds, privacy for the family, and women's control of their own fertility. This definition is familiar from her early work and is largely shared by Edward Shorter. Both argue that industrialization was beneficial; both reject any notion of a pre-industrial golden age.

I applaud Branca's determination to show women shaping their own lives but I find she exaggerates their ability to do so. Chapter Two, on women in the labour force, describes a female work-ethic: short-term work, frequent changes of employer, a preference for the respectable over the remunerative, a conviction that paid work was supplementary and temporary, in short a means to the end of marriage. We are told that this adds up to a more successful response to industrialization than that made by men (46, 49, 218-220). So it may have been, but was it entirely voluntary and was it an exclusively female creation? Branca does note male hostility to women in some trades, and the lack of apprenticeship and training programmes for them in others. This evidence undermines her contention that women's uneconomic choices were deliberate and free. Moreover, Branca does not explicitly deal with the role men played in shaping women's values, although men wrote much of the advice literature she cites. The book needs more intellectual history (i.e. on the woman question) along with its social history. Without this Branca cannot convince me that great-grandmother alone decided that marriage and family were to be women's primary values.

Intellectual history is one missing dimension in this work. Another is the position of women in pre-industrial society. Unless we know where women stood before industrialization began, how can we evaluate its effects on them and their response to it? It would be valuable to know, for instance (although to find out would be a major project in itself), how the replacement of customary law by Roman law since the Renaissance — a track which via the Code Napoléon leads directly into

the nineteenth century — affected women. I suspect that the evidence to be found along this route would strengthen Branca's argument that the later nineteenth century was a period of progress by and for women.

While some of the legal and economic evidence from the early modern period might strengthen part of Branca's thesis, other kinds of evidence should erode her conviction that the nineteenth century saw women discover a right to sexual pleasure (11-12, 88-89, 96-97). Women in late medieval and early modern times were considered to be the lusty, pleasure-seeking gender. Branca rejects Boccaccio's descriptions since his female characters are women of the elites (88) but Boccaccio's insistence on female delight in sexual play seems to me to be a message about his view of the gender not the class. If literary evidence will not do, we have the legal and theological treatises on witchcraft. Consider the classless condemnation made by the authors of the *Malleus Maleficarum* in 1486: "All witchcraft stems from carnal lust, which in women is insatiable." Somewhere along the road from the Renaissance our culture decided that Tiresias was wrong, and that men were the "sexier" gender. When? Why? In arguing that women too may demand sexual pleasure, Dr. Michael Ryan's early nineteenth century *Physiology of Marriage* was inventing nothing. He was closely paraphrasing the standard teaching of late medieval confessors' manuals. What Branca is dealing with is a revival, not a discovery.

Branca is at her best when writing about English women. Europeanists will have to question whether she has allowed herself sufficient space to deal with continental variations, such as the speed of industrialization in different regions. They will certainly be surprised by the absence of any sustained treatment of the Code Napoléon (a few clauses are discussed, 160 ff.), although its effects were formidable and certainly not confined to France.

Likewise there is little on Continental feminism; the absence of the Germans is particularly glaring. In assessing the notoriously weak suffrage campaign in France, Branca does not entertain the possibility that the rather farcical politics of the Third Republic itself made winning the vote a less vital issue here. The right to vote for a deputy was not the same as the right to vote for an M.P., and this variety needs to be remembered. Europeanists will also be puzzled by the cursory treatment of World War I (of two relevant notes, one refers only to Great Britain, and the other to the United States, 17, notes 62 and 63). There is nothing on the interwar years and nothing on World War II.

Croom Helm has done Professor Branca and her readers sloppy service indeed. The text shows little sign of having received the attentions of a copy editor and the bibliography in particular is riddled with typographical errors. Both the index and the bibliography are severely truncated. For all this mass of problems, Branca's book is worth serious attention. We badly need synthetic work to make a forest of the trees of women's history. Branca has had the courage to begin the job. Rather than recite cases, she has made an interpretation. Her interpretation is provocative; we all have something to learn from the debate which must follow.

L.J. Abray

Charles Tilly, ed., *Historical Studies of Changing Fertility* (Princeton: Princeton University Press 1978).

FOR BETTER or worse — and opinions will (and do) differ — historical demography is in the midst of its most vital period of growth. Publication of journal articles (too numerous to cite, but virtually unavoidable), collections of essays (such as that under review here), monographs, methodological essays and guides, incisive, mature criticism and stock-taking, and

even a few, brave forays at general reinterpretation and theoretical explanation have become frequent. Indeed, it may not be too hyperbolic to suggest that demographic questions and techniques have come to dominate much of family, social, urban, economic, and sometimes even political and labour history. Historians at work today will find it difficult to escape some contact or encounter with demographic research. Neglect and inattention come only from ignorance or conscious avoidance; neither of which, I argue, is wise or sensible for the growth of historical knowledge.

The reason for these too-brief preliminary notes lies in my conviction that, on the one hand, historical demography is here to stay; on the other, that non-demographic historians need to come to terms — conscious and intellectual terms — with its *potential* contribution and value to their own studies. Yes, demographic works have (and will continue to) suffered from a lack of readability and clarity, an excess of mathematics and jargon, a neglect of historical context, a general contradictoriness and inconclusiveness, an invasion of social scientists, a concern with the application of historical patterns to the understanding and solution of present-day population problems, and sometimes an emphasis on general theory to the detriment or exclusion of historical knowledge. Other problems and obstacles to communication exist as well. Not all of the relevant research is so obscure, technical, or problematic, however; labour, working-class, and social historians cannot afford *not* to take notice of this research.

These are strong claims, which are not always borne out by the written results of historical demographic research, such as that reported in the volume under review. As in all collective efforts, these papers, the product of a 1972 three week seminar, sponsored by the Mathematical Social Science board, are diverse and uneven: in subject matter (fertility analysis is a common

ingly broad and sometimes ungainly area), quality of findings and their interpretation, significance of questions and their resolution, and level of theoretical and technical virtuosity. The volume, however, reflects well the achievements and importance — and the persisting weaknesses and limitations — of historical demography. The most important essays are those by Richard Easterlin (offering a theoretical framework to synthesize economic and sociological questions and dimensions), E.A. Wrigley (struggling instructively and intelligently with questions of individual and group behaviour and their interpretation), Lutz Berkner and Franklin Mendels (on fertility, family structure, and inheritance patterns in western Europe, 1700-1900), and Rudolf Braun (continuing his magisterial analysis of the impact of early industrialization on eighteenth-century Swiss rural society). Interestingly, these constitute two theoretical, nonempirical statements and two fine, more data-based essays. The other contributions, by Ronald Lee (econometric models of preindustrial English patterns), Maris Vinovskis (regression analysis decline to 1850 Massachusetts' fertility levels), and Etienne van de Walle (French fertility decline to 1850) are less intrinsically interesting, less exciting, but nonetheless worthwhile for specialists in those times and places. Finally, Charles Tilly provides excellent contextual and critical comments in his long introduction and his thought-provoking conclusions. Anyone wishing a clear and comprehensible introduction to the analysis of fertility would do well to consult Tilly's contributions. Readers, I expect, will use this volume selectively; only specialized students of population in the past will read all its parts.

Through the example of fertility, the complexities and perplexities of its interpretation, and the many related factors which its analysts must take into account, this volume supports my claims. To put it tritely but no less truthfully, historical demography seeks to comprehend the

basis of life in past populations; its subjects are the "vital" processes of birth and death, as well as marriage and migration, and the changes in their patterns and determinants over time and space. Without some understanding of the most general and crude trends in population, historical understanding is incomplete. Furthermore, an awareness of the ways in which demographic phenomena and patterns intersect "vitality" should be one element of virtually all historians' mental equipment. For students of labour and working-class history this is especially important. The making and shaping of a working class is in fair part a demographic topic; the Braun and Berkner-Mendels essays illustrate this well, as do Tilly's comments. Proletarianization, the epochal transformation of Western society from a "traditional" agrarian, peasant, and land-based social and economic order to a capitalistic, commercial and industrial-urban domination, is rooted in population processes: components of growth, expectations of life, standards of living, migrations, and so forth. While we must guard against a demographic-determinism, as Robert Brenner has recently reminded us, the key complex of interconnected processes and changes cannot be understood without regard for population factors. From economic change to mass movements, changes in modes of production and social controls, reorientation of community and social organization, technological innovations, cultural orientations, life standards and life styles — in *either or both* consequences of causes, actions or reactions, individual or collective behavior, small or large-scale processes — demography plays roles of some significance.

Historical demography, finally, offers an important epistemological and methodological perspective and advantage to social and working-class historians. More than most other approaches, novel or traditional, it is rooted in collective biographies or ordinary lives. It allows us to gain some small access to the vital events, decisions,

responses, and life courses of numerous common persons, who in few if any other ways appear in the historical record. In some measure, then, demographic approaches — especially if not utilized in an intellectual or contextual vacuum — contribute to the understanding of the shape of past lives, the comparison of basic and primary experiences of individuals and aggregates from different classes, regions, occupations, times, and ways of life.

Many of the published products of historical demographic research hardly realize the claims I advance for them; much remains on the agenda. Tilly's collection has the advantage, however, over some other recent publications of being more firmly *historical*, in fact than demographic. The theoretical forays are either grounded or framed in ways which might be testable. This is important; for the larger field is a fragmented one: demographers ask different questions and consider historical context and historical explanations rather differently than historians do. They differ in what they expect from their analysis of historical sources. As Tom Hollingsworth has aptly written, "For the historian, demography is an exciting new tool, giving a promise, with its exactness and its established methods, of leading him towards, and even of his reaching, new conclusions about historical events, and deepening his understanding of history in general. . . . Demographers, on the other hand, could scarcely see historical demography in a more different way." It is *historical* demography (or as some prefer, demographic history) about which I entreat and exhort here.

In the final analysis, demographic research in history has advanced greatly in its empirical and methodological depth and in its chronological and geographic breadth. These gains, however, have not been accompanied by corresponding progress in understanding and interpretation: the basis of *historical* knowledge. Demographic historians need — now more than ever before — the interest, assistance, and

criticism of other historians. They have much to offer the rest of us; we too can help them. Mutual interests and intellectual progress will only come with more communication and exchange (in frequency and in intensity of contact) — the process is reciprocal and dialectical. *That* is the next step to be taken. . . .

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Alan Wolfe, *The Limits of Legitimacy: Political Contradictions of Contemporary Capitalism* (New York: Free Press 1977).

DURING THE 1970s, the future of liberal democracies has become controversial precisely in those countries in which it has been most deeply rooted. The economic crisis has played its part, yet it alone cannot explain the scope of the debate on the nature of the state in advanced capitalism. The flowering of radical politics in the late 1960s, the rise of Eurocommunism, and the resurgence of conservatism have helped to take the debate beyond issues such as budgetary restraint, deregulation, and reprivatization to question the basic relationship between democratic political forms and capitalism. Alan Wolfe's *The Limits of Legitimacy* is an American contribution to this important debate.

Thus while the book begins with a consideration of six sequential ideal types of the liberal/democratic state, these should not be judged by historians' standards. Rather, they serve to underline Wolfe's main point: that the capitalist state displays a permanent instability, induced by the irreconcilable contradiction between liberalism and democracy which lies at its heart. While C.B. Macpherson's readers will not find anything novel in this, Wolfe's emphasis should be understood as an attack on the twin tendencies to ultra-leftism and apathy supported by instrumentalist and functionalist analyses of the capitalist state. In this, he echoes the call to struggle for the democratic road to

socialism that permeates recent contributions of European Marxists. Wolfe's own conclusion is that the late capitalist state has exhausted any real possibility for reconciling liberalism and democracy. As economic and political elites cynically prepare to jettison the latter, the resulting "legitimacy crisis" prepares the ground for democratic and socialist advance.

In addition, Wolfe's analysis has the merit of trying to mount a direct attack on neo-conservative and neo-liberal formulations. For example, his discussion of the "reified state" confronts the criticisms of public bureaucracy levelled by public administration theorists like Michel Crozier and Aaron Wildavsky. This kind of confrontation is important as these theorists are providing an interpretation of the flaws in an institution central to the contemporary state — an institution that can be made a scapegoat for the failures of liberal democracy. These diagnoses should not be allowed to pass unchallenged by the left.

Wolfe's theorization, however, is not adequate to the task he sets himself. The major flaw lies in his conception of the class struggle. Classes only appear as shadows reflected in the state through the contradiction between the principals, liberalism (the bourgeoisie) and democracy (the working class). Nowhere does he distinguish the working class from other subordinate classes present in capitalist societies. He thus fails to see the importance of inter-class alliances in establishing democracy and in carrying forward the struggle for democratic socialism. Nor does he analyze the concrete forms of popular struggle and their specific effects on the state. This results in a conception of state forms in which popular victories are largely treated as illusions culminating in the "alienated politics" of the contemporary state. This poses a major problem for, by failing to identify the social forces which have supported and undermined the contemporary capitalist state, he cannot provide a concrete assessment of the stakes of the current struggle.

These points can be illustrated by comparing his treatment of the present crisis to one which specifically addresses the question of the configuration of classes operative in advanced capitalist states. Wolfe diagnoses the present crisis as one of (moral) exhaustion. While he refers to James O'Connor's and Ernest Mandel's analyses of the socio-economic contradictions of late capitalism, these are not integrated into his political assessment. Rather, he focuses on two aspects: the "reified state" and "alienated politics." The first refers to the decay (the stench of Watergate permeates his chapters on the contemporary state) and irrationality of the bureaucracy. The latter, according to Wolfe, lives in its own world of lies and illusions. Thus, at the very time that the state is worshipped for its restorative powers, the muscles and sinews with which it is to perform its tasks are weakened by internal rot. The second refers to the "schizophrenic citizen" whose originally broad conception of political life is distorted through socialization to a narrow definition of politics and through the activities of interest groups and political parties which alienate community power just as the capitalist alienates surplus value from workers. While Wolfe argues that this tendency has always existed in liberal democracies, he suggests that under late capitalism, "the antipolitical needs of liberalism conflict so strongly with the politicizing desires of democracy that there is a standoff. Politics, in its original sense, increasingly interferes with the solutions that are advanced to contain the contradictions inherent in capitalist society, and therefore politics must be suppressed." (294)

For Wolfe, the main options are as follows. From the standpoint of liberalism, a corporatist state seems the strongest bet. This would entail a close relation between private monopolies and their public sector supports; restrictions on freedom of assembly and speech; further depoliticization via state-funded parties and incorporation of

"responsible" trade-union leaders; incomes policies; maintenance of a certain level of welfare expenditures. The democratic option, from Wolfe's viewpoint, would involve struggles to maintain social services and the hoarding of popular power via apathy and counter-cultural activities ("citizens' strikes"). These would be complemented by repoliticization through a party of movement which eschews the elite/mass relation that, for Wolfe, characterises all parties in liberal democracies.

This analysis can be criticized on several grounds but the central problem is the failure to identify the contemporary state in relation to a specific class configuration. This prevents Wolfe from grasping why "corporatism" has appeared on the agenda in advanced capitalist polities. Corporatism can only be explained once the position of the organized working class in advanced capitalist states is specified. Further, by failing to identify the political presence of the "new middle class" and "marginal" sectors of the working class, Wolfe cannot pose the question of alliances — a question which lies at the centre of the work of theorists like Poulantzas and Lac-lau.

Wolfe's analysis of the postwar state emphasizes the split between the theatrical spectacle of electoral politics and the covert activities of repressive agencies such as the C.I.A. While this is not surprising given the political importance of such agencies in the United States, Wolfe needed to include what Ian Gough has called the postwar settlement: collective bargaining rights for industrial unions and full employment policies, including welfare measures. These working-class victories may have reinforced business unionism even in Western Europe where socialist and communist trade union centrals are strong. These public policies, however, have contributed centrally to the economic crisis and thus the current questioning of the scope of the state. It is in this context that corporatism appears as a solution — one which recognizes, while attempting to

tame, a strengthened trade-union movement. Yet corporatism is not the only solution being offered. Less "liberal" forms include attempts to redefine the rules for union recognition (e.g. the Nova Scotia government's "Michelin" bill) and to separate rank and file workers from trade union activists (the Thatcher government's Leyland plebiscite).

An analysis which locates the postwar state and its crisis in relation to the working class is more satisfying than Wolfe's on political as well as logical grounds. It pinpoints the basis of the postwar strength of trade unions and thereby indicates the rights which must be defended. In addition, it suggests the dangers and possibilities attendant on the corporatist solution. Certainly, the trade unions must avoid "socialism in one class" — incomes policies oriented to achieving redistribution *within* the working class. At the same time, corporatist proposals place the issues of industrial democracy and workers' rights to shape national economic policy, on the agenda of trade-union movements. Whether corporatism operates as yet another temporary solution to the class struggle largely will be determined by the relative strength, organizational and strategic, of the two main classes. Analyses more precise than Wolfe's are needed if academics are to make their contribution to this struggle.

To argue that the struggle between the bourgeoisie and working class is central is not to suggest that other strata and classes do not have an important part to play. The

existence of social movements involving unorganized workers and the "new middle class" adds important dimensions to the struggle. Those employed in the public sector — which Wolfe's analysis tends to write off as hopelessly corrupt — are also a force to be reckoned with. These can be rallied to the cause of democratic socialism but they could become supports for a restructuring of the capitalist state. A Marxist analysis of the present crisis must identify these forces — their general characteristics and their specifically national features. It must identify the strategies presented to win their support for neo-conservative and neo-liberal solutions. Finally, it must consider the conditions for establishing a principled alliance between them and the organized working class.

These kinds of questions are being raised in Europe where Eurocommunist and socialist parties are prominent on the political scene. Americans and Canadians, however, work in a very different political environment. This may help to explain Wolfe's lacunae. Yet as Wolfe himself suggests, advanced capitalist societies and states do display common features as well as significant patterns of variation. Left intellectuals in both Canada and the United States need to develop an analysis which shows how the questions raised by European Marxists could be answered in their own countries.

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