Labour/Le Travailleur

Reviews / Comptes Rendus

Volume 8-9, 1981

URI : https://id.erudit.org/iderudit/llt8_9rv01

Aller au sommaire du numéro

Éditeur(s)

Canadian Committee on Labour History

ISSN

0700-3862 (imprimé)
1911-4842 (numérique)

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Citer ce compte rendu

(1981). Compte rendu de [Reviews / Comptes Rendus]. *Labour/Le Travailleur*, *8-9*, 349–422.

REVIEWS/COMPTES RENDUS

A.B. McKillop, *A Disciplined Intelligence: Critical Inquiry and Canadian Thought in the Victorian Era* (Montreal: McGill-Queen's Press 1979).

A DISCIPLINED INTELLIGENCE studies the labours of a group of Protestant intellectuals who tried to confront at a broadly philosophical level a crisis of moral authority in the Victorian period. The crisis, according to McKillop, grew out of a tension between the imperatives of rational inquiry and the need for religious certainty. As intellectuals these men participated in the expanding world of rational observation and experimentation, but as men of faith they were also aware of the way this approach to knowledge was undercutting the morality that it was supposed to reveal and sustain. The intellect was forced to abandon the formerly secure foundation of Scottish common sense and Paleyite natural theology and to set out on a long and tortuous journey towards Hegelian idealism, and its manifestation in real life, the Social Gospel.

This interpretation deepens and amplifies what is coming to be a generally accepted understanding of Victorian thought. McKillip's work builds directly on the Irving's study of philosophy in Canada, his sense of crisis, or disorder, is present in other studies of the period (Bliss and Shortt for example), and his search for a new vision that could transcend the tensions of the present dispensation complements Carl Berger's interpretation of imperial thought. Nonetheless, McKillop's study adds to this picture in at least three important ways. It elevates the quality of Canadian intellectual life; it sets out the institutional context of that life (the University); and it analyzes in some detail two specific issues: the reaction to Darwin and the Canadian origins of the Social Gospel. The latter issue is especially valuable for understanding the intellectual roots of radicalism and reform. The image of the intellectual that emerges from the book is highly rational and moral. A host of rather dewar and mostly Scottish philosophers move through a changing world led on by their faith and a sober and rational intelligence. The romantic side of Victorian culture, that is so evident in literature and art, retreats into the background.

While the book adds to a growing consensus, it also moves significantly beyond the mainstream of Canadian historiography in general and Canadian intellectual history in particular, and these attributes mark the real importance of this work. The standard criticism of intellectual history argues that this genre is narrow in scope and questionable in significance. Intellectual history is little more than the history of intellectuals — at best a branch of biography, at worst a study of the impotent and unimportant. While McKillop discusses in considerable detail a number of intellectuals, his work is not in essence biographical for the central character is in fact the structure of the intellect itself. In his hands the mind takes on a life of its own as an internal logic drives it down seemingly conflicting paths. Furthermore, he tries to place the intellect (and this particular tension) at the centre of Victorian society by arguing that a broadly accepted moral system (Northrop Frye's myth of concern) is a necessary prerequisite for a sense of social cohesion. While the social system which this moral mythology should sustain remains frustratingly vague, McKillop's concern with the form and function of the intellect certainly places intellectual history upon a much surer foundation.

Beyond this *A Disciplined Intelligence* is a strong affirmation of the importance of idealism in Canadian historiography. The dynamics of the intellect lead on to an idealist synthesis that becomes the springboard for reordering the material world through social action. It is Hegel turned right side up. Indeed the book becomes a part of what it has beheld. McKillop uses an idealist framework to describe the origins of the idealist tradition in Canada — a tradition to which McKillop himself conforms.

And this symmetry provides, at least by implication, a commentary upon the proper character of the historian and intellectual in Canadian life. The intellectuals McKillop describes are moral and broadly humanistic; they are concerned with broad philosophical questions in their fullest social and cultural setting. He treats the intellectual as a moral philosopher rather than a professional disciplinarian. In the same way that these men had to confront the limitations of empiricism and common sense in order to achieve an idealist synthesis, so McKillop has tried to overcome the narrow objectivity and anti-philosophical bias of Canadian historiography in order to elevate an intellect and study it in a fine cultural context. The book itself is McKillop's own testimony to the role of rational intelligence in the field of Canadian historical scholarship.

William Westfall
York University

Serge Gagnon, *Le Québec et ses Historiens de 1840 à 1920: La Nouvelle France de Garneau à Groulx* (Québec: Les Presses de l'Université Laval 1978).

SERGE GAGNON has analysed the works of 16 Quebec historians (the group includes one assimilated Frenchman and one French kindred spirit) who wrote on the history of New France between 1840 and 1920. His aim is not to illuminate the history of New France; with few exceptions, he does not discuss the veracity of the works considered. Rather, these historians are his primary sources for an investigation of the intellectual history of nineteenth-century Quebec. He is particularly concerned to elucidate the relation of the intellectual product of his historians with the movement and metamorphosis of their society in time — the relation of history to History. So this is a book for the nineteenth-century specialist, although the New French aficionados will be grateful to learn, at last, what is in all these books they never read.

The content analysis is always fascinating, and the writer unfailingly picks out the threads of reasoning that lead straight to the heart of nineteenth-century French Canada's preoccupations: its fears, its ambitions, and its conception of itself. One of Mr. Gagnon's most clairvoyant investigations is of religious biographies. These, he finds, were really not history or biography at all, but hagiography, catalogues of virtues and providential interventions intended to speed canonizations, strengthen faith in the supernatural, and provide models for right conduct. The models thought suitable for emulation say a great deal about the writers' society. Foremost is the strong churchman, the defender of the Church against the encroaching state, a role sometimes filled by women with manly voices. But there is also the obedient religious or lay person, in particular manual workers and women, submissive in their God-given roles. Secular biographies are similar; heroes such as Cartier and Champlain are presented as collages of virtues, religiously motivated.

Of particular interest to the readers of this journal is the demonstration of how history was used to preach due subordination to workers. There is the example of Jouve's *Didace Pelletier* (1910), the story of a manual worker who knew his place, 10,000 copies of which were distributed free to the subscribers of a popular review. There is Rousseau's *Maisonneuve* (1886), which shows that élites must lead nobly so that the people will learn "supporter avec patience et résignation les misères de leur condition." And it is clear that in the fulsome praises of the farmer and the severe strictures against coureurs de bois in so many of these works, the writers are contemplating with fear and distaste the nineteenth-century lumberjack and factory worker. Gagnon includes one maverick, Benjamin Sulte, the most agriculturist of all historians, but one who recognized the history of his day as élitist propaganda. "Nous defendons ici une cause — la cause des Habitants," he wrote.

Here it is not possible to outline all of Gagnon's historiographical demonstrations, which proceed from biography to histories of professions and institutions to the major syntheses of the period. The historians are shown to have mirrored in varying measure, after Garneau, the triptych that Michel Brunet has made familiar: agriculturism, messianism, and a distrust of the state. Brunet's codification can be expanded: this is history as part of a programme of social action in support of an ultramontane, clerically-dominated society without ideological divergence, a society of harmonious hierarchy without class antagonisms, a society based upon family, farm, traditional institutions, and French language.

The point of the exercise, of course, is to relate history writing to social and economic change. In many ways, Gagnon is very successful in doing this. Garneau's liberal synthesis stands out clearly related to the rise of the bourgeoisie of liberal professions in the first half of the century, and the ultramontanism of almost everything that came after is quite correctly related to the failures of the bourgeoisie in 1837-38 and the subsequent clericalization of society. The most shrill agriculturist paeans are skillfully co-related with the rise of urbanization and industrialization. This extends even to the pairing of Sulte's purist agriculturalism to the depression years of the later nineteenth century. There is, however, still plenty of work in this particular vineyard. And all of this new wine will not sit well in the old nationalist bottles.

Mr. Gagnon opts for the neo-nationalist (Séguin-Frégault-Brunet) explanation of French Canadian social and economic development after 1760. In this scenario, because of the Conquest French Canadian society develops in a diminished and one-sided manner, seeking in compensation myths and unhealthy solidarity to save its nationality: *la survivance* but at a cost. Mr. Gagnon accepts nationalism at a cost too. Because he finds his explanation ready-made, he pays little attention to the *renversement de la conjoncture* and to the profound stress on structures in the period 1800-40, limiting his analysis to a perfunctory recognition of the rise of the petit bourgeois. Perhaps more important, he takes no account of what French Canadian society may have been before 1760, which is the essential starting point. Finally, not everyone will have his confidence that the enlistment of Albert Memmi *et al* constitutes science to the rescue, however helpful their models of social development might be. The author's touch becomes surer after 1850, in the handling of the themes of urbanization and industrialization.

The present work gives rise to two reflections on much of our current historical writing. The first is that the conjunctural approach can be excessively mechanical, ideas popping in and out of peoples' heads with the rise and fall of price series. Secondly, the neo-nationalist approach invariably leaves an image of the French Canadian after 1760 as being acted upon rather than acting. This dehumanization of the subject is a danger in social and economic history, and we will no doubt see more of it in histories of Indians, immigrants, workers, and women.

The faults that the present reviewer imputes to this work are reflections of our present historiography. They should not obscure the very real virtues of the original research represented by this volume.

Dale Miquelon
University of Saskatchewan

George Rudé, *Protest and Punishment: The Story of the Social and Political Protesters Transported to Australia 1788-1868* (Oxford: Clarendon Press 1978).

PROFESSOR RUDÉ ANALYZES the patterns of protest in England and Ireland from the late-eighteenth to the mid-nineteenth century and in the Canadas during the Rebellions of 1837-38. The second half of the book examines the experience in Australia

of those protesters transported. The author defines protest as a collective resistance to perceived oppression. A wide range of protesters are, at least summarily, investigated: food rioters, machine breakers, trade union militants, demolishers of turnpikes, Chartists, Scottish Jacobins, United Irishmen, Fenians, Canadien *patriotes*, Upper Canadian rebels and their American supporters, and many others. The author also includes groups of what he calls marginal protesters, that is persons who carried out individual acts but with a sense of social grievance rather than for personal gain or to settle purely personal scores. Examples include poachers who were asserting "common rights" and Irish arsonists acting in the context of anti-landlord agitation. Overall Professor Rudé estimates that about 3,600 of the persons transported (approximately 162,000) were protesters.

Given the diversity of the phenomena studied, it is not surprising that few general themes emerge. After all, the Newport Chartists had little in common with English food rioters, Irish cattle-maimers, or Canadien habitants holding land under a modified feudal system. We do learn that the incidence of protest in industrializing England shifted from the agrarian south to the manufacturing north, but what else could be expected? There is little here about the nature of oppression or the psychology of rioters, beyond what we already know, much of it from Professor Rudé's earlier, excellent scholarship. The marginal protesters remain statistics and vague ones at that.

Nor do we learn much of a general nature about the experience of the protesters in Australia. There is no attempt made, for example, to explain to what degree, if any, the protesters were treated as political prisoners rather than ordinary convicts. We do know from previously published accounts that there was some recognition accorded political motivation. The Newport Chartists, transported to Van Dieman's Land, were allowed to keep their own clothes and were given supervisory jobs. The Canadien *patriotes*, in New South Wales, during their period of assignment, were sometimes permitted to search out their own employers. A few generalizations on this question would have increased our knowledge of the convict system.

Professor Rudé's main concern in the Australian portions is to show that if the traditional and flattering "village Hampden" thesis of early Australian settlement can no longer be taken seriously when applied to all the convicts transported, it still makes sense if restricted to those transported for protest offences. This is proved by literary evidence of the protesters' generally good behaviour and by lower rates of recidivism among protesters than among the convicts as a whole. This finding, while useful, is hardly startling, given that protesters, compared with say thieves, were unlikely to repeat the crimes for which they had been transported and were less inured to crime as a way of life. Not one of the 58 *patriotes* sent to New South Wales, for example, had been convicted of an offence prior to being found guilty of treason. It is not altogether surprising, then, that they managed to avoid any secondary punishments during their period of confinement and to impress Governor Gipps with their exemplary behaviour. One can also question the significance of finding that most protesters deserve the title of "village Hampdens." Could a group of about 2 per cent of all convicts transported have had much impact on the Australian character?

The Canadian portions of the book are particularly weak. Professor Rudé has made little use of the proceedings before the courts martial in both Canadas (1838-39) and none at all of the comprehensive, day-to-day journal kept in New South Wales by the *patriote* François-Maurice Lepailleur (held by the Archives nationales du Québec) or the massive and well organized "*Événements* de 1837-38" series (ANQ) of depositions and voluntary statements. The accounts of the Rebellions are based almost entirely on

secondary sources. Less than half a dozen pages are devoted to the *patriotes* in New South Wales and the Upper Canadians/Americans in Van Diemen's Land. In neither case is the emotional dimension of the ordeal brought out.

There are also numerous blunders, of which a sample follows. William Lyon Mackenzie, we surprisingly learn, never returned to Canada after 1837. (47) The 1838 rebel leader, Dr. Côté, is called Charles instead of Cyrille-Hector-Octave, (48) while the powerful seigneur of Beauharnois, Edward Ellice, MP, emerges as Robert Ellice in the text (97) and Robert Ellis in the index. (262) The General Court Martial, held at Montreal, is made to open a week earlier than it did. (83-4) Notary Hyppolite Lanctôt's claim of non-involvement is accepted at face value (98-99) whereas in fact Lanctôt served as rebel officer at the battle of Odelltown. A more serious error is attributing a British, as opposed to a republican, constitutional ideology to the *patriotes*. (44, 46) The assertion (82, 96) that the repression in Lower Canada after the 1838 troubles was mild compared to the excesses of the previous year seriously distorts the atmosphere of the times. Widespread burning and pillaging by the troops and volunteers, rapes, beatings, mass arrests without trial, derogatory demands for the total eradication of the Canadien way of life, the executions following a farcical and illegal Court Martial, and the transportation of respectable men to the ends of the earth far exceeded anything after 1837 and were long remembered in French Canada.

This book may prove useful to specialists as a source of detail on a wide variety of subjects. Corroboration is, however, recommended. For the student or general reader interested in protest or in the convict system, the previous works of Professor Rudé on the former and the seminal studies of Professors L.L. Robson and A.G.L. Shaw on the latter are to be preferred.

F. Murray Greenwood
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George Raudzens, *The British Ordnance Department and Canada's Canals 1815-1855* (Waterloo: Wilfrid Laurier Press 1979).

QUESTIONS OF DEFENCE rarely involve military considerations alone. As Britain moved to safeguard her North American possessions following the War of 1812, defence strategies impinged on every aspect of colonial life. George Raudzens' study of one little-known department of the imperial administration suggests the magnitude of this military impact. In charge of military property, arms equipment, and fortifications work, the Ordnance Department provided a wide range of technical and scientific services both to the armed forces and to civil departments of the state, helping to shape social and economic development in the process. Though attempting to give at least a passing reference to most aspects of the Ordnance's influence, Raudzens is primarily concerned with the Department's role in the construction of the military canals along the Ottawa-Rideau waterway, particularly the Rideau Canal. He traces the administrative side of construction from the early attempts to secure official approval and adequate funding of the canals, through the petty deceptions and financial wrangling which carried them to completion.

In the thick of the conflicts with imperial and colonial officials and at the centre of Raudzens' study is Colonel John By, known to most Canadians as an heroic visionary, exposed here as a mediocre and dishonest bureaucrat who exploited defects in the imperial administration to pursue his own grandiose schemes. The attack on By is finely-detailed and convincing. More than a revelation of one man's weaknesses, it points to serious deficiencies in the type of history which produces giants like By. Yet Raudzens' conclusions require qualification. Problems endemic to the construction industry and exacerbated by conditions in the Canadas were sufficient in themselves to send costs skyrocketing beyond the most generous projections. Problems

with contractors and the whole contract system, troubles in securing materials and supplies, and the difficulties associated with a labour intensive enterprise in a society lacking a smoothly-functioning labour market — consideration of these problems would have produced a more balanced evaluation of By and the Ordnance.

The imbalance notwithstanding, the book is a careful administrative study of an important department of the imperial administration. However, Raudzens' hope that he will go beyond a study of one department to shed light on the role of the military in post-1815 Canada is barely realized. Trapped by an underlying assumption that the imperial country was rendering a service to her colonies, Raudzens treats the Ordnance and the military in general as benefactors, ignoring the fact that as instruments of British imperialism, the military served specific class interests in the colonies as well as in Britain. Sections of the book read like a catalogue of "services rendered" to the civilian population, while the entire book is lacking in analytical distinctions between those civilians — Canadians, colonists, as he variously refers to them — who benefitted directly from military activity and those who did not.

This superficial approach fails to consider that an integral part of the military's contribution to social and economic development was its role in the process of social class formation and in the conflict between emerging classes. This is nowhere clearer than on the canals of the Canadas. The use of regiments of Royal Sappers and Miners to perform much of the skilled labour on the Rideau is one clear example of the widespread use of both existing and specially created military units to provide disciplined and regimented labour, the advantages of which were not lost on colonial officials such as Lord Elgin who dreamed of organizing all Irish immigrants into military labour units. However, this aspect of the Ordnance's contribution to development is given little consideration in a study which emphasizes the benefits of military, technological, and scientific expertise.

Given no consideration at all is the role of the Sappers and Miners in overseeing and disciplining civilian labourers on the Rideau. In ignoring this, Raudzens misses the military's crucial role in labour control, a role not covered by his reference to "benevolent peacekeeping." He also misses the irony of the Ordnance experience in the Canadas. From the 1820s, Ordnance officers consistently opposed improvement of the St. Lawrence waterway on the grounds that it would render the Canadas vulnerable to American attack. Yet as Britain began her hesitant retreat from Empire, and Montreal merchants realized their dream of a commercial empire, the Ordnance joined other branches of the military in suppressing labour unrest along the St. Lawrence canals. In fulfilling its military commitment to labour control, the Ordnance ultimately was forced to go against its defence objectives, and aid in developing the lines of communication between the Canadas and the United States.

More than simply a matter of choice, the omission of these aspects of the Ordnance contribution to development grew from Raudzens' narrow definition of imperialism and from the false dichotomy of soldier and civilian on which his analysis hinges. Much clearer delineation of divisions within colonial society and within the military itself is necessary if historians are to answer the question which Raudzens poses for himself: "How and why did the military establishment impinge on provincial life in its multiple facets? And to what ultimate effect?" (11)

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John McCallum, *Unequal Beginnings: Agriculture and Economic Development in Quebec and Ontario Until 1870* (Toronto: University of Toronto Press 1980).

ANYONE WHO VENTURES into the vexed field of Quebec's agricultural history

deserves high marks for courage. The argument surrounding the "Ouellet thesis" and the "Naylor thesis" has been remarkably acrimonious and the bloodletting in the *Canadian Historical Review* (March 1974 and June 1975) by Drs. LeGoff and Paquet and Wallot would frighten any but the most venturesome from the field. To his great credit, John McCallum has produced a balanced and civilized study which presents a clear thesis without being ill-tempered or needlessly aggressive in tone. The book is a revision of his 1977 McGill doctoral dissertation and is a model of how a thesis should be transformed into a book. The result is a succinct and readable work, comprehensible to the general reader and challenging to the specialist.

McCallum's work is fundamentally right-headed in that he recognizes that if one is to be true to historical reality one must treat nineteenth-century Quebec and Ontario as full-fledged agricultural, not as proto-industrial, societies. At the time of confederation both provinces were only one-fifth urban and agriculture produced not only the bulk of the cash income of most of the inhabitants of both provinces but dominated export trade as well.

Concerning Ontario, McCallum makes the important points: that not only was agriculture in general the dominant economic sector but that wheat in particular was prepotent; that in terms of cash income wheat was more important to the Ontario farmer of the 1850s than it is to the Saskatchewan farmer of today; that at least until Confederation half or more of this Ontario wheat production was exported and that wheat was the dominant export product from Ontario. In Quebec, in contrast, the wheat economy was relatively moribund. In part, this was because of less congenial climatic conditions, in part because of limited domestic markets, and in part because of transportation difficulties in reaching external markets.

Employing what he describes as "a modified staple approach," McCallum argues for two sorts of linkages. First, between the staple wheat economy and the commercial and urban development of Ontario which initially challenged, and ultimately destroyed, the economic dominance of the province by Lower Canadian interests. Second, he links Ontario and Quebec into a single system of production and distribution, which he feels accounts both for their regional disparities and their economic integration. Even readers who are not convinced by his argument will respect its clarity and certainly it will play a significant part in the continuing debate on Quebec agriculture.

The weakest part of this book occurs not in the centre of the field the author spotlights, but along the periphery. This book implicitly contains a number of comparisons between Quebec and Ontario, the most important of which is the bald fact that in the years before Confederation, the cash income of the average Ontario farmer was reported as three times or more that of his Quebec counterpart. Why? McCallum ascribes it all to climatological, soil, and market factors and refuses to take into account any cultural variables. "Altogether too much importance has been attached to the alleged conservatism, backwardness, ignorance and other unenterprising qualities of the unfortunate habitant" (5), he states, and to the extent that certain cultural characteristics (anti-commercial attitudes, or whatever) have been more asserted by scholars than actually demonstrated, one cannot disagree. However, there are certain cultural characteristics which are both relevant and empirically definable. For example, there is some evidence that the family structure of the French-Canadian family was considerably different than that of the English-Canadian family as it evolved in Ontario. In particular, the French-Canadian family carried more dependants, longer. Obviously, this has a bearing on any comparison of Quebec and Ontario, for it is possible that the lower cash income of the habitant was in large part a result of his having a smaller farm surplus to dispose of, not of his being any less productive.

By focusing on income, rather than upon actual output data, McCallum makes a valid comparison of the Quebec and Ontario economies difficult, if not impossible. And it is on this matter, productivity, that the future of the debate on Quebec agriculture will hinge. Prof. Martin McInnis is presently engaged on a quite remarkable revision of the agricultural data for nineteenth-century Quebec and his work indicates that confusion as to units of measurement (acres and arpents, bushels and minots) has resulted in a considerable under-estimation of the actual production in that province. Until a county-by-county revision of the figures is completed, the debate on Quebec agriculture will remain largely a dance upon quicksand.

Donald Harman Akenson
Queen's University

Raymond Boily, *Les Irlandais et le canal de Lachine: la grève de 1843* (Montréal: Leméac 1980).

LE DÉBUT DES années 1840 inaugure une période nouvelle dans l'histoire du mouvement ouvrier au Québec; en effet, les formes "primitives" de la résistance ouvrière cèdent graduellement le pas à des luttes plus articulées parmi les travailleurs de la base. Le rôle exercé par la Société bienveillante des charpentiers de navire de Québec, durant la grève de 1840, témoigne de la capacité d'une première génération de prolétaires à se doter d'une organisation autonome qui ne soit plus essentiellement dirigée vers des objectifs de secours mutuel, mais qui soit davantage orientée vers une formule de véritable syndicalisme. De même, la grève des terrassiers du canal de Lachine et de Beauharnois, survenue en 1843, nous fournit l'exemple d'une cohésion ouvrière qui a rendu possible la mobilisation massive des travailleurs dans ces deux chantiers de construction d'envergure, près de Montréal. C'est précisément ce dernier conflit que Raymond Boily a tenté d'étudier. Il s'agit en fait d'une reproduction de documents officiels relatifs à la commission d'enquête de 1843, instituée par les autorités gouvernementales "pour s'enquérir des troubles qui ont éclaté sur la ligne du canal de Beauharnois" (et non pas du canal de Lachine comme le titre a tendance à nous le faire croire). Après un bref rappel historique, sur lequel nous reviendront plus loin, l'auteur nous livre les dépositions des principaux acteurs (journaliers, entrepreneurs, contremaîtres, officiers de police, etc.) des événements de Beauharnois, de même que les recommandations émises par les trois commissaires chargés de l'enquête. Bien que cette investigation gouvernementale, extraite des Journaux de l'Assemblée législative de 1843, soit déjà connue de quelques spécialistes qui y font allusion dans leur travaux, elle n'en demeure pas moins un document précieux, en tant que premier cliché officiel sur les antagonismes de classes au Canada, dont on n'a pas encore épuisé la signification historique. Malheureusement, l'auteur, un diplômé en ethnologie, n'a pas toujours su tirer le meilleur parti de cette source, pourtant riche en informations.

Il convient de dégager, à ce stade-ci, le cadre général de la grève des travailleurs irlandais du canal de Beauharnois. En 1842, à la suite des pressions exerçées par les commerçants de Montréal, le Bureau des Travaux publics du Canada-Uni avait pris en charge l'aménagement de la voie maritime du Saint-Laurent et avait entrepris de construire plusieurs canaux, dont celui de Beauharnois. L'année suivante, l'État confiait l'exécution des travaux de canalisation à des entrepreneurs privés au moyen de contrats de sous-traitance. Pour réaliser les excavations sur la ligne du canal de Beauharnois, on avait fait appel à plus de 2,000 ouvriers irlandais venus principalement des États-Unis à la suite de la cessation de l'activité sur les chantiers publics. Dès leur arrivée, les entrepreneurs, regroupés au sein de six firmes, mirent sur pied un système d'exploitation sans précédent à l'endroit des journaliers travaillant sur les 13 sections du canal. Au

mois de mars 1843, les employeurs réduisirent les salaires de 3 shillings/jour, montant attribué sous la direction du Bureau des Travaux publics, à 2 shillings/jour, et ils élevèrent la périodicité des versements, de deux semaines à un mois. Ces gages, inférieur au prix accordé par les entrepreneurs pour la location de chevaux, faisaient dire à Martin Donnelly, un terrassier à l'emploi de la firme Crawford, que le travail quotidien fourni par chaque homme rapportait aux capitalistes une somme variant de six à sept shillings par jour. Quant à la journée de travail sur les chantiers, elle fut prolongée de 12 à 14 heures, probablement afin de pallier les nombreuses interruptions occasionnées par de mauvaises conditions climatiques. Ainsi, l'incertitude du travail chez les journaliers rendit encore plus dérisoire leur maigre salaire. Profitant de l'isolement géographique qui mettait les ouvriers du canal de Beauharnois en situation de dépendance à l'endroit des moyens de subsistance, les employeurs avaient ouvert des magasins, dans lesquels le prix des denrées était plus élevé qu'ailleurs, et ils avaient construit des baraques infectes de 12 pieds carrés, lesquelles étaient louées à des prix exorbitants. En raison de cette cherté artificielle de la nourriture et du logement, les travailleurs du canal devaient s'endetter auprès des compagnies qui les avaient embauchés, ce qui fournissait une occasion rêvée pour celles-ci d'exercer des prélèvements sur les salaires et de faire circuler en vase clos les capitaux investis. Ce système de vol organisé (communément appelé "truck pay") à l'égard de la force de travail fut à l'origine de la grève de Beauharnois en mai et juin 1843. Déjà la résistance ouvrière s'était-elle manifestée puisqu'en août 1842, on avait nommé un magistrat afin de maintenir l'ordre parmi les travailleurs du canal, et, en mars 1843, le gouvernement du Canada-Uni ordonnait à William Ermatinger, directeur de la police à Montréal, d'organiser une force constabulaire provisoire de dix hommes pour patrouiller sur le chantier de Beauharnois. Quoi qu'il en soit, à la fin du mois de mai, les travailleurs employés au canal remirent en bloc leurs outils aux divers contremaîtres et signifièrent leur intention d'arrêter toute activité tant et aussi longtemps que leur gage ne serait pas porté à 3 shillings/jour. Aussitôt, les entrepreneurs fermèrent leurs magasins, provoquant ainsi la colère des ouvriers. Le lundi 12 juin 1843, l'armée intervint à Beauharnois; les actes de violence et les fusillades firent six morts chez les grévistes, si bien que les autorités ordonnèrent une enquête officielle sur les événements de ce "lundi rouge." Celle-ci conclut à l'absence d'un pouvoir civil satisfaisant, capable d'imposer le respect à un corps considérable d'ouvriers. Néanmoins, les ouvriers de Beauharnois obtinrent d'être désormais payés en argent tous les 15 jours; pour sa part, le Bureau des Travaux publics s'engageait à régler lui-même le taux des salaires et à obliger les employeurs d'abolir le système de magasins sur les chantiers.

Le principal reproche qu'on peut formuler à l'auteur est son manque de perspective et d'articulation historique; son exposé sur la grève du canal de Beauharnois, en guise de présentation, ne dépasse guère le récit linéaire et demeure dans les sentiers battus de l'historiographie traditionnelle. Plutôt que de transformer une série d'approximations imparfaites de la réalité en une connaissance structurée de l'objet déterminé d'analyse, comme exige toute démarche scientifique, l'auteur s'en tient à une problématique positiviste qui consiste à éviter les débats de fond pour laisser au lecteur le soin "de porter lui-même le jugement qui s'impose sur la condition ouvrière au Québec au milieu du siècle dernier." (10s) Pourtant, la matière historique "sécrétée" par le commission d'enquête de 1843 constitue en soi un excellent point de départ pour une étude systématique des antagonismes de classes, dans la production et le discours idéologique, à l'aube du capitalisme industriel au Québec. Par ailleurs, on se demande pourquoi l'auteur a délibérément évité d'effectuer les rapprochements qui s'imposaient avec la grève du canal de

Lachine, déclenchée en janvier 1843; à cet égard, les travaux de H.C. Pentland et l'enquête *menée sur ce conflit par* les autorités militaires auraient pu constituer un pôle intéressant de référence.[1] Il est également curieux que l'auteur n'ait pas su exploiter davantage les renseignements inédits, fournis par deux témoignages devant la commission (128-31), à propos d'une grève survenue au canal de Chambly entre 1839 et 1842. Pour saisir l'enjeu *véritable de la lutte des travailleurs* de Beauharnois, il aurait fallu chercher en définitive des explications au sein de l'infrastructure (organisation du procès de travail sur les chantiers) et de la superstructure (mise sur pied d'une dispositif de répression ouvrière).

En ce qui concerne l'organisation du travail productif pour la construction du canal de Beauharnois, les contrats notariés (actes d'engagement et conventions de marché) auraient pu servir d'indicateurs précieux pour cerner les rapports sociaux qui se sont établis entre les agents de production dans cette unité à forte concentration de main-d'oeuvre: ainsi, il aurait été possible de recueillir des informations pertinentes sur la division technique du travail entre les ouvriers qualifiés (maçons, charpentiers, forgerons, etc.) et les manoeuvres (terrassiers) dans les chantiers, sur l'autorité exercée par les contremaîtres et les recruteurs au service des firmes contractantes, et en même temps sur la discipline puis les cadences imposées aux équipes de travail. Une fois cette étape accomplie, il aurait été facile de circonscrire les modes d'appropriation de la force de travail par le capital et de dégager l'ensemble des facteurs qui ont engendré la résistance massive des ouvriers du canal de Beauharnois.

Dans un autre ordre d'idées, l'auteur n'a pas su mettre le doigt sur le véritable but visé par l'enquête de 1843, à savoir, l'anéantissement de "l'insubordination ouvrière" sur les chantiers, afin d'assurer les conditions d'existence à l'accumulation capitaliste. Dans leur rapport sur les événements de Beauharnois, les commissaires ne proposent-ils pas "l'organisation d'un corps de police assez fort pour comprimer des rassemblements [. . .] sur toute la ligne du canal" (190), et l'établissement d'un système de délation parmi les ouvriers, lesquels souhaits seront réalisés par l'*Acte pour prévenir les émeutes sur les* chantiers des travaux publics, voté par l'Assemblée législative du Canada-Uni en 1845.[2] Cette stratégie de surveillance du pouvoir d'État à l'endroit de la nouvelle force sociale des travailleurs organisés sera d'ailleurs complétée par la mise en place de comités de vigilance sous la direction de prêtres catholiques; ces "auxiliaires du *pouvoir policier" joueront un rôle déter*minant dans l'encadrement moral de la masse des travailleurs employés à la construction de canaux.

Plus qu'un événement ponctuel, la grève du canal de Beauharnois constitue un moment historique de la lutte de classes au Québec, et il est regrettable que l'auteur ait occulté cet épisode important de la *mémoire collective pour le réduire à une* formule d'"histoire-marchandise."

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[1] Les documents relatifs à cette enquête sont conservés aux Archives publiques du Canada, dans le fonds RG 8, I, C series (British military records), A1, vol. 60.

[2] Canada, prov. du, Assemblée législative, *Statuts*, 1845, c. 6.

Beth Light and Alison Prentice, eds., *Pioneer and Gentlewomen of British North America, 1713-1867* (Toronto: New Hogtown Press 1980).

THIS COLLECTION OF documents on Canadian women's history is a source of considerable value to the student of women's history, and to the beginning researcher in this field. The wide range of primary source documents which have been compiled include legal papers such as wills, apprenticeship contracts, bills of sale, inventories and petitions, published sketches of daily

life and travelogues by and about women, songs, letters, journals and diaries, school compositions, catalogues and inspection documents, newspaper articles and advertisements, narratives and autobiographies.

Each section is introduced with an essay containing relevant historical information. Each excerpt is preceded by an introduction which explains its origin, gives biographical information on the author, and, most importantly, provides the student with an indication of the document's value to historians. The researcher is provided with archival information preceding each selection in addition to the bibliography appended to the collection. The selections are arranged according to the life cycle of women. The editors first cover childhood, then youth, education, and "spinsterhood." Following these are selections on married women and their work, widowhood, and old age. The final section contains documents on women's role in the public sphere, and on the ideology which structured the private and public lives of Canadian women.

This format is valuable for providing the student of Canadian women's history with a knowledge of the texture of the lives of these women. The selections as arranged document women's productivity and their recognized importance to the Canadian economy, as well as showing how women utilized their moments of leisure. However, given the wide span of time under consideration, the format clouds historical process. The editor's concentration on the life cycle of women over a 150-year period undercuts any sense of change in Canadian women's lives over time. There is, according to the format of this text, little difference between the lives of eighteenth-century Canadian women and nineteenth-century Canadian women. There is little indication that historical events, combined with the activity of Canadian women, changed the texture of their lives over time. The fact that there are so many categories of documents within each section further obscures the historical fact of change through time.

Moreover, the source purports to be an anthology of British-American women from 1713 to 1867. It is in actuality a collection of documents on women who lived during the first half of the nineteenth century. Of the 123 selections, almost 100 are from this period. The few eighteenth-century documents that are utilized tend to cloud rather than elucidate the texture of nineteenth-century Canadian women's lives. Ironically, the format of the collection would have been much more effective (and more historically valid) if the editors had been more sensitive to the limited scope of the documents under consideration. If Light and Prentice had concentrated solely upon early nineteenth-century sources and had attempted to present women's life cycle within this more limited milieu, the need to point out and define change over time would have been minimized.

The fault with *Pioneer and Gentlewomen of British North America* lies not in what it is, but rather in what it purports to be. It is an excellent teaching and research tool for students of early nineteenth-century Canadian women's history. It is not an anthology on the lives of Canadian women from 1713 to 1867.

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Gregory S. Kealey, *Toronto Workers Respond to Industrial Capitalism, 1867-1892* (Toronto: University of Toronto Press 1980).

DURING HIS TENURE at Rochester a decade or so ago, Herbert G. Gutman built up a remarkable graduate program in American working-class history. The fruits of that activity have now begun to appear, including this study of Toronto workers during the late nineteenth century. Gregory Kealey's book bears the marks of what might be termed the Gutman school. It is a community study, richly detailed, highly

specific, thoroughly grounded in the local sources. Kealey has read systematically in the Toronto press, but he also makes good use of the Canadian manuscript industrial census for 1871 and the local records of the Orange Order and the Toronto labour movement. He draws from these sources basic historical data presented in a form useable for other historians — strike statistics, industrial data, electoral returns, the social composition of the Orange order, capsule biographies of Toronto labour leaders. Kealey thus lays the foundation, hitherto largely lacking, for a history of the Toronto labour movement and, in so doing, for a fully satisfactory history of Canadian labour.

It is in the interpretation of this information that the book founders. During the past decade, something of a consensus has emerged about how to think about working-class development during industrialization. From E.P. Thompson we got the notion of a pre-industrial artisan culture transforming itself into working-class consciousness, from Harry Braverman (among others) the idea of shop-floor control as the central issue in the battle between workers and employers. These, in turn, can be fitted into a larger Marxist framework of class struggle. All this, Kealey tells us, he finds in Toronto. He chose "the mid- to late-nineteenth century... in order to capture the Toronto working-class at its inception. The 'making' of this class lay in industrialization and in the workers' response to that process." (xiv) Kealey's Introduction ends with this ringing statement: "The work will have succeeded to the extent that it provides convincing evidence that the working class was not just 'made' but rather was the result of a dialectical interplay of class forces in which it was one of the actors." (xviii)

By that criterion, unfortunately, the book does not succeed very well. Kealey's difficulties are most apparent in his treatment of the shoemakers. The chapter includes a full measure of information on their artisan culture, on their resistance to the factory system, on their struggle for shop floor control. But a close reading of the text and notes reveals that virtually all of this refers to events that occurred outside of Toronto. We discover ourselves reading the findings of Dawley, Faler, Montgomery, and John Hall, not Kealey. This, of course, undermines the principal reason for doing a case study. Equally troublesome is Kealey's inclination to make us see more than meets the eye. On p. 39, he begins a discussion of shoemaker resistance to the sewing machine. There are references to Montreal, England, Lynn, Quebec. When he gets to Toronto, it is not shoemakers, but the journeymen tailors who in 1852 go on strike against the sewing machine. Then Kealey says: "Toronto shoemakers did not resist the sewing machine but used their organization to maintain wage rates." (40) Nothing in the succeeding discussion of strikes suggests that they were reactions against the sewing machine. Kealey does find one instance of machine breaking, but it occurs during a strike in 1871, long after the introduction of the McKay machine. Kealey cautions us not to make too much of this incident (which, the evidence suggests, was as likely the work of the employer as of the Crispins), and then goes on to do precisely that. The next reference to it speaks of "the Crispins' Luddism" (53), and at the end Kealey sees it as "building on traditions of craft pride and solidarity...." (292)

Kealey concludes his chapter by saying that "the history of the shoemakers' response to industrial capitalism consists of much more than strikes and trade-union organization." (52) But those subjects were, in fact, all that Kealey's digging actually yielded. It may be that the Toronto shoe industry does contain "a history of cultural adaptation of old forms to counter the pressures of factory production." (52) But Kealey has not found such a history, and other evidence in the book suggests why not. Among Kealey's most striking findings is the speed of Toronto's indus-

trialization after the 1840s. By 1871, 36 per cent of Toronto's workers were employed in plants of over 100 workers, fully two-thirds in plants of 30 or more, a scale of production certainly comparable to that in the United States at the time. The 49 Toronto shoemakers of 1846 had been succeeded by 1,174 shoe factory operatives 25 years later. Given the small base and the swiftness of the industrial growth, why should Kealey assume that the shoe workers of 1871 felt any "organic link with a past artisanal world" (41) or that they had ever exercised "control... over pre-factory production and over their own time?" (45)

Fortunately, Kealey is not able to sustain this ambitious approach throughout his book. The brief chapters on other work groups are, with some lapses, a good deal more straightforward and modestly stated, probably for lack of a secondary literature comparable to that for the shoemakers. The chapter on the printers is a very nice replication for Toronto of the classic analysis done by George Barnett many years ago. The book opens with two sound chapters on Toronto's industrialization, one dealing with the successful fight for protection, the other describing the industrial growth itself. There is a fine account of the Orange order in Toronto. The final two-thirds of the book consists of a detailed narrative treatment of Toronto labour in politics and of the rise and fall of the Knights of Labor in the city.

The careful reader will, however, want to separate these substantive accounts from the meaning that Kealey superimposes on them. Every strike signifies "class conflict," every labour involvement in public life "class politics." Kealey can so designate these events if he pleases, but he could as well do so for the contents of the most old-fashioned of labour histories. The reader will also want to discount such concluding remarks as that the winding down of the Orange Order by the 1880s meant that "in the economic sphere at least class had triumphed over ethnicity." (123) Nothing in the preceding discussion had shown that ethnicity had ever spilled over into the "economic sphere," and the two paragraphs leading up to the conclusion say just that — the Orange-Green conflict existed "mainly in the realm of politics," and no examples are to be found "of ethnic or religious riot at the work place...." Because it is so transparent, this interpretive overreaching is on the whole harmless. The careful reader will separate the fool's gold from the real thing in the book. But Kealey had paid a price for his determination to assert bigger meanings than his findings will bear.

On p. 217, Kealey remarks on the extent to which Toronto labour leaders made political careers for themselves. He gives a number of examples, and concludes on this opaque note: "This is only to elaborate on a few of the socio-economic ramifications of partyism for Toronto working-class leaders." (217) In his zeal to say things about Toronto labour politics beyond what his evidence will sustain (such as, in the preceding paragraph, "that these political struggles were dialectically [?] intertwined with the economic activities and workplace struggles of the trade-union movement" [216]), he fails to explore a theme that actually was within his range, namely, the impact of trade-union careerism on the character of Toronto's labour politics. One senses, indeed, a series of missed opportunities in the lengthy political narrative that occupies over half the text. Almost from the outset, Toronto labour seemed to have developed, for all the ideological and institutional connections, along different political lines than the labour movement across the border. In Toronto labour was a palpable constituency (although divided ethnically between Orange Toryism and Irish-Catholic Liberalism) resistant to the kind of absorption into the two major parties that one sees in the United States. Now the explanation for that phenomenon is a task that Kealey might profitably have set for himself.

At the start of this review, I remarked that this book shows the influence of Herbert Gutman. In one crucial way, unfortunately, it does not. In all his writings, Gutman invariably has drawn his ideas out from the evidence he has amassed. This is true even of so grand a piece as "Work, Culture and Society in Industrializing America." (1973) One may disagree with the argument, but one cannot fail to see how it arises out of well-marshalled and abundant evidence. Had he adhered to the prudent craft of his mentor, Kealey might have written a powerful book. Having overreached himself, he has produced only a useable one.

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Paul Craven, *'An Impartial Umpire': Industrial Relations and the Canadian State, 1900-1911* (Toronto: University of Toronto Press 1980).

WHILE ENTIRE CITIES, industries, and regions languish in historical oblivion in Canada, the Mackenzie King industry produces book after book. The discriminating shopper may select from a wide assortment of styles tailor-made for every budget: from the bargain-basement popular biography, to the more substantial collection of essays, or the rather belated attempt at psychoanalysis, and all the way up to the deluxe (if microfiche) edition of the famous Diaries, the ideal present for the masochist who has everything. Actually, one should not speak so lightly: I have no doubt that at this very moment departments of history and political science are being rent asunder by bitter theoretical debates over the relative merits of the young King versus the mature King, the romantic King versus the scientific King. . . . Poor King, who never won much acclaim or admiration in his lifetime, is now the object of fascinated academic attention: he bestrides our past like a pudgy Presbyterian colossus. Do we really need a further volume, of no less than 386 pages, devoted to King's "social theory" and his "philosophy" of industrial relations?

Of course we do, because no one doing twentieth-century Canadian labour history can avoid King. Certainly anybody who wants to understand the early Canadian labour movement should read this thorough and scholarly study, based on the author's 1978 doctoral dissertation. This is an important, well-researched, innovative book. It is in fact too important to be consigned to the amiable commendations of the standard review. Since I think the fundamental theses of *'Impartial Umpire'* are invalid, I shall first summarize what I think Craven is saying, and then try to show why this very important empirical and theoretical work inspires respect but not agreement.

Craven's study is divided into two parts. In the first, he looks at the background of industrial relations policy; in the second, he discusses the policy in practice. Craven explains that he is blending three distinct approaches: intellectual history, fairly orthodox Marxist theory (in which state policy has a brokerage function in the struggle between labour and capital), and neo-Marxist theories of the state, which stress the tensions existing between the "legitimation" and "accumulation" functions of the state and its relative autonomy from the economic base. This "methodological complexity" means that Craven has had to simplify things a bit by sacrificing some of the detail. His most serious simplification consists of relying on the Trades and Labour Congress (TLC) and the Canadian Manufacturers' Association (CMA) as representatives of organized labour and capital respectively.

The first section (Chapters 1-6) examines the preconditions of Canadian labour policy. Craven's King is very much the intellectual, a man who absorbed complex economic theories and tried to use them in his practical activity. King's intellectual formation took place at a time of immense upheaval in the study of economics; his allegiance was claimed by the historical

school which competed with marginalism for the position vacated by the Ricardians. Craven sees King as a genuine sympathizer with the aims of the labour movement. The young King saw himself as part of an intellectual vanguard of reform, a trained social scientist who could bring his special knowledge to bear on working-class problems and do something about them. There are times that Craven seems to be hammering on an open door (does anyone deny that King was influenced by the crisis in classical economy or by Ashley and Mavor?) and others when he seems to be wandering down rather obscure corridors (who cares whether King wrestled with the wages-fund doctrine at Toronto or Harvard?), but most of the time he is giving us an admirable guide to the theoretical debates of the day and their impact upon King. Craven examines *Industry and Humanity* (1918) in detail, and argues that it illuminates much of King's earlier activities. King, he says, managed to introduce compulsion into his liberal universe through the dual notion of community that he used in the book. Throughout his life King had a vocation to help the working class, even if his ideas about this task were woolly.[1]

These ideas were developed by King in a context of rapid economic and social change. It would be a mistake, however, to look at Canada in the early twentieth century in "simple class-conflict terms." (113) What set Canada apart was the continuing staples orientation of her economy. Since this entailed a "historic vulnerability," the state, the working class, and the capitalists each had to develop new positions at variance with *laissez-faire*. This is the key to the understanding of why capital and the workers welcomed state intervention, and also helps us see why the TLC implicitly rejected narrow craft unionism on the American model — although craft narrowness did entail a weak labour response to scientific management, another leading innovation in the economy during this period.

Craven follows this discussion of the "structural, institutional and ideological preconditions for the emergence and reception of the new industrial relations policy" (9) with a detailed account of what the policy meant and how it worked. King's role in labour disputes, Craven shows, was not that of the passive conciliator who sat at the bargaining table and helped the contending parties, but that of the mediator, a go-between relatively uninterested in getting the parties together, and occasionally feeding them misleading reports of each other's positions. Throughout his practical career as Canada's leading labour diplomat, King was faithful to the insights into trade unionism he developed in university. There were two kinds of unions: responsible, large, bureaucratic, and "legitimate" unions which had a common interest with employers in success for the business; and irresponsible, socialist-led, militant, and "illegitimate" unions which acted on the assumption of a permanent hostility to the employer. Only legitimate unions won King's favour, and even these found that King was relatively uninterested in their survival or effectiveness in bargaining. Although unions like the United Mine Workers were well within King's boundaries of legitimacy, they did not find him able to give them security — although paradoxically the UMW seemed to accept King's interventions with some satisfaction. King's definitions of proper trade unionism had an effect on determining which unions did not prosper, but did not guarantee that trade unionism anywhere would be accepted. Craven notes that King enjoyed "the favour of the official voice of the labour movement," and suggests that much of his policy, however reactionary it may appear in hindsight, was "the belated if usually partial realization of demands that the labour movement had been making for some time." (236) This is a drastic

[1] Craven has (wisely, I think) dropped the depiction of King as a "non-revolutionary socialist" that he used in his critique of Whitaker's work. See Paul Craven, "King and Context: A Reply to Whitaker," *Labour/Le Travailleur*, 4 (1979), 179.

revision of the evaluation of King's career as deputy minister of labour put forward by the muck-raking team of Ferns and Ostry.

Only in the ninth chapter do we get to the heart of the matter: the Industrial Disputes Investigation Act of 1907. First aimed solely at the coal mines, the Act was broadened to cover railways and other modes of transportation, and public utilities. The IDIA provided for the investigation of disputes by a tripartite board, consisting of one representative nominated by each contending party, and a chairman chosen by these two or, failing that, by the government. No strike or lock-out could take place until the board's report had been handed down; this report, while not binding upon the parties, could be made public if the parties did not accept it as a basis for agreement. Labour was initially impressed with the Act, with the exception of the running trades. It turned against it in 1911, says Craven (others put the date several years earlier), because of unfavourable legal judgments which made the Act more anti-labour than the federal government had wanted it to be. Craven considers two contending interpretations of the Act. The first, put forth by H.D. Woods and C.B. Williams, holds that the IDIA was a precursor of the modern system of collective bargaining and compulsory recognition (which came into full effect with Canadian acceptance of the principle of the Wagner Act); the second, held by Stuart Jamieson, suggests that the IDIA cannot be construed as an aid to modern collective bargaining because it took away workers' freedoms but imposed few restraints on employers. Although Craven does not seem to reach a conclusion about this debate, it seems that he objects to both positions on the grounds that they present "mature collective bargaining" with minimal direct state involvement as the fore-ordained end of labour policy. However, he devotes most of his energy to refuting Jamieson's contention that the IDIA was a hindrance to labour by noting that unions like the United Mine Workers of America were able to extend their organization to cover most Canadian workers in their industry during the period of the Act. With a host of qualifications and reservations, Craven seems to argue that the IDIA was beneficial for the unions most directly affected by it, and that had it not been for misguided judicial interpretations of the Act, the labour movement might well have not changed its mind about the efficacy of state involvement. Surprisingly, Craven thinks that the IDIA, notwithstanding its compulsory features, represented "a contraction in the range of the state's involvement in industrial relations." (370) What he means is that the IDIA hive off part of large-scale industry for special treatment, and that the federal government's activities correspondingly narrowed to that sector. King, in fact, was so suspicious of the state that he missed valuable opportunities to enlighten business. He was misreading the situation: he thought that employers would accept "responsible" unionism but his liberal philosophy provided him with no way of making this acceptance more likely. Had King not been so suspicious of state intervention, he might have laid more than just the elementary bases of Canadian labour policy.

This is a compressed version of a dense and difficult argument. Either because he is interested in large theoretical questions or because he has exposed himself to nearly lethal levels of King's sophistries, Craven writes in a prolix and opaque style. Weighty theoretical terms (such as the Althusserian "overdetermined") make their appearance unannounced and unexplained. Craven's "methodological complexity" itself makes this a difficult book to understand, since this allows him to present three possible explanations of changes in labour policy without giving us any idea which one he prefers. Was it primarily caused by changes in the economy? By changes in intellectual life? Craven does not say. It leaves one with the effect of hearing three separate papers at a colloquy of the deaf, followed by a long narra-

tive film. This approach seems to combine the faults of both neo-Marxist and traditional historiography: we have all the difficulties of sociological style without any notable advance on the methods of the "life and times" school of narrative historiography. Is this diffuse eclecticism that much more penetrating than the average historical account, with its randomly interacting "factors"?

But if this summary of Craven is fairly correct, I think I can show that his book suffers from extremely serious flaws. I shall quickly make five objections to Craven's position, in ascending order of importance.

Easily the most accomplished and interesting sections of this book are those which deal with the intellectual formation of Mackenzie King. However, even in comparison with the much shorter treatment accorded the subject by Whitaker, this account seems narrow and pedantic.[2] This is an almost completely theoreticist interpretation of King: he seems to resemble a disembodied brain, roused to life only by the lectures of his professors. Craven seems to imply that King's outlook was almost created by his university education, forgetting that most professors, even when they are exposing the fallacies of Ricardian economics, have a limited impact on their students. This would be more forgivable, had Craven not announced that he was considering "a profound sociological problem" (18) and wanted to look at the links between ideas and society.

It seems strange to devote all this attention to King's absorption of economic doctrines and so little to his lifelong devotion to the crudest form of Positivist Sociology. Even though King came to dislike the Fabians, and certainly diverged from their vision of state socialism, the parallels between his thought and that of the Webbs are striking. Like the Webbs, King extolled the large, steady, bureaucratic unions, shrewdly appraised the conservatizing effects of large defence funds and the growth of the labour bureaucracy, and praised the large-scale organization of industry. Both King and the Webbs thought in terms that equated industrial problems with the arrangement of government. Most crucially, both positions were heavily imbued with the spirit of positivism: the Webbs in *Industrial Democracy*, which was an overwhelming and brilliant study of trade unionism, and King in *Industry and Humanity*, which was not. What else was in the *Labour Gazette* if not the Positivist faith in objective data, a reassuringly solid measurement which could turn the workers' unpredictable activities into graphs? What better way to look at the social limits of objectivism than the debates between the Department of Labour and strikers over when a strike should be struck off the books? Craven's interpretation of King ideology has real merits, but it would have benefitted from a more ample consideration of the positivist tradition — which, after all, is reflected in the emphasis King placed on impartial investigation.

I find Craven's portrayal of the economy a very difficult one to accept. A minor problem is the emphasis he places on scientific management — the one occasion on which he ventures out into the real world of working-class life — does not help explain the genesis of federal labour policy. The IDIA was aimed at those areas of the economy least affected by scientific management — whose impact anyway has been seriously inflated. But this objection is not the most important one.

What I understand to be Craven's economic argument proceeds in five steps: (1) The Canadian economy in the early twentieth century remained staple-based, and therefore highly vulnerable to external economic forces. (2) With the shift from a British to an American metropolis the integrity of the national market came under attack, particularly the infrastructure of transcontinental transportation. (3) This transportation sector was historically put

[2] Reginald Whitaker, "The Liberal Corporatist Ideas of Mackenzie King," *Labour/Le Travailleur*, 2 (1977), 137-169.

in place by the state and protected by it. (4) Consequently, when the state came to intervene in industrial relations, "it was to the infrastructure that most of the original initiatives were directed." (91) (5) Also as a result of the peculiarities of the staple economy, workers viewed state intervention more favourably than their American counterparts.

Now, the jury is still out on the question of the long-term compatability of the staples tradition and historical materialism. If some labour historians have made too simple and dogmatic a response to the work of Innis, surely Craven here is guilty of the opposite sin of uncritical acceptance. It seems strange to have the National Policy touted as a distinctively Canadian device (not to mention the alleged "codicils" of union legalization) when in fact many industrializing countries adopted high tariffs in the nineteenth century. Surely Craven is simply misguided in dismissing the merger movement as an ephemeral phenomenon involving bits of paper and not production — as though the interpenetration of banks and industry were not a crucial factor in the period just before World War I. Few economic historians would go along with so unreconstructed a staple interpretation of the early twentieth century. And finally, how well-founded is Craven's insistence that the Canadian working class was distinctive in its attitudes to the state because of the staples economy? How does this stand up if we compare it to the experience of workers elsewhere — such as the British workers and their extensive series of factory reforms? As to the IDIA and the transportation sector, *reductio ad absurdum* surely makes the point: if Canada had had a fully industrialized economy in which staples production for metropolitan markets was unheard of, would the state have regarded labour unrest on the railways with benevolent neutrality?

A third set of problems concerns the treatment of class. Craven has already anticipated this criticism of his procedure by noting that what he has done amounts to "reification," but he defends the principle of focussing only on the TLC and the CMA by claiming that to do otherwise would entail an endless accumulation of detail. This seems to be a reasonable defence: no one can criticize Craven for not integrating material on working-class communities into his analysis, when such material is only just being compiled. But in fact this defence does not hold. Craven could have retained his pristine institutional approach by examining the records of a few more unions and trades councils and regional bodies. In truth, he *had* to do this if his central theses were going to hold. Everything he writes about the tariff and the working class would be subject to the most serious and penetrating criticisms east and west of the Ontario border. The stock response to all this is to say that such additional details from the periphery would qualify but not change the essential argument. But for Canadian labour the periphery of the economy is the heartland of the class. It was against this heartland, and most especially the coalfields, that federal labour policy was aimed. Craven's focus on a few Ontario labour bureaucrats is wrong, not on the basis of some querulous demand for equal regional representation, but because it misconstrues the object of his research and consequently produces a false resolution of a badly posed question.

A related objection is that Craven, by relying so much on the "official voice" of the labour movement, credulously assumes that this "voice" spoke for "labour." This ironically comes close to accepting King's own definitions of the labour movement. When he comes to discuss the IDIA, for example, Craven never considers whether such men as Frank Sherman of the UMW might not have felt a bit flattered to be thought "labour statesmen" and even pursued contacts with King, encouraged by the lively expectation of a tangible reward. Again it would have been appropriate to look at a far wider

range of unions and industries — empirical work which would have weighed far more heavily than pounds and pounds of "methodological complexity."

Craven's approach is highly misleading when he discusses the IDIA. Craven does establish a number of new and important arguments here — particularly about the doctrines of fairness which had so big an impact on workers — which deserve to become permanent parts of labour historiography. But such arguments are a breath of realism in a parched theoreticist wasteland. Craven reconstructs the IDIA as a theoretical gesture; he never tries any actual assessment of its real impact on various industries. His argument that the IDIA represented a *contraction* of federal involvement in industrial relations would never have survived such inquiry. By any measurement, the IDIA was a massive *extension* of the power of the state in the most important sectors in the economy. Surely Craven is merely being provocative when he fails to see that there is all the difference in the world between the *ad hoc* interventions of the boy minister flitting from one adventure to another, and the *routine* involvement of conciliation boards after 1907? How is it that one can tell the story of collective bargaining in Maritime coalfields after 1907 as the story of one conciliation board after another after another? And Craven's mistaken view of the IDIA is compounded by the simplistic and formalist insistence on only looking at the letter of the law. Craven never discusses the emergence of contracts themselves — except in passing, as when he notes that King foisted an unfavourable one on the miners in Rossland or that the majority of miners in Nova Scotia in 1909 were working under agreements brought about by IDIA boards. Craven does not even note the provision whereby a report under the IDIA could be made a binding Rule of Court.

Because he misses these essential features, Craven's assessment of the IDIA and his "resolution" of the debate between its admirers and detractors are inconclusive. Craven is surely right to object to the teleological undertone of much of the writing on this subject. Workers did not suffer cruel wrongs and countless deprivations until some point in the 1940s when they suddenly emerged into the bright future as certified bargaining units. But his own alternative seems to be that of even more state intervention. The real resolution of this debate lies in the refusal of the terms in which both sides have asked the question. Did the IDIA assist "modern collective bargaining?" Certainly: it made "responsible" unionism more secure and radical unionism more vulnerable, it deprived thousands of workers of the possibility of spontaneous self-activity, it provided a powerful ideological impetus for the growth of a labour bureaucracy secure from the importunities of the rank-and-file. All these modern aspects of "collective bargaining" can be traced back, in our major industries, to the early twentieth century. To the extent that "collective bargaining" functions to consolidate the hold of capital, King surely deserves a large part of the credit; to the extent that it still, after everything that has occurred, occasionally defends the rights of workers, shows that even the best-laid schemes of philosopher Kings go awry.

There is a final point, and the most serious one of all. Craven tells us half the story of state labour policy. The other half of Canadian labour policy was made by the militia. Craven seems to have forgotten — almost — that most of the story of "Industrial Relations and the Canadian State 1900-1911" can be understood only in terms of actual or potential violence. When Craven recognizes the huge role played by state violence in these years, it is with an almost audible yawn: "In some respects, the dispute on the Grand Trunk railway system in the summer of 1910 was a very ordinary strike. It had its quota of riot scenes and organized strikebreaking, suspected sabotage and military intervention,

but in this it was little different from any of a score of other industrial disputes in those years." (318-319) This rather jaded dismissal of state violence stems from Craven's earlier theoretical decision to exclude coercion as an essential constitutive function of the state. There must surely be more room, however, for a more traditional view of the state and its monopoly over the legitimate access to violence; and coercion, actual or implied, was present in the IDIA, in *Industry and Humanity*, and (most importantly) in the streets. Under the IDIA union organizers could be put in jail for feeding striking workers who had not conformed to the Act; and Craven's interpretation of this aspect of the IDIA as a mistaken judicial interpretation seems to overlook the portions of the IDIA which justified this procedure and which endured long past their judicial "misinterpretation." How can one write the history of industrial relations and the Canadian State and not once refer to the fact that from 1895 to 1904 the militia were called out 11 times (71 days) and no less than 17 times (1232 days) from 1905 to 1914?[3] How can one reconstruct the attitudes of the labour movement towards the state, without at least once acknowledging that the alternative to the acceptance of state interference was violence? One might almost think that Mackenzie King has pulled the wool over the eyes of his most sophisticated interpreter.

This failing stems from the ethereal quality of the book. Only if the IDIA and King are seen primarily as artifacts of intellectual history can we forget the actual conduct of industrial relations in this way. The account never seems to touch ground; in comparison with Keith Burgess's study of British industrial relations, we never find out what impact any of these ideas or policies had on actual industries or people.[4] At his best Craven has shown how the IDIA was supposed to work, but what we need to know is how it actually changed things, at the bargaining table and on the picket lines. Craven remarks in *'Impartial Umpire'* that writing about Canadian labour policy without Mackenzie King would be like mounting a production of *Hamlet* without the prince. But he himself has given us *Macbeth:* as a long disquisition about modes of succession in feudal societies, but without the knife.

Yet it will not do to end without recalling the positive features of the book. Craven deserves to be commended for introducing new concepts and questions to the study of Canadian labour history. He has carefully analyzed the intellectual formation of King and has decoded, in new and crucial ways, *Industry and Humanity*. This is important reading for historians and militants. Canadian workers now confront a heavy burden of state control and bureaucratization; the intellectual origins of this situation deserve careful study. By beginning debate on the ideas of King, Craven has implicitly raised a host of questions about alternative positions. His work deserves, and doubtless will receive, careful consideration.

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Michiel Horn, *The League for Social Reconstruction: Intellectual Origins of the Democratic Left in Canada 1930-1942* (Toronto: University of Toronto Press 1980).

ON THE INTELLECTUAL Left the reputation of the League for Social Reconstruction has of late not fared particularly well. Reg Whitaker clearly includes the LSR when he criticizes the early CCF's proclivity for economic regulation, social engineering, planning, and public ownership. According

[3] Desmond Morton, "Aid to the Civil Power: The Canadian Militia in Support of Social Order, 1867-1914," in Michiel Horn and Ronald Sabourin, eds., *Studies in Canadian Social History* (Toronto 1974), 429.

[4] Keith Burgess, *The Origins of British Industrial Relations: The Nineteenth Century Experience* (London 1975).

to this social democratic image of the state, far from being socialist the LSR was an exponent of a state capitalism that was arrogantly centralist and technocratic. Alvin Finkel in *Business and Social Reform in the Thirties* levels similar charges but is also more personally pointed in his criticism. The LSR, he says, was irremediably infected by the class origins of its members. Mainly they were middle class do-gooders with an aristocratic mien who insisted on intruding their "genteel hands and bleeding hearts" into a radical political process no doubt requiring much more horny-handed qualities. Michiel Horn's finely written and meticulously researched history of the LSR will correct many of these conceptions as it will confirm some others. So complete and balanced are his claims that future historians will have to argue most capably if they wish to disagree with him.

Horn's book is both an account of the fate of the LSR as an organization and a perceptive analysis of its ideas, particularly those contained in the major intellectual product of the League, *Social Planning for Canada* (1935). The League was hatched in the fertile imaginations of Frank Underhill and Frank Scott in August 1931. Its official beginning was in Toronto in January 1932. Among its most gifted, early members were Harry Cassidy, Eric Havelock, J.F. Parkinson, Irene Biss, Graham Spry, King Gordon, Eugene Forsey, and David Lewis. Founded at the height of the Depression, the LSR conceived its purpose as the educating of mainly middle class opinion to appreciate the desirability of a wholly new economic and social order, one in which the satisfying of basic material needs would replace the imperative of private profit. The League's distinctive contribution to democratic socialism in the 1930s was its minute elaboration of the procedures of public planning in the new society. Hence arose its reputation for elitism, social engineering, and technocracy, a reputation that Horn argues was not altogether undeserved. Of course the League was characterized by a number of other intellectual positions: an insensitivity towards Quebec nationalism and regionalism in general, an exceedingly centralist account of Canadian federalism, a disdain for the League of Nations as a league of capitalist powers, and correlatively support for a highly neutralist and isolationist view of Canada's role in the world.

Horn argues that the LSR, perhaps because of its overblown optimism regarding the rationality of the middle classes, never quite fulfilled its self-defined mandate as the educators of a democratic mass movement. Branches of the League came and went with alarming rapidity, and only in Toronto and Montreal did it have any significant membership. Finally it began to founder with the outbreak of World War II. Horn concludes that once Canada decided to fight alongside Britain the LSR's viability was badly compromised by its unconcern for the British connection and its neutralism. The LSR was finally given an honourable burial in 1942, with most of its remaining members more or less content to throw in their lot with the CCF.

All of the LSR's members were, according to Horn, intellectuals: university professors, teachers, social workers, students, and clergymen. Using the theory of the Belgian socialist, Hendrik de Man, author of *The Psychology of Socialism* (1928), Horn provides a suggestive account of the social origins of the radicalism of the LSR. Those that deal with the world of ideas view their vocation as one requiring intellectual freedom and occupational integrity, features of their life-world that are undermined by late capitalism:

> De Man cites dislike of attempts in industrial enterprises to bureaucratize and specialize the functions of the 'brainworker', the dismay that a professional man or artist may feel in view of the indignity involved in having to sell his skill or talent, 'usually to persons incapable of understanding its true worth', and the sense that administrative personnel may have that their desire to serve the public is being subordinated

to the wishes 'of others whose sole interest is acquisitive'. (32-3)

What prevented the members of the LSR from being lonely, perhaps anarchic victims of such a fate was their almost universal experience of Christianity in their early years. Many of them were either clergymen like King Gordon or sons of the manse like Forsey, Scott, and Escott Reid. From this background they were able to develop a sense of social sympathy that generalized from their individual predicament and led them to socialism.

Horn provides I think a plausible argument for the view that the LSR did expound an assuredly socialist critique of society. Their view of small businessmen and farmers may, admittedly, be seen as out of step with much socialist theory, Marxist and otherwise, but if one refuses to see Marxism as the sole bearer of socialist truth, and if one defines socialism as a commitment to planning, equality, public ownership, and fraternity, the LSR had as much right to the title of socialism as others. One misconception that Horn does lay firmly to rest is the notion that the LSR held that every undifferentiated measure of statism was one step further on the road to socialism. In fact it recognized very clearly that such policies as Bennett's New Deal were all too likely to stabilize and reinforce the dominance of monopoly capital and thus had to be rejected by socialists.

Because this was the general character of the LSR's world view, Horn's conclusion is predictably that the League largely failed. It did not convert many to the goal of the cooperative commonwealth. And with the introduction of Keynesianism and the welfare state after 1945, the Canadian state discovered a new means of legitimizing the continuance of a capitalist economy. What successes it did obtain were small, although not altogether historically inconsequential: a legitimizing of the role of the intellectual in politics, the establishment of academic freedom in universities, the popularizing of economic intervention and the welfare state, and the saving of the *Canadian Forum*. Finally, whatever success the CCF had in Canadian politics was in some part due to the LSR. Horn establishes that the Regina Manifesto was derived more or less directly from the LSR's original 1932 manifesto and thereafter the likes of Underhill, Lewis, Scott, and Forsey were very significant in the CCF's inner circles.

One point of criticism I would make of Horn's work. His monograph is sub-titled, "intellectual origins of the democratic left in Canada, 1930-1942." The impression might easily be taken from this that the LSR was mainly if not solely responsible for the development of democratic socialist ideas in Canada either before, during or after the 1930s. This is of course misleading. Horn himself admits that the basic form of the LSR's ideas were already extant before it was even formed. There is I think a veiled central Canadian and intellectualist bias, rather similar to the LSR's for that matter, in Horn's account of the intellectual origins of the social idea in Canada. He seems to believe that apart from some weak excrescences of Marxism and pockets of social gospel thought in the Methodist and United churches there was really not much socialist theory in Canada before the founding of the LSR. But there was, in urban areas in British Columbia, Alberta, Manitoba, and the Maritimes. Such thinking may not have been conceptualized by individuals pursuing the specialized role of intellectual, but we cannot overlook the profound, honest-to-goodness thinking of urban workers before 1930. They may not have produced a single work of coherent socialist doctrine to equal *Social Planning for Canada* but that is not to say that they did not produce a myriad of tracts, pamphlets, and speeches that in their own way were as intellectually credible and influential as anything produced by the LSR. The formulating of socialism in Canada has not been the sole preserve of intellectuals.

In any event, this is a fine book, thor-

ough in its scholarship, elegantly written, and balanced in its judgments.

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Gregory Baum, *Catholics and Canadian Socialism: Political Thought in the Thirties and Forties* (Toronto: James Lorimer 1980)

AT LAST Canadian Roman Catholics, not to mention the rest of us, have a substantial book at hand which attempts to recover a largely lost history of radical Canadian Catholic social criticism and social action. Gregory Baum, its author, needs no introduction as undoubtedly the best-known Canadian interpreter of the new currents pulsing through the church since the Vatican Councils of the mid-1960s.

The book follows hard upon the heels of Baum's recent book, *The Social Imperative of the Gospel*, and takes up the challenge to abandon the age-old tactic of framing idealistic blueprints for a Catholic social order in favour of the discovery in local conditions of social struggle the terms and structures in which a Catholic (or Christian) response to social disorder and injustice may be made. Baum therefore undertakes to rehabilitate for Catholics the central tradition of Canadian socialism, subjecting it and papal social teaching alike to a searching analysis, before passing on to a revealing scrutiny of the reasons for the official rejection by Canadian bishops of the CCF, Canada's social democratic party, in the 1930s. We learn of little known local Catholic papers sympathetic to reform, like the *Prairie Messenger* in Saskatchewan, and the efforts of editors of the *Beacon* in Montreal and the *Catholic Register* in Toronto to cast the CCF party in a favourable light. Catholics long forgotten by most — E.J. Garland of Alberta, Joe Burton and Eugene Cullinane in Saskatchewan, Henry Somerville in Toronto, Murray Ballantyne in Montreal, Clarence Gillis in Cape Breton — are restored to memory as men who refused to accept their bishops' pronouncements that papal social teaching forbade Catholic participation in Canadian labour parties or democratic socialist politics. These figures were a distinct minority in the church, but Baum argues convincingly that they and not the forbidding bishops were right.

Alongside these dissenters, were Quebec Catholics like Esdras Minville and Paul Gouin, concerned to formulate a broadly reformist programme for the depression-stricken province, and the better known efforts of the Antigonish Co-operative and adult education movement among fishermen and farmers of Nova Scotia. Baum's careful analysis of both developments lays bare the underlying ironies marking Canadian Catholic social thought and action at the time. The haphazard efforts of the École Sociale Populaire in Quebec to document a case against the CCF produced a document, *Pour la restauration sociale au Canada*. That document, in turn, however, provided the basis for Gouin and his colleagues to formulate a programme of considerable economic radicalism — a "purified version of the CCF" for Quebec, as Baum puts it, which had a notable success in its first electoral trial. And it was among conservative, Scottish Catholic settlements in Nova Scotia that Moses Coady, promulgating social teachings well in advance of the social thought of the church anywhere prior to World War II, galvanized his fisher and farmer listeners into a formidable social and economic movement.

Unfortunately, not all the figures in the book — which include a fascinating anarchistic communitarian, Catherine de Hueck — are as susceptible as Coady is to substantial analysis of their social thought. Not surprisingly in so preliminary a venture, the results are uneven, somewhat episodic, and occasionally Baum's hand is heavily didactic; but the treatment is always fair, and the commentary illuminating, drawing as it does on his broad knowl-

edge of social theory and Catholic social and religious thought. The Canadian hierarchy in the 1930s, he concludes, led its people into a land of political illusion from which they have only recently begun to escape thanks to Christian-Marxist dialogues and liberation theologies in the second and third worlds. But these are exotic imports and no substitute for home grown traditions. More recently the Canadian Catholic bishops have opened up the social and political options available to Canadian Catholics. But Baum has effectively cleared a Canadian space on which Catholic social activists on the left can stand without compromise; he has added notably to that roster of Canadians for whom the claims of social justice stood first; and he has broken the broadly Protestant stranglehold in the annals of Christian social reformism in English Canada. On all these counts the book can be widely commended.

Richard Allen
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Leopold Infeld, *Why I Left Canada: Reflections on Science and Politics*. Translated by Helen Infeld. Edited with Introduction and Notes by Lewis Pyenson (Montreal: McGill-Queen's University Press 1978).

THE SHADE OF Leopold Infeld does not haunt the University of Toronto and the Canadian scientific community, to which he belonged for 12 years, 1938-50. Perhaps it should. Perhaps a stimulus to renewing the sense of his life may come from this attractive book, with Infeld in mid-sentence gesturing to us from its frontispiece.

These are essays Infeld wrote for a general audience in Poland, during the 1960s, after his return there. They have been translated by his widow, Helen Infeld, and carefully edited for the Canadian reader by Lewis Pyenson, who supplies an introduction which briefly but most perceptively fills out their context. What was Leopold Infeld's life in Canada, and what can we learn from this view of it?

He was a conspicuous figure on the Canadian scene. Not so much as a leading mathematical physicist — for his scientific work had more resonance in Princeton and Dublin than in Toronto at that time. But Infeld was also (in English, his fourth language) one of the supreme explainers of science to the non-scientist. During his years at Toronto appeared his joint book with Albert Einstein *The Evolution of Physics*, which may not have sold more than any other non-technical account of modern physics but has likely taught more; his unusually contemplative autobiography *Quest*; and *Whom the Gods Love*, his extraordinary biography of the 19th-century mathematician and revolutionary Evariste Galois.

Another respect in which Infeld stood out was politics. As co-founder with Barker Fairley during World War II of the Canadian-Soviet Friendship Society, later as one of two professors — the other was Fairley — to work for the defence of those accused as spies by Gouzenko, and still later, as a prominent public speaker debunking the synthetic "secret of the atomic bomb" scare, Infeld fit the niche of the radical professor. But did that niche exist then? If it did, it was more comfortably occupied by someone like Barker Fairley, with his long presence in the Canadian cultural world and his deep involvement in specifically Canadian social movements. Leopold Infeld, who came to Toronto after forty years spent in Kraków, Lwów, Cambridge, and Princeton, was a sitting target for the Red-hunt.

Post-war official Poland was eager to build a scientific research effort where the pre-war government (as Infeld remembered well) had offered almost no support; it was natural that Infeld should be invited to assist Polish physics, and ultimately to organize and direct the first institute of theoretical physics in that country. Post-war official North America was center-

ing its international policy on anti-communism and nuclear weapons; this could not help making Toronto feel less comfortable to Infeld than it had at first. There were both a pull and a push impelling him to leave Canada, and he describes both with cool hindsight. It was curious how the push and the pull conspired. Infeld himself, willing though he was by 1949 to revisit Poland and to counsel the few mathematical physicists there, fully intended to remain a Toronto professor the rest of his life. But his plan to spend 1950-51 on leave in Warsaw drew a strident right-wing attack in the Canadian press. (After all, he might have told the Reds that E equals mc^2.) President Sidney Smith and Dean Samuel Beatty of the University put strong pressure on him not to make the visit. As a last resort, they cancelled his leave: he was suddenly obliged to choose one country or the other. Slightly panicked, and feeling perhaps more bitterness of rejection than he displays in this retelling, he chose Poland, and wrote Dean Beatty from Warsaw resigning the Toronto position. Emboldened by his departure (and confirmed: the immigrant shows his basic unreliability, you see?), the Canadian right passed on to higher levels of obloquy and harassment, going to the length of revoking the citizenship of the two Canadian-born Infeld children.

Now an institution like the Department of Mathematics at the University of Toronto has some real existence through time (not that an institution lives independently of social reality, but that it *is* part of social reality). I joined the Department twelve years after Infeld left it. I work in a new building, and Sidney Smith and Samuel Beatty never administered me. Still, I have colleagues and friends who were in the Department throughout Infeld's years here, and one who was his thesis student. There is enough link that I can rightfully claim Leopold Infeld as part of my tradition, and I gladly do. Yet the institution in which he lived, and to which he contributed, is one from which he became an outcast. What is its memory of the crisis?

G. deB. Robinson, the professor who best spans this whole period, has written a history of the Department (published by the Department, 1979). In it he reprints the obituary he wrote for the Proceedings of the Royal Society of Canada following Infeld's death in 1968. Duly parading Infeld's academic honours, and recalling his kindness and help to students and colleagues, this ungrudgingly appreciative and friendly essay ends its short passage on his last year in Toronto with the conclusion that "we who knew him considered his involvement in the politics of the day unfortunate but inevitable and the published attacks on him without foundation." Unfortunate that "He did what he could to fight Naziism?" Unfortunate that "he struggled against the evils of our day?" Unfortunate that "Together with other scientists he signed the Einstein-Russell appeal that gave birth to the Pugwash Movement for peace and against the armament race?" Or unfortunate that he was attacked by sections of the press and finally by the Government? Though the memory of Infeld's presence is preserved and valued, still the memory of his departure is not fully faced. There is more to be learned about how it happened and what it means. Infeld's reminiscences, gentle and anecdotal though they are, will serve well as a starting point.

This review till now has gone along with the somewhat unjust impression given of Infeld's book by its title. The subtitle is more accurate. These essays are not all about Canada and the Red-hunt, but about many other things as well: memories of his life as teacher in a Jewish secondary school near Kraków, his friendship with Einstein, what it was like to return to Poland after the war, and more. Always the calm but not dispassionate observer, his opinions often give one to think. One is interested in them not just as opinions, but (since one knows him rather well, through his writing) as *his*

opinions. Especially striking perhaps is this crisp statement: "Had I had any idea what was really happening during the Stalin period, which I learned only after the Twentieth Congress, I would not have gone to Poland." He is not addressing us in Canada. I wish I knew what more he would have had to say to us on this subject. To his Polish readers, who had their own first-hand knowledge, he merely alludes to the major evils of Stalinism, and deals at length with only that minor tendril which stung him: the attack on his work — especially on the discussion of relativity in *The Evolution of Physics* — as philosophically "idealist" and hence anti-Marxist. His account is circumstantial as to incidents where the criticism was made, and clear and definite (though brief) as to his view of the philosophical issues. One finishes with gratitude for what has been said and full of questions which it is now too late to ask.

He writes freely about the anti-Semitism he contended with in his youth; nothing about anti-Semitism at the University of Toronto (which he must have found painful too but which is not part of this story); and only a few sentences about the brief flare-up of anti-Semitism in 1956 Poland. Again, one wishes unreasonably to continue the conversation. These are eyes through which one would wish to view the new and worse recrudescence of anti-Semitism there after 1968. Would he have re-assessed the realism of Einstein's warning to him before he left for Poland?

> I cannot help being very doubtful about the stability of conditions there [Poland]. After some time the evil men may emerge from the mouse-holes in which they are now hiding — not much different from what happened in Germany in the twenties.

The experience would not have shaken Infeld's devotion to humanity, to science, and to Poland, but his response to the challenge would have had something more to teach us.

One offers thanks for this valuable book and especially thanks to the translator, Helen Infeld. She was far the worst sufferer from their move in 1950 to a country whose language and culture she did not know. Then after his death, deprived of the status of a leading citizen's wife, she was left with no other status she could assume. In time she forged a new life as an editor and translator in Warsaw, and her unique perspective on all these events. She too has much to teach. But this is Leopold's book.

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University of Toronto

Marcel Fournier, *Communisme et anticommunisme au Québec (1920-1950)* (Montréal: Éditions coopératives Albert Saint-Martin 1979).

MARCEL FOURNIER publie une vulgarisation d'une thèse de maîtrise effectuée au département de sociologie de l'Université de Montréal en 1969. L'auteur était déjà connu pour sa participation à la revue *Possibles* et certains articles publiés dans *Recherches sociographiques*. M. Fournier avait déjà abordé le thème du Parti communiste dans un article publié dans la revue *Socialisme 69*. L'histoire du Parti communiste, section québécoise, n'avait jamais été entreprise si ce n'est d'un article assez dense dans le défunte revue *Mobilisation* (vol. 4, no. 4). Le choix du sujet constitue donc un apport nouveau dans l'historiographie du mouvement ouvrier au Québec.

Nous devons reconnaître que M. Fournier a fait un véritable travail de bénédictin en dépouillant systématiquement les rares sources et en les complétant d'interviews avec les premiers militants du parti. Nous déplorons cependant que le livre ne comporte aucune bibliographie et que les nombreuses notes de référence soient reportées à la fin du livre. Le texte est concis: 128 pages plus les annexes. Le style est clair et les extraits d'interviews ajoutent même une certaine dynamique à la lecture.

Puis qu'il s'agit d'un historique, l'auteur commence par les origines du mouvement communiste. Nous appren-

drons sans surprise que le Parti communiste nait d'une division du mouvement socialiste. Il eut été pertinent de faire ici le rapprochement avec la situation internationale en spécifiant que c'est au début des années 1920 que naissent la plupart des Partis Communistes (PC) des pays industrialisés. Bien que l'auteur mentionne que les statuts du PC canadien étaient conformes à ceux de la 3e Internationale, il n'insiste pas assez sur la *subordination* des PC nationaux envers les décisions politiques de la "métropole," Moscou. La représentation russe à l'Internationale était écrasante et la direction contrôlait, et le terme n'est pas fort, l'ensemble des orientations idéologiques et politiques. L'auteur escamote d'ailleurs l'important débat entre les éléments trotskystes et stalinistes sur la stratégie du mouvement communiste international.

Les premières années sont à la fois difficiles et empreintes d'un certain optimisme. Difficiles à cause de la censure et la répression, optimistes par le degré relativement important de mobilisation que susciteront les communistes. L'expérience la plus connue des années 1920 est certes celle de l'Université ouvrière d'Albert Saint-Martin: véritable lieu de rencontre où plus de cinq cents personnes pouvaient s'entasser des soirées de week-end pour entendre des conférences sur les sujets les plus variés. Le livre de Claude Larivière, *Albert Saint-Martin, militant d'avant-garde* (même maison d'édition) est à ce sujet plus explicite. Mais cette partie nous laisse perplexe parce que l'auteur avoue lui-même que Saint-Martin n'était pas véritablement communiste. De fait les membres canadiens-français étaient plutôt rares: une trentaine au début des années 1930, soit pourtant en pleine crise économique. On aurait pu s'interroger sur cette stagnation puisque les masses ont tendance à joindre les solutions politiques extrémistes en temps de crise.

Dans un second temps M. Fournier dresse un bilan de l'action politique des années 1930. L'étude de Jacques Rouillard "L'action politique ouvrière au début du 20e siècle" dans *Le mouvement ouvrier au Québec*, dirigé par F. Harvey et *L'action politique des ouvriers québécois*, collectif publié aux Presses de l'Université du Québec, portaient sur la période antécédente. La ligne politique est conforme aux directives de l'Internationale Communiste: stratégie d'affrontements de luttes de classes avant 1935 puis participation stratégique avec la petite bourgeoisie après cette période. Le PC, sous l'étiquette du Parti Ouvrier-Progressiste remportera, via Fred Rose, une victoire surprenante aux élections fédérales de 1943 et 1945 dans le comté montréalais de Cartier.

Nous saisissons cependant très mal les liens qui existaient entre le Parti Communiste Canadien et l'aile québécoise. Cette lacune nous semble très importante. Par ailleurs M. Fournier englobe beaucoup trop la période 1935-47 à l'époque Duplessis. Le gouvernement libéral de Godbout prit le pouvoir en 1939 et ne le perdit qu'en 1944, après avoir lui-même institué les premières balises de ce qui deviendra en 1964, le Code du Travail.

De plus, l'auteur est très avare de "la ligne à suivre" de 1939 à 1941. On connait l'existence du pacte germano-soviétique, du partage de la Pologne, de l'annexion par les armes de la Lithuanie, de la Lettonie et de l'Estonie. Quelle était la position du Parti Communiste, section québécoise, sur ces questions? Parallèlement, le PC met sur pied des comités de solidarité féminine, travaille activement à l'organisation syndicale et met l'accent sur les intérêts immédiats.

Puis 1947. Année fatale. C'est le début de la chasse aux sorcières, de l'expulsion des communistes dans les syndicats. Mais c'est en outre au Québec la scission sur la question nationale. Le débat n'est pas nouveau. Le nationalisme québécois français est-il compatible avec le nationalisme canadien anglais? Oui, dans la seule mesure où ces nationalismes seraient d'abord anti-capitalistes-américains. Mais le PC n'osait pas véritablement, malgré les

interventions de l'éminent Stanley B. Ryerson, attribuer le qualificatif de nation au Québec. Le parti se divisa. La répression monta. Il faudra attendre les années 1970 avant que certains québécois donnent une nouvelle impulsion aux idées communistes.

L'auteur termine par une postface en émettant la thèse que le PC aurait dû consolider ses assises provinciales avant de se lancer sur la scène fédérale. Comme s'il fallait que le NPD attende que le Québec le porte au pouvoir avant de conquérir une trentaine de sièges au fédéral!

le livre de M. Fournier n'a rien de comparable aux livres parus en anglais sur le PC canadien (Buck et Avakumovic). Mais il nous apparait présentement comme une source intéressante et unique sur l'historique de l'idéologie et de l'action politique et sociale de la section québécoise du Parti Communiste canadien de son origine à la scission de 1947.

Michel Pratt
CEGEP Saint-Hyacinthe

Robert Bothwell, Ian Drummond, and John English, *Canada Since 1945: Power, Politics and Provincialism* (Toronto: University of Toronto Press 1981).

NO PREVIOUS UNIVERSITY press publication, certainly not an historical survey written by an academic, has been launched with the fanfare which heralded this book. To supplement the usual notices in scholarly journals and direct mailings to college and university teachers, *Canada Since 1945* was featured in large ads on the Saturday book pages of major English-Canadian dailies. Both its promotion and its presentation make evident that the University of Toronto Press and Professors Bothwell, Drummond, and English mean this history of Canada from Gouzenko to Gzowski to reach a wider readership than their colleagues and those captive students compelled by a university course reading list. At last historians trained in the academy have donned armour and entered the lists with the Grays and Bertons to battle for the book budgets of the tiny percentage of Canadians that buys and reads serious books.

Most of those who opt for Bothwell rather than Berton should not be disappointed. *Canada Since 1945* reviews the past 35 years of Canadian history from a national vantage point and succeeds in touching most of the bases. To quote the dust jacket acclaim of Mitchell Sharp, the authors provide "a comprehensive, factual, lively and opinionated account of the transformation of Canada during the postwar period." One suspects that Sharp, a former Liberal finance minister, would agree with most of the opinions which are vigourously expressed throughout the book. Resolutely whiggish, committed to progress, the authors are without nostalgia for the vanished past. "The 'golden age' of the past," they make clear at the outset, was "a pretty tarnished piece of silver plate with a large admixture of base metal." (25) Because of the economic growth produced by our capitalist economy the "great masses" have been provided with "an escape from a life of grinding poverty." The period since the war's end "was one of almost uninterrupted economic growth whose benefits were widely shared among all levels of Canadian society and in all regions," leading to today's "unprecedented abundance of material prosperity." (457)

The subtitle of *Canada Since 1945 — Power, Politics and Provincialism —* accurately encapsulates the perspective of the authors. They have considerable respect for "power," especially when they feel it is wielded competently by the central government of the federal Liberals of the pre-Trudeau era. "Provincialism," on the other hand, is always bad, whether the word describes the behaviour of provincial governments, the attitudes of Canadians in the 1940s, or the economic nationalism of Walter Gordon. "Politics," especially national politics, is what written history

should concern itself with, and Bothwell, Drummond, and English profess the traditional trinity of Canadian historiography. Political history is the father, economic history the son, and social history a very pale ghost.

In the section titled "The Pearson Years," to provide one example, 65 pages of political and economic history are followed by six on "Nationalism and Culture," which student activism and the declining birth rate must share with Margaret Atwood and the Watkins Report. Of the 15 photographs which illustrate the text, only three are not pictures of politicians. Among the non-political personalities are John Lennon and Yoko Ono, but instead of depicting their "bed in" for world peace at Montreal's Queen Elizabeth Hotel, the authors have chosen a photo which shows them in conversation with Prime Minister Trudeau!

This concern with things political does not extend to provincial politics or to parties of the left. The only province which receives any attention is Quebec, presumably because of the importance of separatism as a national issue. The social democratic governments which came to power in Saskatchewan, Manitoba, and British Columbia are pointedly ignored. The CCF — "a minor electoral sect" — and the NDP — "the retreaded version" — are presented throughout as irrelevant to the course of Canadian affairs. T.C. Douglas and Ed Broadbent each appear but twice in the text; M.J. Coldwell is mentioned only in a sarcastic aside that Canadian socialist leaders "are so powerless that it is no fun to make them look ridiculous." (251)

The only sympathy the authors express for the NDP comes when "the forces of moderate democratic socialism" are attacked by those further left, "the Marxist-nationalist Waffle." (333) When discussing the ideological ferment of the 1960s, Bothwell, Drummond, and English temporarily abandon the witty cynicism with which they usually express their derision and resort instead to kidney-punching prose. Mel Watkins is described as a renegade who "discarded the muted business suit of the academic liberal economist" for "the open-shirted, leather-jacketed garb of a theorist of the New Left;" Robin Matthews, we are reminded, "had begun but not completed his doctoral studies." (260) The movements in which they were active are collectively described as "gaseous bubbles that resulted during the Trudeau years."

Readers of *Labour/Le Travailleur* will be especially interested in the authors' treatment of the working class and labour history. As a general category, economic class does not really exist in *Canada Since 1945*. The primary cleavage factor in Canadian society is French-English; regional division comes a distant second in importance. Labour unions, however, are given efficient if cursory treatment. The important milestones in the development of the labour movement — the vast increase in trade union membership, the CCL-TLC merger, the creation of national and provincial labour codes — can be extracted with careful use of the index. The authors' attitude to organized workers is best summed up, however, in the conclusion to chapter 2, "Economic Growth and Change." "Why has Canada done so well for more than thirty years?" they ask rhetorically. Because of "all-important efficiency, up-to-date technology applied correctly . . . despite the dislocation that a new machine or a new production process or a new arrangement may entail." (24-5) Canadian workers, the implication is blunt, had better continue to "adapt" themselves to such changes, lest Canada catch the dreaded "British disease" of low growth and stagnant or declining living standards.

External as well as domestic affairs have preoccupied post-war Canada and the authors' collective line on Canadian foreign policy may be surmised from the book's dedication to Colonel C.P. Stacey. Proud of Canada's world role, they defend NATO and NORAD and reject the "old

fashioned isolationism" of those who have been critical of the ready acceptance of participation in American-dominated military alliances. (282) Less than enthusiastic about America's adventure in Vietnam, they nonetheless stigmatize Canadian abhorrence of the war's brutality for its "derivative quality." (279) Those who oppose Canada's acquisition of nuclear weapons or who advocated disarmament were "the optimistic and the naive." (245) Bothwell, Drummond and English certainly do not wish to be counted among their number.

But to criticize the authors for their point of view and for what they leave out is a little like railing at Milton Friedman for refusing to be a Marxist economist. Although occasionally infuriating, *Canada Since 1945* is engagingly written, almost never dull, and extremely useful. It has an extensive and reasonably accurate index and a bibliography which includes Levitt's *Silent Surrender* and Laxer's *The Energy Crisis* even if it does warn the innocent that such books "can mislead." It is attractive, well-organized, and by today's standards not outrageously expensive. The book deserves to be a financial success and Bothwell, Drummond, and English deserve praise for their effort to write for a non-academic audience. More historians should join them in making that effort, if only to demonstrate that there is another version of Canadian history in addition to the one they have presented.

John Herd Thompson
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Michel Pratt, *La grève de la United Aircraft*, Collection histoire des travailleurs Québécois, no. 8 (Montréal: Les Presses de l'Université du Québec 1980).

LES ANNÉES 1970 au Québec ont été marquées de conflits ouvriers majeurs qui ont amplifié l'image d'une société québécoise en pleine mutation. L'ouvrage de Michel Pratt, *La grève de la United Aircraft*, tente d'en dévoiler un volet important.

Professeur du sciences politiques au CEGEP de Saint-Hyacinthe et chargé de cours en histoire à l'Université de Montréal et à l'UQAM, l'auteur n'hésite à montrer ses couleurs: la United Aircraft "symbolisa tout ce qu'il y a de plus méprisant non seulement envers les travailleurs de ses usines à Longueil, mais aussi face à l'image du Québec." (2) Ce "préjugé défavorable à la partie patronale" (3) s'assortit d'une charge à l'État capitaliste, et au gouvernement libéral de Robert Bourassa qui se montre antipathique à la cause des travailleurs. Pratt s'efforce d'identifier les moyens dont dispose l'État capitaliste au service des firmes transnationales, pour ainsi mettre en évidence la fragilité du mouvement ouvrier. Pour ce faire, il choisit de se pencher sur un conflit ouvrier majeur: la grève de la United Aircraft (aujourd'hui Pratt and Whitney). Le sujet se prête certes aux objectifs de l'auteur, puisqu'il représente un des événements clé du mouvement ouvrier au cours des dernières années.

On ne peut cependant espérer que Pratt livre une marchandise nuancée; c'est une position rigide qu'il adopte. On ne saurait s'en surprendre; inspirés de certains leaders syndicaux de l'usine, invité par le syndicat à assister à une "séance" de négociation à huis-clos au bureau du ministre du travail, l'étude se caractérise par un partipris évident et une prise de position prosyndicale et ouvrière. En outre, c'est tout un système politico-socio-économique qu'il remet en question: "accepter les règles du jeu fixées par l'État capitaliste, c'est reconnaitre, à l'avance, que l'on se conformera aux ordres des agents du capitalisme. La firme profite donc d'un allié: l'État bourgeois." (80)

Pratt développe sa thèse en quatre chapitres. Dans un premier temps, il fixe les caractéristiques sociologiques des acteurs: la compagnie et ses actionnaires, le syndicat et ses palliers nationaux et internationaux, et finalement, l'État. Cette partie, indispensable, permet d'insérer le conflit dans son contexte propre en plus de dépis-

ter les connées historiques qui sous-tendent le conflit. Ces traits esquissés, il établit le rapport de forces entre la firme et la syndicat. Selon lui, la transnationale dispose d'un avantage grandissant sur les travailleurs, en vertu surtout des ses assises économiques puissantes. Les syndiqués, réduits à de minces rémunérations syndicales, se voient vite acculés au pied du mur.

Ces bases établies, Pratt plonge dans le vif du sujet: le déclenchement, le déroulement et l'issue du conflit sont abordés, alimentés par des sources syndicales et patronales, par des rapports gouvernementaux, par des coupures de journaux ou de revues. S'il établit une démarcation entre la base des syndiqués et l'élite dirigeante, Pratt se montre néanmoins peu enclin à signaler les réactions de cette majorité silencieuse. Les fluctuations dans les pour-cent lors des votes de grève ne sont pas commentés, la participation effective aux assemblées et leur représentativité ne sont jamais signalées, malgré les récriminations de la compagnie à ce sujet. La démobilisation progressive des travailleurs demeure donc à notre avis absente du schéma explicatif de Pratt, malgré certains efforts ménagés en conclusion. N'amenons qu'un exemple: l'auteur signale que l'emprisonnement et la surveillance serrés des leaders syndicaux explique la défaite rapide des grévistes, puis, du même souffle, affirme que "le conflit de la United Aircraft s'insère dans cette prise de conscience des travailleurs contre l'oppression du système capitaliste." (85) De quels travailleurs parle-t-il donc alors? La démobilisation d'une masse ouvrière privée de ses élites ne prouve-t-il pas plutôt que cette prise de conscience tarde à se manifester parmi la majorité des grévistes impliqués?

Dans un dernier chapitre, l'auteur se penche plus à fond sur les stratégies des opposants, comme pour mieux tirer les leçons de l'expérience vécue. S'il est important de dégager les tactiques des parties qui s'affrontent, on se demande les motifs qui ont amené Pratt à ne pas en insérer les composantes chronologiquement. Car ces stratégies, énoncées par les élites, ont une influence déterminante sur les négociations, sur la pertinence de poursuivre la grève, elles engendrent des actes de violence que l'auteur ne manque d'ailleurs pas de signaler. Les extirper ainsi d'un contexte pourtant bien documenté, serait-ce pour satisfaire une théorie explicative, nous apparait donc regrettable.

Pratt aura néanmoins fait oeuvre utile. Comme il le signale, l'étude, amorcée dans le vif de la grève, aura permis de récupérer de nombreux documents qui s'avèrent aujourd'hui introuvables. C'est là l'apport essentiel de l'ouvrage de Pratt. Pour le reste, on aurait souhaité un étalage moins évident d'une opinion (légitime) érigée jusqu'à la théorie, au profit d'une observation plus approfondie de ceux qu'elle désire défendre: les travailleurs de la base à travers leur comportement, leur prise de position réelle, leurs aspirations.... Car si l'on ne veut pas déclencher une révolution ouvrière *malgré* les ouvriers, il faudra d'abord les rejoindre au niveau de leurs préoccupations propres. Pour cela, il faut d'abord savoir les accepter dans toute leur diversité.

Jean Letendre
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James Lorimer and Carolyn MacGregor, eds., *After the Developers* (Toronto: James Lorimer 1981).

FOR ALMOST A DECADE James Lorimer, as author, editor, and publisher, has been an important source of insight into the inner workings of contemporary Canadian cities. At his best Lorimer has contributed penetrating analyses of both civic government and the property-development industry. *After the Developers* does not live up to this earlier standard and can best be described as a rather uneven and poorly focused collection.

This slim volume contains an introduction by Lorimer and twelve papers which are grouped under four headings; namely,

Cities in the Economy: The Industrial Strategy Issue (two), Shopping and Cities (five), Reform and Power at City Hall (three), and City Planning: Expanding the Art of the Possible (two). Only the middle two of these receive more than cursory attention. In fact, the first section really contains one paper and the text of an address by Toronto Mayor John Sewell, while the last section contains a useful report on a conference of Canadian radical planners and a brief discussion of a large co-op housing project in Montreal. This lack of both balance and substance is a shame, for as Lorimer notes in his introduction "Canada's cities have entered a new period in their history. The era of the developers . . . is over." As a result "we badly need to analyze the experiences of the last decade and the present situation in order to work out our position on the question of what is to follow the developers' era and in order to fill the intellectual and political semi-vacuum that now exists in urban policy in Canada." (6) Unfortunately, there is not much in *After the Developers* that will contribute in any meaningful way to the debate over the future shape of Canadian cities. Too many of the papers are either reworked versions of earlier efforts, or impressionistic and speculative. Too few provide the penetrating insight that has come to be associated with James Lorimer and Company. The overall impression is one of a book that is decidedly premature.

One of the real problems with this book is that its intended audience remains unclear. The length, depth of analysis, and level of research vary widely from paper to paper. On the one hand, politicians and other non-academics will be confused by the use of both undefined economic geographical jargon (market potential, city-serving employment) and maps with a zenithal equidistant (square root transformed) projection in the paper by Michael Ray and Roger Roberge on "The Pattern of Post-War Urban Growth: Multinationals as City and Regional Planners" (some of which has appeared several times already in the professional geographic literature). Academics, on the other hand, will find the lack of documentation and analysis in several of the papers less than satisfactory. For example, the essay entitled "Main Street: Can It Survive the Shopping Centre?" by architects George Baird and Barry Sampson explores a crucial issue facing many smaller Canadian urban centres, but is loose in its use of terms (malls are not the same as shopping centres) and provides the reader with no concrete sense of the retail structure in the three places chosen for discussion — Napanee, Brockville, and Brantford. The blame for this unevenness and lack of direction, however, clearly rests with the editors of the volume. At best, the papers represent a brief glimpse at where Canadian cities have been with respect to a few issues. There is precious little here concerning the future directions for the Canadian urban system except for some of the observations made by Lorimer in his introduction.

The fullest section of the book is that dealing with Shopping and Cities. Lorimer notes that "shopping centres have been the last of the prototype projects of the corporate city to fall under the scrutiny of citizen activists and others concerned about urban form." (8) The five papers in this section at least raise some of the important issues associated with retail development in Canadian cities. These issues include the process of shopping centre development (which is documented in a fine essay by Graeme Lang entitled "Shopping Centres: The Case Study of Corner Brook"), the impact of shopping centres on downtown retail areas, the difficulty of locally based merchants compared to chain stores in gaining access to shopping centre space, and the general need to regulate shopping centre growth and development. There is indeed a need to comprehend better these issues in urban Canada. While the papers in this section do make a contribution in this regard, tighter editorial control could have produced a much better package. For

example, the title of the section, "Shopping and Cities," is misleading. These papers deal, for the most part, with shopping centres. More papers on the various other trends in retailing or an introductory essay which places shopping centres within the larger Canadian retail context would have been useful. No real attempt is made to map out the future retail structure of urban Canada, yet in two separate places the role of the Ontario Municipal Board is spelled out in some detail. While the analyses of shopping centres in such all-too-often-neglected provinces as Newfoundland and P.E.I. are most welcome, the section ignores several important trends in central Canada. There, and especially in the major cities, older shopping centres have had to be upgraded to remain competitive with the newer and larger regional centres that appeared in the 1970s. For many centres this has meant enclosure (often accompanied by expansion) of the existing structure and a search for an appropriate tenant mix. For example, Dixie Plaza in Mississauga to the west of Toronto was forced to adopt a discount orientation in order to remain as a viable alternative to Etobicoke's massive Sherway Gardens. In North York, older Bayview Village found itself sandwiched between two large regional malls — Yorkdale to the west and Fairview to the east. To carve a niche for itself in Toronto's retail structure, the plaza was enclosed and the majority of the shops were leased to *independent* retailers. This revitalization process deserves some scholarly attention, for it is subtle changes in the existing retail structure of our cities such as the examples described above that are likely to direct consumer behaviour over the years to come.

For many reasons then, *After the Developers* is a disappointing book. From time to time it teases the reader about the future of Canadian cities, but most of the papers merely describe what happened during the developers' era. And looking at the construction activity evident today in Toronto, I'm not so sure that that era has passed. We need to assess, for example, the role that foreign capital will play in keeping that era alive. (Who really benefits when offshore money drives up the price of housing?) One almost wishes that Lorimer would recall all of the circulating copies of this book, put more thought into its direction, commission several more essays, and reissue a volume of some substance and importance.

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Catherine Wismer, *Sweethearts: The Builders, the Mob and the Men* (Toronto: James Lorimer 1980).

FOR A 20 year period beginning in the early 1950s, the cityscape of Toronto underwent a dramatic restructuring as suburban housing developments, high-rise apartments, and shopping centres sprang up almost overnight like mushrooms in a well-fertilized yard. This massive urban development demanded the involvement of big business, big money, and a large labour force. Catherine Wismer claims that hers is the first study to reveal the real story behind this construction boom. As indicated by the book's subtitle, Wismer weaves the tale around three main threads: the builders, the mob, and the men. The "builders" refers to a group of profit-seeking opportunists, many of whom attended Harbord Collegiate in the 1930s, who recognized a good thing when they saw it and directed their business interests towards land development in the early 1950s. The "mob" includes a number of figures from the world of organized crime, such as American gangsters Salvatore "Lucky" Luciano and Meyer Lansky, whose laundered casino savings found their way into legitimate, if dubious, development schemes in Toronto. Many of these charming characters also turned to union organizing for purposes of extortion and under-the-table agreements with

builders, known as "sweetheart" contracts. The "men" are the massive numbers of immigrant Italian workers who suffered at the hands of ruthless employers and self-serving union leaders. When tied together, these threads expose a pattern of exploitation and corruption which characterized the growth years in Toronto.

Wismer writes in a clear and agreeable journalistic idiom. She begins the study by rotating chapters on each of the three major themes. While this approach provides a neat and tidy introduction, the important connections between the three areas emerge too slowly. By the middle of the book the mixture is more apparent, although at this point Bruno Zanini, one of the few honest union organizers to appear in the study, dominates centre stage. Near the end of the work, the connections become a little tangled and, despite the sometimes exciting pace, the reader must struggle to keep the story's various branches sorted out. Although the study focusses primarily on characters and events directly connected with the Toronto building trade, the book ends abruptly and inappropriately with the reminiscences of American mobster Meyer Lansky.

Organization aside, the author's motives and methods deserve close scrutiny. Wismer states that she decided to pursue this topic after someone "in the apartment business" approached her with his story. Although she does not identify this character, the slant of the book suggests that it was union organizer Bruno Zanini, or someone sympathetic to him. This is purely speculative, but Wismer quotes Zanini at length throughout the work and much of the story is devoted to vindicating this thief turned worker turned union man who, we are led to believe, has been unjustly accused of illegitimate activity. Although Wismer consulted a number of periodicals, government inquiries, and secondary sources, she relies on oral history for much of the narrative. Her findings cannot help but be coloured by the opinions of the individuals she interviewed, none of whom can be considered objective bystanders to the events depicted in this book. The author believes that she was "placed by chance in a position of trust" by her contacts. This may be true, but the possibility remains that some of these mysterious characters used her as a vehicle to cast favourable light upon their own roles in this controversial drama. Wismer accepts much of this personal testimony at face value. But without stronger corroboration, her case sometimes lacks a convincing air.

Wismer's reluctance to look beyond the immediate horizons of her sources results in a couple of careless statements. In the foreword to the study, the author declares: "At times it seems that some unions are little more than branches of organized crime." While it is undeniable that mobsters have penetrated the higher echelons of some large unions, this general statement unjustifiably implies that rank-and-file workers tacitly support underworld activity. Union manipulators may benefit from the apathy of regular members, but they do not receive endorsement. Referring to the effects of environment on behaviour patterns, Wismer offers this assessment of the main characters in the book: "If you see people in the context of the world they are part of, it is possible to understand the true logic of their actions." This statement is a genuine attempt at empathy, but it is too generous. Thousands of individuals suffered through "poverty, depression and prejudice" without succumbing to the calls of a violent business world or organized crime. Mobsters such as Luciano and Lansky in particular are as much the makers of their own world as they are the products of it.

The book's most serious shortcoming, however, lies not in what it says, but in what it fails to say. Although Wismer's treatment of the story's central figures sometimes lacks conviction, she accurately and powerfully describes the routine nature of corrupt business practices and the horrendous living and working conditions of the workers. Yet nowhere does she use

this information towards a systematic analysis of the institutions and traditions which created and prolonged these injustices. This perhaps exceeds the author's stated objectives, but the universal value of the book would have increased substantially had she attempted these judgements. The inefficiency of the early housing schemes reveals the inability of free enterprise to provide essential social services. The creation of monopolies and the common use of rigged bidding procedures destroy the myth of the competitive market. As Terry Copp and Michael Piva have demonstrated for an earlier period in Canadian history, it is painfully clear that workers do not necessarily share in the fortunes of economic expansion. Wismer's revelations expose the failure of government, police, and union bureaucrats to regulate a violent and exploitative industry. The author prefers narrative to analysis, however, and examines none of these factors within the larger context of historical development. Like a fickle jury, Wismer muses over volumes of incriminating evidence, but defaults on delivering the hard verdict.

Sweethearts is an informative and provacative study of one important area of urban development. It adds a new and enlightening dimension to the impression created by labour history texts that white-collar and public service unionism dominated the affairs of labour in these years. The study's storybook character and lack of historical analysis, however, limit its academic usefulness.

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Fernand Harvey, *Le mouvement ouvrier au Québec* (Montréal: Boréal express, coll. Études d'Histoire du Québec no. 10, 1980).

IL NE FAUDRAIT PAS s'y tromper: la second édition de ce recueil de textes sur le mouvement ouvrier québécois a bien peu à voir avec la première. En effet, Fernand Harvey a choisi de ne retenir que deux des six textes de l'édition de 1973, le sien sur les Chevaliers du travail et celui de Louis-Marie Tremblay sur "l'influence extragène en matière de direction syndicale au Canada," d'ajouter sept autres textes et d'inclure une annexe sur les effectifs syndicaux, le tout dans une présentation renouvelée et fort attrayante du no. 10 des éditions du Boréal express.

Proposant davantage une "histoire thématique" du mouvement ouvrier québécois qu'une histoire linéaire-chronologique, Harvey nous avertit dès le départ, dans une introduction fort intéressante et complètement renouvelée, que sont exclus du recueil des textes portant sur la condition ouvrière ou sur la législation du travail (à l'exception du texte de Jean Boivin). On pourrait ajouter que la question des femmes et de la place de leurs revendications dans le mouvement ouvrier, la question de l'action politique ouvrière après 1920, en particulier le rôle des communistes et des sociaux-démocrates, et enfin la question des organisations syndicales marginales et indépendantes ont été mises au rancart; ces questions commencent à peine à faire l'objet de recherches historiques qui, il faut bien le dire, s'en tiennent pour la plupart bien plus à l'étude des mouvements institutionnalisés qu'à celle de la *classe* ouvrière proprement dite.

D'ailleurs, Harvey a bien raison de signaler que l'avènement tardif de l'historiographie des travailleurs québécois "reflète la difficulté d'être de la conscience historique de la classe ouvrière" au Québec. (44) Le caractère périphérique de la société québécoise, la superposition d'influences extérieures et la série d'allégeances contradictoires (ethnique, religieuse versus l'appartenance de classe) proposées à l'ouvrier québécois de même que l'absence relative d'événements historiques marquants expliqueraient ainsi la faiblesse pour ne pas dire l'inexistence d'une tradition de gauche au Québec selon Harvey. (44-48) Par ailleurs, le renouveau

des années soixante-dix dans la recherche historique sur les travailleurs québécois trouve ses fondements dans l'évolution historique du Québec moderne (industrialisation accélérée après la Second Guerre mondiale), dans l'évolution même de la méthodologie historique (apport de l'école des *Annales*, recherches de J. Hamelin à l'Université Laval, essor de l'approche marxiste, recherche en relations industrielles, etc.) et bien entendu dans l'émergence d'une conjoncture sociale qui révèle clairement les rapports conflictuels sur lesquels repose notre société et qui redonne par conséquent toute sa validité à la recherche sur les classes "oubliées" par l'historiographie nationaliste traditionnelle.

Le recueil lui-même rend compte de ces progrès, compte tenu des limites que nous avons indiquées plus haut. Le texte de Harvey lui-même sur les Chevaliers du travail demeure la meilleure introduction à l'étude de ce mouvement négligé dans le passé. La recherche de Margaret Heap sur "la grève des charretiers de Montréal, 1864" illustre bien cette nouvelle tendance de la recherche historique qui rejette l'apologie ou la relation épique pour retenir le sense historique et les manifestations sociales révélatrices de l'agir collectif d'une classe; il s'agit ici d'une grève des artisans-charretiers en lutte contre la prolétarisation et la victoire du capitalisme industriel à Montréal. Il faudra bien d'autres recherches de cette qualité pour que nous puissions mettre à jour l'idéologie, la conscience et la pratique sociale réelle de ce que l'on appelle, parfois trop rapidement, la classe ouvrière québécoise au XIX^{e} siècle.

Pour le début du XX^{e}, le recueil nous propose un texte revu de Jacques Rouillard sur l'action politique ouvrière qui remplace avantageusement le très subjectif article d'Alfred Charpentier ("Le mouvement politique ouvrier de Montréal [1883-1929]", pp. 147-168 dans l'édition de 1973): les courants socialiste et travailliste sont bien campés, et on comprend que cette période ait été "l'âge d'or" de l'action politique ouvrière, mais comme le signale fort justement l'auteur en conclusion, "sans le support financier des organisations syndicales, le Parti ouvrier était condamné à végéter. . . ." (213) L'article de Robert Babcock, "Samuel Gompers et les travailleurs québécois, 1900-1914" soulève bien des problèmes. L'auteur cherche à démontrer à tout prix que la division du mouvement syndical canadien repose sur une base ethnique et que la cause majeure en est "l'ambition de Gompers de réunir tous les travailleurs de l'Amérique du Nord sous la bannière de l'*American Federation of Labor*." (148) La scission de 1902 au congrès de Berlin du Congrès des métiers et du travail du Canada qui expulsa de ses rangs les Chevaliers du travail alors majoritairement implantés au Québec et l'incapacité des unions américaines à tenir compte des traits culturels propres aux Canadiens français auraient contribué à cette division du mouvement syndical canadien. Or c'est là ne pas tenir compte du fait majeur que c'est le clergé et la petite-bourgeoisie au Québec qui ont contribué à diviser le mouvement syndical canadien sur une base ethnique en s'attaquant au caractère étranger des syndicats américains et en dénonçant leur "neutralisme" et leur soi-disant "socialisme;" c'est également reléguer au second plan les victoires importantes remportées par le CMTC au Québec après 1902, victoires que Babcock mentionne sans les expliquer; c'est finalement négliger les efforts réels, malgré une indéniable incompréhension à l'origine des traits culturels canadiens-français, du CMTC qui embaucha Joseph Ainey comme organisateur en 1909 malgré des ressources très limitées. Gompers a beau ne pas inspirer confiance, il ne faudrait pas retomber pour autant dans les travers de la propagande clérico-nationaliste du début du siècle en guise d'explication historique.

Deux articles proposent des éléments de synthèse fort pertinents sur l'évolution de la CTCC/CSN et de la FTQ: Jacques Dofny et Paul Bernard, "L'évolution historique

du syndicalisme au Québec" et Louis-Marie Tremblay, "L'influence extragène en matière de direction syndicale au Canada." S'ajoutent les deux articles bien connus de Hélène David, le premier, "La grève et le bon Dieu," remettant en question les interprétations de Pierre-Elliott Trudeau et de Jacques Cousineau sur les rapports entre la grève de l'amiante de 1949 et la société québécoise en suggérant que la grève a marqué "l'émancipation de l'État... de la tutelle idéologique de l'Église;" (182-183) ce qui est certain et peut-être moins ambitieux comme thèse, c'est que "l'échec de l'Église à imposer sa vision du monde dans les relations de travail" (intro. de Harvey, 34) aura été consommé lors de cette grève. Le second article, "l'état des rapports de classe au Québec de 1945 à 1967," fournit une analyse très fine de la conjoncture de cette période, décortiquant celle-ci en six moments différents et concluant que la révolution tranquille n'aura été qu'un "intermède" pour le mouvement ouvrier qui reprendra sa pratique de militantisme et de lutte qu'on avait connue vers la fin du régime duplessiste. (248-255)

Un dernier mot sur l'article de Jean Boivin, "Règles du jeu et rapport de force dans les secteurs public et para-public québécois:" il s'agit d'une excellente mise en situation des paramètres de la négociation collective dans ce secteur suite à la publication du rapport Martin-Bouchard. Mais le caractère nettement engagé des conclusions de l'auteur, les appels à la "raison" et l'approche technocratique qu'il véhicule nous font nous interroger sur la pertinence de ce texte dans un recueil d'histoire du mouvement ouvrier.

Il faudra certes modifier le titre de la première annexe qui porte sur les centrales syndicales au Canada et au Québec de 1873 (et non de 1882) à 1980 et y ajouter la *Centrale des syndicats canadiens* (CSC) du regretté Kent Rowley qui compte au moins un syndicat au Québec, de même que la *Ligue d'unité ouvrière* (LUO) qui regroupa près de 40,000 travailleurs et chômeurs canadiens sous l'égide du Parti communiste canadien de 1929 à 1935.

Sommes toutes, ce recueil nous invite à reprendre les termes d'une histoire de la classe ouvrière au Québec, histoire du mouvement *et* histoire des hommes et des femmes qui composent tous les jours la réalité complexe et multiforme de cette classe ouvrière.

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Louise B. Lavoie, *Les Débuts du Mouvement Ouvrier à Sherbrooke, 1873-1919* (Université de Sherbrooke: Groupe de Recherche en Histoire Régionale, Département d'Histoire, 1979).

THIS SHORT VOLUME sets out to trace the early development of organized labour in the city of Sherbrooke and thus provide the foundation for a general history of the workers' movement in the Eastern Townships. The author views it as a pioneering work in local history: external factors therefore are to be discussed only when they bear a direct relation to the local union movement. (8) This statement of purposes presages the strengths and weaknesses of this study.

Les Débuts . . . is a pioneering work in that it brings together in a systematic fashion little known information about the early years of organized labour in this important regional industrial centre. Ms. Lavoie is to be commended for undertaking a difficult task for there is little documentation for the period. As a result, she has been forced to rely primarily on the scattered and sketchy information contained in local newspapers, the published census, and a few related government documents. The heavy reliance on these sources no doubt explains to a considerable extent why this work, in a methodological sense, is not as innovative as one might wish. Much of the volume is given over to a simple listing of which unions existed in a given year. While acknowledging the scar-

city of sources, the reader is entitled to greater analysis.

To begin with, the author tends to create the image of a monolithic male working class in Sherbrooke. Even if one is restricted to using census data, it is still possible to provide a breakdown of the working population by sex, ethnic origin, and age for the last decade under examination. We are told that the study begins in 1873 because that year appears to mark the first concerted action on the part of workers when 25 young women went on strike at Paton's woollen mill. Nonetheless, women receive virtually no discussion in this book.

According to the 1911 census, one quarter of the work force in Sherbrooke's manufactures was female. One would like to know how the substantial number of female workers affected, or was affected by, the local trade union movement. Equally important, how did the ethnic composition of the labour force influence organization? Were not several of the most successful early unions, such as the machinists, predominantly British in their membership?

The significance of fluctuations in the fortunes of union organization in particular, and of worker militancy in general, could be more clearly established within a larger context. There are several points at which the author might have related her own findings to provincial trends. The local focus is applied too rigidly, and the failure to draw upon other studies for significant comparisons only aggravates the cursory nature of the discussion.

A more rigorous theoretical framework is also needed to allay the descriptive quality of this work. The author's concepts of class and class consciousness, and their application to Sherbrooke's workers, remain unclear. On page 65, she suggests that certain economic factors facilitated the emergence of a class consciousness while 20 pages later, she concludes that it was difficult to find common ground among the various unions. Similarly, the discussion of health-impairing working conditions is predicated on the belief that such conditions were a factor likely to promote the growth of unions. Evidence for this view is drawn exclusively from the writings of middle class observers whereas the subsequent discussion of workers' strikes, and of the opinions of union organizers such as Narcisse Arcand, indicates that salary, and not work environment, was the issue around which workers organized.

Finally, the author concludes that clerical opposition was the principal obstacle to union organization in Sherbrooke prior to 1919. She thereby implies that the workers were submissive servants of the Church who only embraced union membership in an important way once Catholic unions were formed. Such an interpretation takes no cognizance of the fact that it may have been as much the form the Catholic unions took (industrial unions as opposed to the earlier craft unions), as their religious affiliation, which accounted for the substantial increase in union membership after World War I.

Notwithstanding these shortcomings, this volume will serve as a useful reference work. The section dealing with salaries and prices is particularly interesting and informative. No doubt this work will also serve the author's goal of encouraging additional local studies.

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Benoît-Beaudry Gourd, et al., *De l'abbitibbi-témiskaming 5* (Rouyn: Collège du Nord-Ouest, Cahiers du Département d'Histoire et de Géographie no. 5, 1979).

IN MANY WAYS, the eight years of work by Benoît-Beaudry Gourd and his colleagues at the CEGEP of Rouyn-Noranda constitute a model enterprise in Canadian regional and oral history. This series, which they call an "étude de fond," will help us pro-

ceed to the levels of analysis exemplified by *History Workshop*, *Mouvement Social*, *Labor History*, or Tamara Hareven's study of Franco-Americans in *Amoskeag*.

The Gourd series includes a bibliography, a catalogue of public church, union, family, and newspaper archives, and oral-history taping of the living memories of pioneers, priests, and workers. The five volumes of *Abitibi-Témiscamingue* (or *l'abbittibbi-témiscaming*) used conferences, CEGEP seminars, and other work in progress. The have established the basic structure within which more detailed narrative, biography, theory, map and photo collections are planned. Some gaps, however, are apparent: the series so far has been short of class, élite, and stratification analysis; of family history; and of studies on the changing role of women.

It is by comparison with other regions that the strengths, weaknesses, and future potential of the series may best be shown. Maurice Asselin's "Le rôle de la frontière dans les relations entre le Nord-ouest québécois et l'Ontario" in this volume argues that the open frontier has been a determinant in the twin region. Forest, mine, and agricultural industries (in that order) developed on both sides, in Quebec and Ontario regions sufficiently similar that social, cultural and economic comparisons may be sharpened to a fine point. Gourd's study of the French press, and Dan Glenday's of unionism, are a step in this direction. Moreover, Abitibi-Temiskaming is central in the "bilingual belt" (described by Richard Joy over a decade ago) that stretches from Sault Ste Marie/Hearst to Moncton: it is in this belt that the major questions of francophone rights or their converse, assimilation, are of critical importance.

This belt was largely created by two major waves of colonisation, in 1885-1917 and in 1932-1939: French Canada's version of the "last best west." Like homesteaders elsewhere, the *colons* had to work for rail, mine, or logging wages for their annual (and essential) cash stake; they were "foiseurs de terre neuve et leur misère était à bout de bras."

Reported *missionaire-colonisateur* Abbé Minette in 1946: "On défriche comme au temps de Champlain, c'est comme si la mécanique, la gazoline, les tracteurs, les essoucheuses, les outils savants n'étaient inventés que pour les gens des villes." (189) Here the women had no wool to spin. They helped the men pull stumps with their bare hands. There was no capital to buy machines and their horses foundered in the desperate rush of winter pulpcutting in order to earn cash. When they had to abandon the homestead, it was the end of a traditional way of life, "la fin d'une rène" — a silent revolution which removed almost two-thirds of Canadian farmers from the soil in the 1960s, and which must be analyzed in a continent-wide context, with the rise of agribusiness.

Common to the twin region in Ontario and Quebec, according to Asselin, Gourd, and Glenday are:

1) the two great waves of colonization.
2) the role of miners' unions in a multilingual workforce.
3) a reserve army of labour located in marginal or under-capitalized agriculture, available at all times for seasonal logging, and occasionally for strikebreaking. (Government regulations ensured that timber and mineral rights did not go with the homestead. Pioneering in the mid-north thus became a trap.)
4) questions of ecology: pollution, resource management, control of the means of production, and their rationalization by "timber wolves" and "mining barons."
5) the colonial development of towns (Stelter) and of the region as a whole under metropolitan Montreal or Toronto (Are they not colonies of a colony? Thus the creation of ghost towns during downswings in the trade cycle, and of a regional mentality of abandonment, separatism and protest.)
6) the perennial shortage of local capital, affecting farmers, small business, ethnic stratification, and the region as a whole.

(Recent research by Leo Johnson suggests that the low-wage economy of the Ottawa Valley dates from as early as the 1840s. On a world scale, the region's crises and uneven growth are part of a general "development of underdevelopment.")

7) the economic inferiority of Québécois, Franco-Ontarians, and Franco-Americans, a poverty cycle of low income and low education.

8) the multiple bind in which cheap-labour ethnic groups find themselves: ethnocultural, educational, class and sex discrimination.

9) Nouvel-Ontario developed as a frontier of labour mobility around transportation lines, Québec Nord-Ouest as a frontier of group immobility based on parish and lot. In 1934 Témiscamingue still had no roads to the Ottawa or St. Lawrence valleys, and was cut off entirely in spring and fall; its dependence on Ontario's TANO Railway, as Asselin shows, made it vulnerable to both economic and cultural assimilation. Now it suffers twice the unemployment rate, massive depopulation, and a consequent threat of much greater assimilation.

10) attempts by Quebec workers to better their economic position put their culture at risk. Part-time farmers gradually become full-time wage-workers; those who sojourn seasonally in Ontario mining towns eventually stay, and induce their families to move. As many as a third of the workers went to Ontario in the 1950s. Unionism, which must emphasize class solidarity over ethnicity (at least in Ontario) speeds anglicization. Meanwhile, the educated youth is drawn to Ottawa or the South, leaving the 35 per cent Franco-Ontarian population with a truncated elite.

The chronology of French-Canadian colonization and of industrial change is common to both regions. Between 1880 and 1910, two generations of Quebec *colons* followed the railway and logging industry up the Ottawa to Lake Temiskaming, and west to Sudbury. Originating from Joliette and the lower St. Lawrence, isolation made them a distinct group, with a rich popular culture; but their elites, distrusting the ease with which they "sink their nationality and pass off for what they are not," mounted a coup that in 1910 separated the French schools, caisses populaires, parishes, and voluntary associations. The Clay Belt was to be a new frontier of Laurentia; according to *nationalistes*, it was an "autre Québec ouvert à notre action." (189) Throughout the area pulplogging, as in *Jean Rivard, Menaud Maître-draveur, Nuages sur les brûlés*, or *Mon Oncle Antoine*, was so essential that it was difficult to tell whether the people were small-holding *colons* or wage-earning *bucherons*. This cheap, docile labour force was ideal for the growth of pulp and hydro empires.

It was an uneasy symbiosis. Disputes over Sunday work, tires, and other disasters showed how fragile the social and ecological balance could be; an Abitibi Paper dam, built without Quebec government permit, in 1922 ruined 100,000 acres of homesteaders' land. Premiers of Quebec and Ontario spent years, alternately bonusing the newsprint and hydro barons, and trying to rationalize the industry to keep wages above starvation level. Frustrated by the newsprint cartel's refusal to observe quotas and wage decrees, one Quebec leader said, "I have never met a group of men so disloyal... the agreements between them are mere scraps of paper." In Ontario, the Reesor siding murders of 1963 and the Boise Cascade strike of 1979 show that big paper companies are still able to cut wages and break unions by recruiting dependent contractors among the *colons*.

Capitalism was equally unchecked in the mining industry, where the Western Federation of Miners, ancestor of Mine Mill, was beaten in a series of strikes from 1906-1919. Relatively high wages, a seven-day week, and silicosis after five years, were the miner's lot. The mineowners soon learned the strategy of playing "white" workers against "Reds." When the big Hornepayne mine opened in the

1920s, local promoters of colonization became drumbeaters of the boom: "Quelques centaines de milles habitants de plus! Les vastes usines à côté des fermes canadiennes-françaises. Pourquoi pas?... Sous le soleil du bon Dieu présentement, l'or et le cuivre semble reluire davantage." But *colons* did not enter mining in any number; indeed, they protested racial discrimination by the mine foremen. (54) Not until the desperate foreigners' strike against wage cuts in 1934, in which half to a third was blacklisted by the mineowners, did the foremen shout "Come on, Frenchies." Out in the back concessions the *missionnaires* noted: "Duval Rodolphe — parti sans raison aux mines." The tactic would be used, with the open support of Catholic Action, in the 1941 Kirkland Lake strike, the 1943 Sudbury union drive, and in the Cold War. Obviously, such industry-wide phenomena cannot be explained from the viewpoint of one linguistic group. It is not only *canadien* workers, recent migrants from a conservative rural milieu, who manifest too much "individualisme," too little "solidarité."

The precarious life of the *colon* family was also a powerful predisposing force. While mining, power, and paper companies strode across the frontier like colossi, the settlers were sunk in the "muskai." After some bumper crops in the 1920s, a family could support itself for a year here on $500 in the 1930s; in Montreal mere subsistence was $1,200. But a rich farmer was "unique en Abitibi." Even if pulpwood was as high as $4.50 a cord, the merchants only paid in truck, not cash, in the Val d'Or of the 1930s. The *colon* had to leave the land "la majeure partie de l'année" — and the reliefers who were brought here on the plan Vautrin were in desperate straits. Roadwork earned them $15 a month, they ruined their horses, freight charges were $2.40 on a bag of flour, $2.55 on a bag of potatoes that sold for 85¢ in Montreal. Michel Chartrand remembers, furious, how they died of typhoid and the polluted meat on the plan Vautrin: $100 of supplies a year, $10 a

cleared acre: "It was then that I realized it wasn't the English or the Jews that were killing us. . . ."

What needs to be explained, then, is how working-class solidarity grew. Glenday emphasizes the periods of the rise of Mine Mill in 1937-1948, the Steelworkers' raiding in 1950-1957, and the bosses' defeat of union security for the years up to 1964. We need to correlate these with the gold boom of the 1930s, the company houses, company control of every aspect of municipal life, the infamous Bedeaux "bonus" system that had entered all mass industries. By 1937 the speedup, graft, and tyranny had discredited the foreman system. We need to know more about the factors that led to union success after decades of failure: organizing on the cell or "Anyox" system, recruiting organizers from each ethnic group, winning over key crafts, the wartime labour shortage, patriotism and production, health and safety. Was the *curé* still "quasiment un dieu sur la terre" in 1943? And in 1948 when he denounced equally the Reds and the Social Credit for preaching "class war?" What subtle shifts in class structure had occurred in five years? The government's policy of evasion and delay, and mineowners' use of police and paramilitary Frontiersmen to awe unionists in both provinces needs examination. So do the subtle but massive shifts in public opinion: the rise of social Catholicism, the Bloc Populaire, the Union des Electeurs, the CCF, and the role of the Church in relation to each. That the rise of the Creditistes, regional separatism (the call for an "eleventh province" from the Soo to Manicouagan), the final collapse of the subsistence farm system, and tremendous out-migration to Ontario, all occurred together, requires some explanation. Above all, we need a deeper, richer exploration of the human experience. Two examples of such research must suffice: the NFB's magnificent film on the workers' takeover of a shutdown plant in *Temiskaming* (1979); and Joanne Drake's examination of the heroic age of local history on a British Columbia frontier. She juxtaposes the standard pictures of local notables like the "first druggist on Main Street," with comment from the women who worked for *him, and why, and how their work (paid or* unpaid) was essential both to the family and frontier. Drake's *A Matter of Doing Something to Live* (video from Women in Focus, Vancouver) is an approach that cries out to be applied to the "gens" as well as the "notables" of our "villages et visages."

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Warren Caragata, *Alberta Labour: A Heritage Untold* (Toronto: James Lorimer 1979).

ALBERTA HAS BEEN and remains, like its topography and climate, a province of considerable variety; the contrasts and diversity it presents have long challenged the serious observer. As the birthplace of the North American version of Social Credit, for example, it set both New York financiers and Mackenzie King atwitter; yet, at the same time it accommodated some of the most Neanderthal-like enterpreneurs and politicians in the country. Likewise it offers both opulence and poverty, restrained good taste and garish overstatement. One of the reasons for these manifold contrasts is that the opportunities for employment and resource exploitation have held magnetic attraction for thousands around the globe. Its society reflects this fact. Its population has been consistently among the most transient in Canada with roots stretching back to virtually every corner of the world. Consequently, Alberta has been able to absorb both American-style fundamentalism and old world piety as well as strident ethnic expression benevolently supervised by probably the most unrepentantly WASP leadership of any in the prairie provinces.

Since most early residents of Alberta came to work, the labour force offers simi-

lar variety. For example, those employed in extractive industries were as far removed culturally from the urban bureaucrats who claimed to serve them, as were the lonesome farm labourers from the workers in a manufactory in Edmonton or Calgary who produced the overalls for the agriculturalists to wear. Consequently, the tensions and divisions within the "working class" are obvious challenges to objectivity for anyone working in the history of the region and the province.

Another challenge to the researcher has always been the large void due to a paucity of serious work undertaken on Alberta's history outside of the political sphere. Hence, any major contribution on a subject as large as Alberta labour was sincerely welcomed simply for its potential to fill many gaps in our understanding of the province. Since no one had attempted a project of the magnitude of *Alberta Labour: A Heritage Untold*, (since one of Eugene Forsey's research assistants tried to chronicle the development of Alberta's trade unions a decade and a half ago) the results were eagerly anticipated. The product of Warren Caragata's pen, however, has proved to be an example of opportunity lost as what appears within its glossy 162 pages falls far short of expectations.

The book is disappointing in several respects. Its scope is one of them. The title promises a discussion of Alberta labour; the text deals only with the trade union movement and forgets the majority of the province's workers who were involved in non-union sectors. This major omission aside, however, even the discussion of Alberta unionism leaves a lot to be desired. There is no apparent theme to the book apart from a closing cliché that there had been progress for some workers and none for others. Caragata does not say why Alberta unionists deserve special treatment in a book. One can only assume, therefore, that nothing was particularly unique about the movement's development, an assumption strengthened by Caragata's detailed discourses on affairs often far removed from the province or the region.

By comparison with the book as a whole, the early chapters dealing with the origins of trade unionism in Alberta possess some directional focus but the theme is unfortunately simplistic and shallow. In Caragata's view union organization emerged from weak, vulnerable workers rising up spontaneously in response to large, oppressive corporations like the CPR backed up governments and police, and each instance of exploitation is hammered home with unnecessarily long quotations. (The ones on page 70 are typical.) Consequently, radical organizations like the IWW and the Western Federation of Miners are glorified out of all proportion, as are the romantic communists between the wars whose sole aim Caragata sees as the betterment of the condition of the Alberta toiler. Perhaps this "them versus us" interpretation is what the contemporary unionist (for whom the volume was obviously designed) wishes the past was like but once placed within the proper context it can be seen to be erroneous. The past must be viewed on its own terms and with the realization that changing technology and attitudes have contributed as much to the present condition of labour as did the strikes and struggles Caragata so laboriously portrays.

One suspects that more assiduous use of the documentary collections to which his footnotes refer would have produced a more sustaining account. For example, the author cites the CLC collection in the Public Archives of Canada and yet he obviously missed the voluminous correspondence there concerning the almost heroic day-to-day struggle of dedicated organizers in the 1940s against not only recalcitrant employers but also tight budgets imposed by union brass more concerned with spending the money in Central Canada. (Volumes 127 and 136 contain cases in point.) Even some of the secondary sources available but not consulted might have tempered Caragata's view. A.A. den Otter's M.A. work on the social development of the Coal Branch and

the sense of community which prevailed there is one such study. Similarly, these like Frank Karas' on the miners in the Crow's Nest area and the W.R. Askin's examination of press reactions to labour activities in Edmonton in 1918 and 1919 could have been used to good advantage. Other glaring omissions include John H. Thompson's *Harvests of War* and David Bercuson's *Fools and Wise Men*. Both books could have rewarded the author with a much needed perspective on prairie society during the years before, during, and after World War I.

Surprisingly, despite its simplicity, Caragata's interpretation is extremely difficult to follow. His story jumps about chronologically and wanders aimlessly through a seemingly unconnected series of events and incidents. The chapter on the depression, for example, requires a particularly alert reader to trace the narrative line through the maze of forced transitions. Added to this frustration are the irritations caused by numerous typographical errors, beginning with the first sentence of Chapter 1.

The single bright spot in *Alberta Labour: A Heritage Untold* are the photographs which constitute fully half the book. Many have never been published before. Their reproduction is first rate, a tribute to the skill of the technicians. Unfortunately, once the author becomes involved there are problems. Some of the captions are (deliberately) misleading like the one accompanying the picture of Premier Brownlee (121) which attributes greater power to the province than does the BNA Act. Similarly, a turn-of-the-century photo (100) of deportees is used to illustrate the fact that numerous immigrants were sent home for being public charges during the 1930s.

What strikes the reader most vividly about the photos Caragata has chosen is that they unintentionally undermine his basic premise that Alberta's working-class tradition was rooted in the deprivation resulting from worker exploitation by employers. Almost the entire collection shows Alberta labourers either enjoying social pursuits current in the period, or more often, proudly assembled at their work places. Most are well turned out and well fed and appear indistinguishable from other people. These scenes spotlight a simple fact, entirely ignored by the author: workers attached great importance to their jobs and that this sense of pride contributed considerably to working-class awareness, a fact that is captured in many of the photographs. Unfortunately, the subtitle of the book remains as valid now as on the day Caragata began to write his text.

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David J. Elkins and Richard Simeon, *Small Worlds: Provinces and Parties in Canadian Political Life* (Toronto: Methuen 1980); and Carlo Caldarola, *Society and Politics in Alberta: Research Papers* (Toronto: Methuen 1979).

No doubt the Communist historian of the next century will point to this curious eagerness of Canadian political scientists to focus their attention upon their federal political institutions instead of upon their capitalist economic institutions as merely another variation in the escape technique adopted by timid intellectuals in a revolutionary period.

Frank Underhill, Address to the Canadian Political Science Association, 1935.

WITH ITS JUXTAPOSITION of an underlying "crisis of capitalism" and a perceived "crisis of federalism" the current scene in Canada bears some resemblance to the situation so well described by Underhill in 1935. As in the 1930s moreover, the modern political crisis has called forth, along with a new crop of what Underhill called "Canadian Calhouns," a new "outburst of scholasticism" on the traditional and related topics of federalism, provincialism, and regionalism. Within this genre may be considered two new offerings in Methuen's Canadian Politics and

Government Series, each of them weighty compendia of essays, probably stated for use as university textbooks: *Small Worlds: Provinces and Parties in Canadian Political Life*, edited, and, to a large degree, authored by, Elkins and Simeon; and *Society and Politics in Alberta: Research Papers*, edited by Carlo Caldarola.

If my hunch about *Small Worlds* (which includes contributions, most of them collaborative, by Richard Johnson, Donald Blake, Mike Burke, and Robert Miller) is correct, freshmen in Poli-Sci 101 may expect a very rigorous introduction to their subject. They deserve, in fact, our sympathies, for this anthology is not recommended for the non-specialist. True, the Introduction and Conclusion are concise and useful (though prosaic) summaries of the major issues in contemporary "federal-provincial relations," broadly defined. However, these pieces are curiously abstracted from the eight essays sandwiched between, which can better be described as methodological exercises in the use of the most sophisticated new quantitative techniques than substantive discussions of any of the issues. Seven structure themselves around complex regression analyses of survey results; while the eighth does the same basic exercises with data on government expenditures. It may seem odd to write about provinces, parties, and politics without mentioning any politicians, or citing a platform, a campaign speech, a parliamentary debate, an editorial, or a piece of legislation. Nevertheless, this is what the contributors have done. For those who might wish to sample the methodology without purchasing the book, two of the essays have been republished from the *Canadian Journal of Political Science* (Elkins and Simeon, "Provincial Political Cultures in Canada," and Elkins, "The Structure of Provincial Party Systems"), with minor revisions.

Well over 100 tables and charts grace the 300 page volume, and form the core of the scholarship which informs it. What is needed, however, from this "new" political science is some means of informing us who are non-specialists — not least the hapless student — what it's really all about. The stark fact is that only a trained statistician with a solid grounding in upper-level mathematics would be able to read this book intelligently, or comment thereon. It represents the "latest" in methodological precision, but also, unfortunately, a case study in academic sectarianism.

At least half of the tables and charts cannot be understood without a knowledge of the formulae which accompany them (that is, calculus). Some are more straightforward. Some of these re-inforce a visceral suspicion — born of ignorance of the mysteries of the art — that the methodology masks a fair degree of obfuscation. In "Dimensions of Variation in Electoral Coalitions," Mike Burke informs us that New Brunswick is "the most unusual of provinces;" this because the Liberal and Conservative Parties receive a much larger percentage of their votes from the "working class" than in other jurisdictions. Burke calls this "surprising," with no editorial comment given. A few pages later, however, he admits that the New Brunswick data is skewed by the fact that "over 70 per cent of New Brunswick residents classify themselves as working class," far above the Canadian norm. So much for the "Control Variable" of social class: his tables merely compare apples and oranges. Any *other* result than the one cited concerning "class voting" in New Brunswick would have been most surprising — indeed, a mathematical impossibility.

It should be added parenthetically that three-quarters of New Brunswickers do *not* classify themselves as "working class," but rather, a certain percentage of a survey sample which nowhere in the article is clearly identified as to its size or representativeness. Throughout the book, the reader is asked to take all the basic data on faith.

At one point, the editors make the remarkable confession that some of the findings are "anti-climactic." This is true of certain of them that one knows, intuitively, to be correct: for instance, Elkins' painstakingly-constructed index of identification with the "national" interest — call it an index of patriotism if you will — which appears in his lead article "The Sense of Place." Quebec's stolid "anglophones" come first on the index, followed by the people of New Brunswick — the only province where they stand to "Oh Canada" in the cinema (on the basis of Burke's survey, can this be seen as a peculiarly proletarian ritual too?). The Québécois and the Newfoundlanders naturally come last on the list, in that order, with the Albertans not far behind as national slackers, but is there anything here which could not be deduced by any reasonably intelligent observer after a good camping trip across the Dominion? I should think such an expedition would be of vastly more educational value than grinding out sometimes meaningless statistics.

In some Alberta campgrounds, the migrant worker predominates as a social type — one of the reasons for the recent abolition of that fine old populist institution, the free government campsite. Alberta is one of the "small worlds" which has changed radically in its character and status, which forms the immediate backdrop to the publication of the Caldarola collection: 400 oversized pages with a score of articles on various aspects of the provincial experience, written by not less than 23 contributors, 12 political scientists, 4 sociologists, 2 professors of business, 2 historians, 2 civil servants, and a lone economist. Only four of them, incidently, list their residence as east-of-Regina. Evidently writing in and about "Western Canada" is a growth industry; equally so is the impossibility of doing justice or otherwise to such a large anthology in such a short review. Generally speaking the articles are like the petroleum reserve, some light, some heavy, some conventional, others non-conventional, still others quite unproved.

There is evidence here of the same mind-numbing empiricism and banalité which mars the work discussed earlier. I regard such a statement as "Peter Lougheed's mean score of 7.7 on the ten-point scale indicates just how successful this man was in projecting himself to the public in a positive manner" in the context of a supposed analysis of "The Conservative Takeover" (by Elton and Goddard) as just short of an insult. Is it too much to ask that the general degradation of Canadian public life to the level of personal popularity contests not be reflected in academic political commentary?

On the other hand, there is also evidence in the collection that survey research can be used imaginatively and fruitfully. Sweeney and Schenck's article on "Political Attitudes of Middle Business Managers in Alberta," where the sample group is clearly outlined, its relevance explained, and the results sifted through a coherent thesis, immediately comes to mind. These writers carefully examine the phenomenon of modern Toryism in the province through their research, concluding that a "new middle class," not the premier's popularity (and he is hardly the most engaging of politicians), lies at its heart. Skogstad's "Farmers and Farmers' Unions in Alberta" is another case in point: here the survey work has been supplemented by socio-historical research into the changing structure of the agricultural community with good results. This piece, among others, reminds us of the existence of other people in the Wild Rose Country besides the politically hegemonic fractions of the bourgeoisie, with autonomous concerns and problems (in this case, the problem of survival) which are muted by the general din. It is obviously important to note the "end of ideology" in Alberta's farm community, but the struggle continues. Today one hears news reports of "redneck" farm-

ers arming themselves against the encroachments of the Seven Sisters.

There are obvious gaps in this anthology: much discussion of Alberta "alienation" but none at all of real alienation, of native Indians, of the new immigrants, of women, and so forth. Workers and the workplace, urban problems, and other issues of import are conspicuous by their absence. A good deal of plain old "false consciousness" infuses the work, notwithstanding its merits.

Historians will want to read *Society and Politics in Alberta*, but selectively. L.G. Thomas's "The Liberal Party in Alberta, 1905-21" provides a useful synthesis of his old monograph on the subject, long since disappeared off most library shelves. Betke's "The United Farmers of Alberta, 1921-35" does much the same things with the author's excellent Masters' thesis on the topic. Caldarola's "The Social Credit Party, 1935-71," on the other hand, scarcely does justice to its subject, and contributes little to the extensive, but still inadequate, literature on the Socred phenomenon. Myron Johnson's "Failure of the CCF in Alberta: An Accident of History," however, is a real addition to the literature on Prairie social democracy, and is one of those rare works on political history which delves into the thorny question of party organization. Similarly Flanagan's "Ethnic Voting in Alberta Provincial Elections, 1921-75" is unique in its sensitivity towards the local dimensions of the Alberta political tradition.

Readers of this journal will be disappointed by Masson and Blaikie's "Labour Politics in Alberta," a patently token effort at taking into account the province's labour past, and probably the worst of the lot. Professors of politics who describe the "International Workers of the World" [*sic*] as "antieastern" might think of enrolling themselves in Labour Studies 101 next term.

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Donald H. Akenson, ed., *Canadian Papers in Rural History*, Vol. I (Gananoque: Langdale Press 1978), and C.F. Wilson, *A Century of Canadian Grain* (Saskatoon: Western Producer Prairie Books 1978).

THE IMPORTANCE OF agricultural society and the rural community has been emphasized frequently and written about by many notable figures such as Marx, Engels, Lenin, Lukacs, Wallerstein, Thompson, Mousnier, Porchnev, C.B. Macpherson, and others. This element of society has not lost its significance even though governments, like the Canadian, prefer to ignore or at best de-emphasize it. These two books deal with aspects of that society: Akenson's volume contains four articles by economists, geographers, and historians; Wilson's tome deals with government policy to 1951.

According to Akenson, the purpose of the essays is to fill a gap in an area that "traditional historians" are loath, or at least reluctant, to fill. This is a most laudable objective, although many academics specializing in certain aspects of agrarianism or rural society might be rather surprised by this assumption. Does this group of articles fulfill the objective? The piece by R.E. Ankli and R.M. Litt is the most interesting of an uneven collection. They note the problems confronting economists trying to understand the cost and type of economic growth of prairie agriculture. In admirably attempting to reconstruct the financial burdens of agriculturalists, they have perhaps too naïvely accepted a journalist's collection of oral history without critically realizing that time tends to distort memories. K. Kelly's study of eastern Simcoe county and the evolution of the marketing agencies and the competition between centres has fascinating potential which the long chronological narrative almost destroys. J. Little, on the other hand, briefly compares the social and economic development of different races — Scots, English, French — in two Quebec townships. There are

some problems with the consistency in the number of French-Canadian farmers and landholders, but the major difficulty is that the conclusion which attributes the French-Canadian problems to wanting to remain in their country — Quebec — is inconsistent with the whole tenor of the article. K.H. Norrie writes on that delight of academics, the National Policy. He attacks the tariff, the prairie west's sacred cow, and suggests that the federal government policy was in fact beneficial to the prairie farmers. Indeed, he concludes "that there is an important difference between actual out of pocket transfers and a failure to appropriate capital gains when the regional equity aspects of the National Policy are at issue." (29) The argument is buttressed by innumerable assumptions, hypotheses, implications, and estimates; thus the reader is confronted with one of the most glittering productions of an economic scientific "argument;" stripped of the glitter, the result is specious at best.

Wilson, a retired civil servant, in over 1,100 pages, purports to write "such a *review*" (my emphasis) as "to compile a continuous record of the decision-making process, and thereby make the background material readily available for the policy-maker." The review is replete with charts, tables, and appendices, and, as mentioned on the cover, "a wealth of material from original sources." The review contains that, albeit mostly undigested. There is, too, a lack of perspective. The periodization is only one example: 231 pages deal with the beginning to 1930; 258 pages with the period 1930-5; 294 pages for 1935-43; and 260 pages for 1944-51. Does this signify that the federal government had little grain policy in the first 60 years of confederation? If so, why not state it? Another major complaint: the review is defensive. It is written from the vantage point of a civil servant and one is constantly aware of civil service perspectives. There is no attempt to understand the grain growers and their plight. Thus the review's value is limited. Two questions recurred constantly in reading the review: is this an example, thinly disguised, of governmental efficiency? Did the fact that the review was dedicated to Otto Lang condemn it to the oubliettes? There is little for academics in Wilson's review. Grain farmers would have difficulty ploughing through this, but if they did, they might recognize their problem — an insensitive government and bureaucracy.

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Raymond L. Foote, *The Case of Port Hawkesbury: Rapid Industrialisation and the Social Unrest in a Nova Scotia Community* (Toronto: Peter Martin Associates 1979).

THIS BOOK IS THE eighth in the multidisciplinary Canadian Experience Series which offers "inexpensive and readable accounts of life in Canada." (v) It is a useful complement to Matthews' (1976) earlier study of the relocation of Newfoundland outports. Both books examine, through the use of informal interviews and in-depth participant-observer analysis of local communities, the social impact of imposed economic change. Whereas Matthews analyzed the effects of policies which relocated local communities into larger, "more viable" settlements, Foote examines the implication of moving "the work to the workers."

The Strait of Canso region, with its centre Port Hawkesbury, was subjected to substantial economic change during the 1960s and 1970s through development of the ice-free, deep water harbour potential, and the arrival of several large-scale industries. The period of industrialization is divided into two phases: the first involved the establishment of a Swedish-owned pulp mill, and the second, more dramatic period, was highlighted by the construction of an oil refinery, a heavy water plant, a thermal power station, and deep water harbour facilities.

The author introduces the book with chapters on the theory of community and social change together with a detailed description of the methodology used. Compact histories of the Strait of Canso region and the Cape Breton labour movement are offered. Foote goes on to examine the local effects of the two phases of industrialization in Chapters 5-7. The core of the book is formed by analysis of the impact of economic change on employment patterns, decision-making processes, and social stratification in the community of Port Hawkesbury. Foote concludes that the earlier phase of industrial development was successful because the local community was able to control the process of change and that the new developments were absorbed without the destruction of traditional communal relations and value systems. During the later phase of more accelerated industrialization, local community infrastructure was unable to cope. The influx of large numbers of construction workers, the influence of migrant professional classes, and increasing provincial and federal government intervention led to social unrest, divisions within the community, and the loss of domestic decision-making power. The contrast between the two phases parallels Pross's (1975) comparative study of the planning and development processes in Bridgewater and Port Hawkesbury. Both authors make a plea for regional development planning to be sensitive to local cultural conditions, and condemn external interference through the imposition of planned change.

One of the most interesting chapters in Foote's book examines the social unrest which plagued the construction phase of the industrialization process. Periodic wildcat strikes, absenteeism, violence, and alleged closed-shop hiring practices upset the traditional stability of the local community. The author attributes the cause of the unrest to the large-scale influx of previously unemployed miners from the Sydney region, to the historical militancy of these workers, and to the unsuitability of mineworkers' value systems to the nature of construction work in the new growth centre.

The book is a most readable account of the impact of large-scale industry on a small community. It fails, however, despite the goals of the Series, to set these changes within a wider economic and social context, in this case that of regional underdevelopment in Canada. This weakness stems partly from the fact that the research appears to have been completed at least ten years ago, even though the book was published in 1979. The Department of Regional Economic Expansion, for example, is not mentioned until the three-page Epilogue, which is somewhat inexcusable for a study published a decade after the inception of that organization. The restricted outlook is also due to the methodology which stresses very detailed, parochial investigation of endogenous change. The question of why the region might be underdeveloped in the first place is never really addressed.

Recent contributions to theories of uneven regional development suggest that underdeveloped regions play an integral role in the process of economic development. In the period of capitalist development, underdeveloped regions are first created and then exploited. The latter process occurred in the 1960s with the location of large, often multinational, capital in lagging regions to take advantage of one or more local attractions in the form of unemployed labour, lower wage rates, lack of union organization, and state subsidization. It is the local social ramifications of this phase of capitalist development that Foote is describing, though he does not put it in these terms. By failing to examine this context, the author attributes the loss of local decision making to state intervention and not to the nature of modern multinational capital. Foote is forced to argue, therefore, that the apparently easier assimilation of earlier industrialization is due to the absence of external interference rather than to a slower and smaller scale of

change. The less obvious increase of dependence on externally-controlled capital is not examined.

Readers of a more Marxist persuasion will find the methodology unsatisfactory because no class analysis is made. An alternative analysis would examine community change in terms of the penetration of the capitalist mode of production into a peripheral region where pre-capitalist remnants still persist. Much of the challenge to local value systems stems from the implantation of a comprador class, and the incorporation of the working class into a more strict capitalist industrial discipline.

Nevertheless, Foote does offer a readable and useful insight into how industrial change affects the daily lives of local people and makes a good case for examining the sensitivity of local cultural systems. While stressing the importance of local control of political decision making however, he fails to examine the loss of local economic control and avoids evaluation of the consequent dependence. It would be interesting to see a follow-up study now that one of the major industries has fallen victim to the current economic crisis.

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The WPIRG Reader: Case Studies in Underdevelopment (Waterloo: The Waterloo Public Interest Research Group, University of Waterloo 1980).

THIS SHORT BOOKLET presents four public lectures sponsored by the Waterloo Public Interest Research Group in 1979. The unifying theme is the concept of underdevelopment, and each lecture throws light on one part of a world-wide pattern of economic dependency. John Watt gives a useful historical sketch of underdevelopment in Atlantic Canada, describing how the region has been progressively colonized by private capital and the Canadian state. Terrence Downey examines the rise and fall of uranium mining at Elliott Lake: the fortunes of the industry and fate of the community were always beyond local control. Ernie Regehr argues that Canadian military spending not only contributes to a deadly arms race but also aggravates some of Canada's characteristic economic weaknesses. And in a discussion of modern Africa, Jan Jorgenson explains how innovative strategies have enabled multinational corporations to maintain their economic power. In each lecture the author has challenged conventional wisdoms and offered an alternative approach to the topic. Specialists will find little that is new here, but as a modest exercise in popular education *The WPIRG Reader* succeeds in reminding us that "underdevelopment is a global phenomenon, by no means limited to the Third World."

I read these lectures with one large misgiving, however. Too much "radical" political economy is written with an exclusive eye on the baneful influence of "outside" forces. In this kind of analysis local responses often remain an absent category. The lectures in *The WPIRG Reader* share this common weakness. In discussing modern Africa, for instance, it needs to be said that neocolonialism is a response to modern liberation movements. And in the Canadian context, the authors seem to forget that under various names (the latest is "regional disparity") the problem of underdevelopment has been familiar for several generations. The authors might have given us some useful glimpses of how Canadians have responded to this problem. For instance, it may be that one reason Canadians accept a large and costly military sector is that young people in the hinterlands see the armed forces as an attractive road to economic security and social mobility. Also, it seems clear that the prominence of the state in dependent regions is in part the outcome of attempts to gain greater control over local industries and resources; for instance, the history of Sydney Steel since Black Friday in 1967 has been one of considerable frustration, yet the achievement of public ownership

was the outcome of a long struggle on the part of the steelworkers and their community. Unfortunately, the authors of *The WPIRG Reader* missed the opportunity to provide a more difficult but also a more optimistic analysis.

David Frank
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J. Donald Wilson and David C. Jones, eds., *Schooling and Society in 20th Century British Columbia* (Calgary: Detselig Enterprises 1980).

SINCE THE LATE 1960s, the history of education has proven to be one of the richest fields of critical inquiry. In the context of new concern for theory and method, historians found a wide variety of subjects which had been ignored or inadequately addressed: school attendance patterns, educational ideology, and literacy, to name only a few. Much of the best work was produced by graduate students who came to educational history with a sense of excitement and challenge. As a result, a series of excellent masters and doctoral theses laid the foundation for a new understanding of today's most important institution.

The focus of research was initially south central Upper Canada/Ontario but in recent years historians have turned their attention to the Prairies and Western Canada. As earlier, graduate students are contributing significantly to this development. *Schooling and Society* is a well-produced collection of six essays based on masters and doctoral research done at the University of British Columbia. The topics of these essays range quite widely over the history of schooling in the province. Timothy A. Dunn addresses the public school expansion of 1900-1929 while David C. Jones relates educational thought to the ideology of land settlement during the same period. The careers of G.M. Weir and H.B. King are considered by Jean Mann who raises questions about the nature of progressivism and its meaning for schooling in British Columbia between the wars. Three other essays focus on specific topics: Diane L. Matters examines the Boys' Industrial School; Jean Barman analyses the Vernon Preparatory School; and Gillian Weiss explores the debates surrounding the establishment of kindergartens in various areas of the province. In addition to these essays, the volume also includes a very useful bibliography prepared by Frances M. Woodward and a stimulating introductory essay by J. Donald Wilson.

The essays are generally written with a good deal of pioneering fervour and the tone of the collection is set by Wilson's introduction. Wilson sees a developmental process at work in the recent writing of Canadian educational history. Over the past 15 years, Wilson perceives a three-stage progression in which the traditional perspectives of those such as F. Henry Johnson were first replaced by Wilson, Robert M. Stamp, and Louis-Phillippe Audet and then by the radical revisionism of Michael Katz and his students. The *Schooling and Society* essays generally reflect the Katz influence and some authors attempt to advance this context, most notably Jones' emphasis on the "myth of the land." However, none of the essays offer any critique of sources or the kind of systematic analysis germane to recent social historical writing. The traditional focus on educators and policymakers rather than on teachers and students is also apparent. In the future, historians will undoubtedly study the women and children who actually lived British Columbia's educational history, and as this occurs, pioneering fervour will indeed be justified. The general strength of the collection is in its contribution to the history of ideas. All the essays attempt to establish a broader theoretical context for these ideas and it is this fact which supports the claim of innovation.

The one unfortunate aspect of the otherwise exceedingly helpful introduction is Wilson's surprising underestimation of the role which educational historians have played in advancing "mainstream" histori-

cal thought during the past 15 years. Wilson suggests that, in recent years, educational historians have been struggling to catch up with other Canadian scholars. In fact, it was educational historians who were often responsible for drawing attention to the importance of topics such as the history of ethnicity, labour, and women, and, thereby, it was they who breathed new life into a sometimes moribund discipline. It is simply not accurate to suggest that educational historians had to learn theory and methodology from their mainstream counterparts; rather, the reverse was generally true. Similarly, Wilson seriously downplays the impact which research on the educational history of Canada has had internationally and he is unnecessarily apologetic about accomplishments to date. While *Schooling and Society* is in many ways only a tentative first step in terms of the educational history of British Columbia, its general focus and analyses immediately carry it near the forefront of current historical thinking about the province.

Chad Gaffield
University of Victoria

F.T. Denton, A.L. Robb, and B.G. Spencer, *Unemployment and Labour Force Behaviour of Young People: Evidence from Canada and Ontario* (Toronto: University of Toronto Press for the Ontario Economic Council 1980); and P. Osterman, *Getting Started: The Youth Labor Market* (Cambridge, Mass.: MIT Press 1980).

THESE TWO STUDIES are an interesting contrast in methodologies that are typical of labour market research. In the tradition of *dual and segmented labour market* analysis, Osterman utilizes data from interviews of employers and younger (usually minority) workers in the Boston labour market, as well as some national data sets such as the National Longitudinal Survey of Young Men. In addition, in an attempt to place the issue in a broader and more historical perspective he examines the historical and sociological literature on young workers. His chapter 4 on the evaluation of the youth labour market is one of the most interesting in the book. There Osterman argues persuasively that the youth labour market emerged as a distinct labour market in the early 1900s, and that this transformation resulted from increased enrollment in schools, a reduction in the demand for unskilled young workers, and an increase in the supply of other unskilled workers notably from immigration and from the pool of unemployed during the depression. Denton, Robb, and Spencer (hereafter DRS), on the other hand, apply econometric techniques to existing data sets, (mainly for Ontario, and where that is not available, for Canada), their objective being "not to explain unemployment among young people but to describe it in helpful ways." (5) Their emphasis is on finding empirical regularities in the data, not on establishing cause and effect or investigating the historical origins and evolution of the youth labour market.

Both studies provide substantial original empirical work and not just a review of existing literature or a description of published data tabulations. Both utilize econometric techniques that may be difficult for some readers. (Osterman uses logit analysis and simultaneous equation estimates as well as conventional regression analyses; DRS utilize logit analysis, spline functions, and transitional probabilities as well as conventional regression analysis). This raises the question of whether such material is not more appropriate for refereed academic journals where it would reach a wider audience and be subject to more scrutiny. My preference would be to have this material written as journal articles, or at least as self-contained appendices or sections in the text, and the results discussed in a non-technical fashion in the text itself.

In spite of the different methodologies, data sets, and countries analysed, the two studies provide a remarkably similar picture of the youth labour market. Both

studies indicate that the problem of youth unemployment is not unique to North America, nor to the 1960s and 1970s. Historically and internationally youth unemployment rates have tended to be higher than adult unemployment rates. They emphasize that the youth labour market is a very dynamic market with youths constantly changing jobs and moving in and out of the labour force and unemployment. For *most* youths, unemployment tends to be of short duration and it reflects a rational search for a suitable job or a rational transition between school and eventual longterm commitments to career and family. As emphasized by Osterman, however, for a small number of youths (notably blacks in the United States) unemployment is not a temporary and transitional experience: these young people bear a disproportionate burden of youth unemployment and this may have serious long-term effects on their labour market behaviour.

Both studies also emphasize that just as with unemployment in all age groups, there is no unique cause of youth unemployment nor a consensus in the literature concerning the relative importance of various contributing factors. Insufficient aggregate demand is a contributing factor, and is emphasized by Osterman as the single most important factor for the high unemployment of black youths who are last in the hiring queue. The large supply influx of baby-boom youths who entered the labour market in the 1960s and 1970s (competing with the influx of women, especially married women) certainly contributed. Both studies tend to minimize the importance of minimum wage legislation which the economics literature suggests can have a disproportionate adverse employment effect on youths. The availability of unemployment insurance, especially in Canada where the benefits are fairly high, may also enable some young people to remain unemployed longer than they otherwise would; nevertheless, neither the existing empirical literature nor the econometric results of DRS indicate this to be a major determinant of high youth unemployment.

Both studies find that the high youth unemployment rate appears to prevail mainly because of the transitional nature of employment in the youth labour market, as younger workers engage in job search and enter the transition between school and permanent employment, and as employers fill the temporary jobs with younger workers. Though insufficient aggregate demand, the minimum wage, and unemployment insurance may have contributed somewhat to a worsening of youth unemployment in the 1960s and 1970s, the youth unemployment rate would still have been high relative to the adult rate due to its higher transitional component.

Neither study was intended to provide a comprehensive treatment of youth unemployment and the youth labour market; hence, they do not provide a comprehensive review of the literature nor even a comprehensive bibliography. Important issues that are not given full treatment include the relationship of youth unemployment to female unemployment, the impact of public policies, the substitutability of younger workers for other workers, and the extent to which an early period of unemployment has a permanent effect on the labour market behaviour of young workers (although Osterman provides some evidence on this point suggesting that the permanent effect is not serious for teenagers but it is serious for older youths, especially blacks).

While neither study is a comprehensive analysis of the youth labour market, both are invaluable inputs into our understanding of that market. They are especially useful because they illustrate that similar conclusions emerge from the quite different methodologies that are employed in the two books.

Morley Gunderson
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Frank R. Anton, *Worker Participation: Prescription for Industrial Change* (Calgary: Detselig 1980).

WORKER PARTICIPATION is one of the more useful additions to the growing number of books and articles dealing with the subject. The increase in literary output is matched by the growing interest among employers, governments, and to a lesser extent, unions. Worker and union participation in the management of workplaces and union involvement in various bipartite and tripartite bodies at the provincial and national level raises profound political questions, that go beyond the narrow confines of industrial relations.

Given the political nature of the subject, the reader should be clear about the assumptions and aims of the author which are stated frequently throughout the book. Anton begins with the unsatisfactory state of labour-management relations in Canada citing the fact that Canada has had the second worst work-stoppage record among industrial nations during the past decade. Increased cooperation between labour and employers is seen as essential. If it is not arrived at voluntarily, Anton believes that it will be imposed by governments. While the author sees the need for government initiatives, he clearly favours *limited* government intervention with the emphasis on voluntary bi-partite arrangements between labour and capital, in contrast to more massive government intervention through extensive legislation and tri-partite bodies. The overriding concern of the author is to preserve the free enterprise system which, in his view, can only be achieved through worker participation in decision-making coupled with a redistribution of power in favour of employees and unions.

Anton uses evidence from West Germany, the Netherlands, the United Kingdom, and the European Economic Community to support his advocacy of worker participation in Canada. His particular interest in the West German system of co-determination is understandable given the impressive economic performance of that country and the declared policy of the German Trade Union Federation (DGB) to support the capitalist system. It is unfortunate that the author has excluded Sweden and Norway from his survey. The inclusion of these two countries would allow the reader to evaluate both bi-partite and tri-partite decision-making and to explore options beyond the free enterprise model since the capitalist/socialist debate is still very much alive in both countries. While the author's approach to the subject does not depart from previously published material, the presentation is well organized and easy to read. The aims of participation are characterized as ethical/psychological (development of personality, job satisfaction, reduced alienation), political/social (extension of democratic principle to the economic and industrial sphere), and economic (more effective utilization of human resources leading to increased efficiency and profitability).

The forms of worker participation are separated into direct and indirect participation with considerable emphasis given to the latter which includes collective bargaining, works councils, and worker representation on enterprise management and supervisory boards. An important requirement, according to Anton, must be the satisfaction of perceived needs of labour as well as those of management. Since most innovations in direct participation, such as Quality of Working Life (QWL), are in fact initiated by management we need not worry too much about the perceived needs of employers being jeopardized. Indirect participation, such as the proposed tripartite National Council for Social and Economic Planning or the bi-partite major projects task force (the "Blair-Carr Task Force"), tend to be initiated by business or governments although the CLC's *Labour Manifesto for Canada* comes to mind as an exception to this rule!

One of the situational factors discussed by the author would seem to be of particular relevance to Canada. The participatory

potential may be affected by the extent to which an enterprise is free to make managerial decisions. Multinational enterprises, which dominate the Canadian economic scene, may well be restricted in their decision-making on issues of importance to either direct or indirect participation. Evidence from Norway would tend to support this view. One of the premises of the author, however, must be challenged. Anton argues that the introduction of worker participation in Canada will result in a redistribution of power in favour of labour. If we restrict ourselves to a strictly formalistic interpretation of participatory innovations, it is possible to argue that decisions previously made solely by the employer must now be shared with labour. The political reality as well as the evidence from Europe, however, does not support such a claim. The case of Sweden is of interest here. In order to advance their goals of worker participation, organized labour in that country felt compelled to press for supportive legislation to curtail the prerogatives of management. Even the West German system of co-determination, apparently favoured by the author, is firmly anchored in national legislation.

Advocates of worker participation in Canada will find this book useful. The material is well researched and persuasive for those sharing the basic assumptions and aims of the author. The discussion of obstacles to worker participation in Canada, particularly those related to the introduction of worker representatives to company boards, will be of interest to "progressive" employers and interested unions.

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Robert Comeau, Bernard Dionne, *Les communistes au Québec 1936-1956* (Montréal: Les presses de l'unité 1980).

THIS IS THE second book on communism in Quebec in less than a year, and judging by the theses referred to in both books, there will probably be at least a few more in the near future. Like Marcel Fournier's *Communisme et Anticommunisme 1920-1950*, this publication is sympathetic but critical; its major weakness is that it devotes too little attention to the way in which the Communist Party handled the national question.

Most of the book is devoted to the activities of the Quebec section of the CP which were identical to the activities of all the other provincial committees across the country, except for the fact that important documents issued in Toronto at the Party's national office, or even by the provincial committee in Montreal, had to be translated into French. In other words, while it had to use the French language in Quebec from time to time, the politics of the Quebec party were definitely not French. Neither was the leadership. Even though French Quebeckers did hold important positions, at least in the titular sense, they were always outvoted and dominated by the Anglophones in the Quebec organization and, of course, by the national leadership in Toronto. On all vital questions of interpretation, it was the English view that won out without exception. Every new turn in the Party's line on French Canada was created by the English comrades, often against strong disagreement from the French. Comeau and Dionne say as much in the issues which they deal with, but they deal with only a few of them, and thus we are denied a more intensive examination of what is really the key question about the communists in Quebec.

The main issue they discuss is the Party's position on the national question during the period after 1941 when, as a result of the Soviet Union's participation in the war, the CP of Canada changed from opposing the war to supporting it. In the period from 1939 to 1941 the CP in Quebec made common cause with the most extreme French Canadian nationalists in opposition to the war, but this is dealt with very briefly. In supporting the war the Quebec party had to spend a considerable effort to denounce these same nationalists

who continued to oppose the war and especially to oppose conscription. The party's theoretical position had to be altered to fit in with this new task. Its publications now stressed that while Quebec was a nation, it had already won its main political demand, the right of self-determination. What remained was to win economic equality with the English!

After the war, the Party took a stronger line on the right of self-determination, but immediately got involved in a bitter debate with its French members over two basic questions: the Francophones demanded more autonomy for the Quebec section of the Party, and opposed Buck's position that the Party should demand that social policy be taken from the provinces and given to the federal government. The Party leadership in Quebec and in Toronto decided that the position taken by the French comrades was "bourgeois nationalism" and expelled or pushed out almost the entire French section. This happened at the 1947 provincial convention, and the fact that the Party had lost at one blow practically its entire French membership, did not upset the leadership at all. Instead, it issued several ultimatums, stating that each individual would have to publicly declare that he or she was wrong and the party correct, in order to be re-admitted to the ranks.

The authors cite a few other examples where the Party reversed its positions, even while denying that it was changing. The major example given is the period of the war when the Party adopted the "Browder line" which supported the position of the leader of the CPUSA that the post-war world would be one of partnership and harmony between the Soviet Union and the major imperialists who had become allies against Germany. But the authors make the mistake of accepting the position of the then leader of the B.C. section, Fergus McKean, who accused the party of going against Stalin by advocating this line. Here is another place in which the authors are too skimpy in their research. The major documents of this period such as Joseph Starobin's *American Communism in Crisis, 1943-1957* and Fernando Claudin's *The Communist Movement from Comintern to Cominform*, and many others, show that Browder and Buck took this line directly from Stalin. The mistake that Browder made was not changing after the war, even though this was no longer the position of the Soviet Union.

The last section of the book deals with the crisis that shook the party in 1956-57, after the revelations about Stalin made at the 20th Congress of the Communist Party of the Soviet Union and the invasion of Hungary by the Soviet Army in October 1956. It is regrettable that the authors did not examine or refer to the wide ranging debate that took place at the time, for in fact this is the one instance in the Party's history, that all points of view were able to get published in the party press. This was due to the fact that almost from the beginning of the debate to the Convention of 1957, the dissidents had a majority on the National Executive Committee. Instead of giving us the main lines of the debate, the authors limit themselves to brief interviews with some of the dissidents, whose views, as given, are incorrect or incomplete.

The authors have wisely made no attempt to find an overall explanation for the failure of the Party to establish itself among French-Canadian workers. Several explanations by others are presented, but none are endorsed. The book gives a fair account of both the positive and negative sides to the efforts of the Party, but does not make the mistake of suggesting that without the errors, the Communist Party might have become a mass organization in Quebec and elsewhere in Canada.

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Alice Hanson Jones, *Wealth of a Nation to Be: The American Colonies on the Eve of*

the Revolution (New York: Columbia University Press 1980).

DURING THE PAST DECADE, cliometricians have done much to illuminate dark corners of the past. By sensitively applying statistical methodology, such historians have been able to tell us a good deal about topics which previously were either unexplored or approached imprecisely. Their contributions have been especially noteworthy in the study of the American colonial era. Scholars such as Kenneth Lockridge, Russell Menard, Philip Greven, and John McCusker have utilized techniques drawn from the social sciences to enrich our understanding of the period.

But cliometrics by themselves do not necessarily enlighten. Indeed, when used without adequate concern for historical reality, these techniques can often obscure as much as they reveal. The obvious case in point is *Time on the Cross*, but Fogel and Engerman's study of slavery is not alone in this category. The use of statistics and modelling does not absolve the historian of the responsibility for being sensitive to the particular conditions in the period under study.

By and large, Alice Hanson Jones falls into the category of those cliometricians who have not done disservice to the past. The publication of *American Colonial Wealth*, a three-volume collection of documents based upon her Chicago PhD dissertation, immediately thrust her to the forefront of economic historians of the colonial period. In that work, Dr. Jones dealt not only with a number of basic questions about wealth holding patterns in the immediate pre-Revolutionary period but also posed a host of questions for further analysis. *Wealth of a Nation to Be* deals brilliantly with most of these.

The core of this new volume is based, as was her earlier work, upon a detailed study of the wealth holding of 919 individuals resident in 21 sample counties throughout the thirteen colonies. Probates were the most important source, but where such records were not complete, particularly in the South, the author supplemented them with a wide variety of other primary sources. Dr. Jones hoped to be able to draw what is known as a "self-weighting sample," but when this proved impossible for a number of technical reasons, she designed a series of rather ingenious controls to ensure that her picture of wealth holding was as accurate as possible. For example, she needed to take into account the wealth holding of the large mass of individuals whose estates were never probated, as well as controlling for bias introduced by the fact that individuals were generally wealthier at later stages in their lives.

The results are seminal. Dr. Jones has been able to estimate that the total physical wealth of the thirteen colonies in 1774 was approximately £88 million sterling, excluding the value of slaves and servants, or about £37.4 sterling for every man, woman, and child, whether free or slave. She has also been able to demonstrate that wealth holding was not equally distributed regionally. With slaves excluded, the Middle Colonies were the wealthiest on a per capita basis; when slaves were included, the South was by far the wealthiest region. By any standard, however, New England emerged as the least wealthy region. There also was inequality between urban and rural locations. In the South, rural residents tended to be wealthier than the urban compatriots, but north of Virginia, the situation was generally reversed. Dr. Jones was also able to determine discrepancies based upon occupation, although the situation in this regard appeared to be relatively fluid.

All of these pieces of information are of great importance in understanding the colonies on the eve of the Revolution. Yet there are several problems with the book. First of all, by applying a cross-sectional analysis (analyzing wealth holding in only one year), Dr. Jones failed to give us a sense of change over time. This is rectified partially by comparison with studies of the post-Revolutionary period, but it is less

satisfactory for the colonial period, where her study is the first of its type. Had she concerned herself more with change over time, she might have been able to throw greater light on the continuing controversy over the nature of the Revolution. Were Americans on the eve of Independence growing more or less wealthy? Did wealth-holding patterns exhibit any evidence of an economic "crisis," particularly among merchants and planters? Were the urban poor becoming increasingly impoverished? These all would have been important questions, but the work will have to be done by others.

The nature of Dr. Jones' sample also leads to a bit of dissatisfaction. She chose to stratify her sample by county, which allows her to generalize about wealth holding on a colony to colony basis. Unfortunately, her generalizations are less secure for other variables, such as occupation. Without an adequate census from which to build in weighting schemes, the author's conclusions about the patterns of wealth for various occupational groupings are not on very firm ground, since there is no evidence that her sample adequately measured average values of wealth for the various groups in society.

One final warning. Although Dr. Jones clearly displays the sensitivity of an historian, she presents her results in the manner of a social scientist, taking her readers step by step through the analytic process. While this is fascinating, it is also likely to be disturbing for many historians. Not until the final summary chapter does the author pull all the strands together to paint a global picture of wealth holding in the colonies. Unfortunately, there are many who are not likely to persevere to that point, largely because the author's prose is often a chore to read, and there is little evidence to suggest that her editors have done much to help. This is a pity, because the author has much to say both to specialists and general readers interested in the period. A careful reading of the volume will more than repay the student, but this does not preclude the wish that this important work had been presented more felicitously.

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Allen Pred, *Urban Growth and City-Systems in the United States, 1840-1860* (Cambridge: Harvard University Press 1980).

How CITIES ARE organized into systems and how they pull together regions have engaged geographers, economic historians, and historians for some decades. Best known to Canadians is the Laurentian metropolitan model of Innis, Creighton, and Careless strongly connected to export staples. Recently James Simmons has been working toward an evolving model for Canada based on quantitative data. As reviewed by Pred (3), several approaches to U.S. nineteenth-century city systems have emerged in recent years. Although Pred finds wanting idiosyncratic and abstract views, it is clear after four books that he himself is still going around in circles with the same feedback model as he developed in his first book of 1966. He does, however, contribute new insights based on new empirical data, and further, he is now more cautious in attempting to deal with four questions: "How simple or complex were the economic interdependencies of major centers within the U.S. system of cities and its various regional city systems during the 1840-1860 period? How did the economic interdependencies of that period interact with the feedback processes that apparently affected population growth within major centers and other individual cities? How can those same interdependencies be related to the ongoing processes of national and regional city-system growth and development? Why was major-center rank stability solidified or maintained? . . ."

Considering the last question first, Pred continues to stress the stability of the system or, as he put it in 1966, the "initial advantage" of places. Against his critics

who suggest somewhat more variability, Pred criticizes the U.S. census definitions of urban as running counter to his view. While the period under discussion is still seen as that of the most rapid urban growth in U.S. history (compared to 1891-1913 in Canada), he argues that the census skews the data by admitting only places of 2,500 to the urban category. Hence, far too many "new" places are actually old, and in the aggregate appear to contribute more to urban growth than do larger places. Through his analysis, Pred argues that larger cities attracted relatively more immigrants and generated more employment than what appears on the surface.

Stability and the strength of large places is also stressed through an empirical discussion of interdependencies among places. Complexity emerges from this view, more so than in the 1790-1840 period (as discussed in his previous book) but less than in the post-1860 era of corporate industrialization. From this discussion he develops a typology of five "interdependence dyads" between large, intermediate and small places regionally and interregionally, and then his oft-described feedback probabilistic model.

Spatial biases result from uneven regional development and skewing within the urban system. The final discussion deals with "specialized economic information" through newspapers, postal services, business travel (by rail then coming into its own), and telegraph. New York and other large places enhanced their control. This is an interesting chapter stressing the speeding up of flows.

Because of his behavioural language and perspective, Pred does not discuss or sidesteps a number of issues. First, although he touches on capital flows he pleads that another kind of book would be necessary. Perhaps instead of refining the model for yet a fourth time, he might well have taken this tack. This might have led him also to consider political power and even labour. Further, empirically, as before, London and British economic power are hardly mentioned. British capital invested heavily in American technology as it had through "credit" in the colonial era. In all his works pre-1790 developments are not discussed. Third, telcology creeps in. The 1790-1840 city system was in an "infant" state, and therefore it seems not as complex as between 1840 and 1860 and later. Even if one were to accept (wrongly) the biological analogy, infants are hardly simple! Throughout there is not much sense of people acting here.

But to conclude on an upbeat note. His 1973 volume on 1790-1840 offered gratuitous advice to the third world, and dismissed the empirical historical geography to promote his "time-geography." This time around he not only uses the work of historical geographers, he concludes skeptically and cautiously that "the state of knowledge concerning . . . urban growth (and decline) is still much too rudimentary for scholars to accept unquestioningly either my model formulations or those of anyone else." (171)

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Alfred D. Chandler, Jr. and Herman Daems, eds., *Managerial Hierarchies, Comparative Perspectives on the Rise of the Modern Industrial Enterprise* (Cambridge, Mass.: Harvard University Press 1980).

WHY DID the managerial hierarchies of modern corporate enterprise arise when and in the way that they did in the United States, and how and why did their evolution diverge and converge with corporate growth in Great Britain, Germany, and France? Why, for example, did large-scale managerial enterprise dominate major sectors of the American economy before World War I but not become a major force in the British economy until the 1920s and 1930s, and not until after World War II in the case of France? And why did the shift from market to managerial mechanisms of coordination occur in some particular

industries in a country and not in others: in mass-produced machinery in the United States, but not in cotton textiles? And why in a particular industry in one country but not in the same industry in another country: in food-processing in Britain, but not in France or Germany? And how is one to weigh such diverse factors as market size, energy and transportation technologies, social attitudes, and legal institutions in explaining the results? These are the kinds of questions that guide this ambitious foray into comparative institutional history.

Because the essayists take as their point of departure Alfred D. Chandler's findings for the United States, the collection possesses a degree of conceptual clarity and intellectual coherence unusual for a collective enterprise of this sort. Emphasis upon the Chandlerian framework will disappoint some students of labour history to be sure. It focusses upon business management, not factory management; on why and how managers manage other managers, not how they manage their labour force. Chandler studies the organization of the firm, not the organization of production technology, to repeat a distinction that Oliver Williamson draws in a thoughtful review of *The Visible Hand* (1977) included in the volume. Following this tradition, the volume's six other contributors omit any reference to labour's role in the origins of modern enterprise, or in the spread of those managerial techniques designed to recruit, train, and control an industrial work force. The seminal nature of Chandler's work on business organization is not in doubt, however, and this volume joins *Evolution of International Management Structures* (1975), edited by Harold F. Williamson, as an example of his spreading influence.

In the opening chapter, Chandler summarizes the views he has developed at length in *Visible Hand* and elsewhere. He argues that the multiunit, multifunctional, managerially-coordinated firm emerged in the United States when technological breakthroughs and market growth increased economic activity to a speed and volume that superseded market mechanisms of coordination. When volume was small and marketing requirements fairly simple, manufacturers had few incentives to integrate distribution with production. Coal energy and the railroads and telegraph transformed this situation in the mid to late nineteenth century. Older organizational forms persisted in such labour-intensive industries as wood, cloth, leather, and specialized goods, but entrepreneurs in new-product industries integrated production and distribution processes and established hierarchical levels of salaried managers to co-ordinate the flow and allocate expanding resources. Once in place, these managerial hierarchies became their own sources of economic growth. Mangers, determined to keep their organizations profitably employed, increased the speed and volume of their activities, absorbed more business units, and internalized more and more market decisions. Chandler refers in passing to the cultural and legal factors that might account for European contrasts with his model, a theme which Morton Keller explores in a chapter on the regulatory response to large-scale enterprise in each country. But technology and markets, not cultural patterns, and certainly not entrepreneurial personality, are the central determinants in Chandler's formulation.

It is not possible to summarize all the data and hypotheses that Leslie Hanna, Jürgen Kocka, and Maurice Lévy-Leboyer introduce in their essays on Great Britain, Germany, and France respectively, but the greater persistance of marketing middlemen in Europe as compared to the United States is certainly worth noting. Leslie Hannah argues, for example, that a "highly developed and efficient British system of markets for commodities, skills and distributive and financial services, as well as for final products" (63-4) obviated the need for internal management hierarchies in that country, despite the acceleration of economic flows occasioned by the

application of coal and steam to industrial processes and by the growth of a railway network. In other words, the conditions that Chandler argues necessitated vertical integration in the United States did not produce that outcome in Great Britain. Lévy-Leboyer also finds for France that merchant control over wholesaling impeded forward integration. In a concluding essay, Herbert Daems argues that such national differences in marketing makes an understanding of when, where, and how marketing mechanisms became hierarchically integrated with production a key question for future comparative study.

Managerial hierarchies emerged at different times in different economies, but when they did appear, these authors argue, they clustered in the most technologically advanced industries in all four countries. The American and German cases are most comparable both in timing and in large-scale concentration in chemicals and electrical machinery. Kocka finds that the distribution of the largest manufacturing firms among industrial groups was similar in both countries before World War I, a finding that suggests that technological factors predominated over legal differences as a force for integration. Even when German managers had the option legally to choose a cartel form of coordination, in contrast to their American counterparts who nervously eyed anti-trust laws, they chose to integrate.

The evidence is hardly definitive for the wide variety of comparative questions that Alfred Chandler's work in institutional economic history has stimulated, but *Managerial Hierarchies* suggests both the force of the questions and the important implications of the accumulating results.

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Philip S. Foner, *History of the Labor Movement in the United States, Volume V, The AFL in the Progressive Era, 1910-1915* (New York: International Publishers 1980).

THE FIFTH VOLUME of Philip S. Foner's *History of Labor in the United States* follows the same style as its predecessors. Written from a traditional left-wing perspective, it contrasts with most general surveys of the subject including the "progressive" J.R. Commons' edited volumes — the only other multi-volume history of American labour. But, like Commons, Foner writes "old" labour history. He is concerned with central leadership and with nationally significant strikes. Readers interested in ideas about working-class culture or in new methods of statistical analysis will have to look elsewhere. Still, even if there is nothing particularly novel in Foner's account, one is constantly impressed by the wide range of his sources and by his ability to compress them into a well written survey. The momentous and often horrific events he describes lose nothing in the retelling.

The years covered by this volume, 1910 to 1915, seem rather arbitrarily chosen. In discussing labour's relations with "progressive" movements, for instance, (in some ways the best part of the book), Foner reaches back to the turn of the century. Meanwhile, vital subjects like "scientific management" and "relations between the AFL and the IWW," which are essential to an understanding of the period, appear in other volumes. It is therefore difficult to treat this volume in isolation from others in the series.

Still, a pattern of sorts does emerge. The work opens with the highly dramatic McNamara case, in which the lawyers defending the two brothers accused of perpetrating the Los Angeles Times building explosion tried to use the trial to bring about a grand reconciliation of labour and capital through confession by the accused. Instead of forgiveness all round, as they

had planned, the result was a harsh sentence. The story describes the period in microcosm. Thus, in the first half of the volume, Foner explores the AFL's attempts to create a more just society through participation in progressive politics, and the rejection of independent labour or socialist politics that was apparently favoured by an increasing number of the rank and file. Success for the AFL leaders seemed to be at hand with the election of Woodrow Wilson, his appointment of a trade unionist as Secretary of Labor, and his passage of the Clayton Act which was supposed to free labour from the horrendous power of court injunctions based on anti-trust legislation. Samuel Gompers considered it a noble victory — labour's Magna Carta. But it was all a sham. As the courts were soon to show, the Clayton Act gave labour nothing. The efforts to achieve fairness through class collaboration proved futile. Meanwhile, the harsh realities of American industrial life were evident in the momentous strikes of the period. These "revolts," which Foner examines in the second half of the book, comprised the General Strike in Philadelphia in 1910, the shopmen's strike on the Harriman Railroad system 1911-15, miners' strikes in West Virginia 1912-13, Colorado 1913-14, and the Michigan copper belt 1913, and finally the garment workers' strikes 1909-14. What makes these upheavals most significant was that they took place within the orbit of the AFL — not the radical IWW — at a time when its leaders followed a policy of accommodation.

Though Foner tells a good story with a certain dramatic flair, he fails to draw any overall conclusions about the period. In particular, he avoids being explicit about one vital question — namely the true state of workers' consciousness and the effect, if any, that it had on developments. The one place in which he raised the question, in his brief introduction, can be taken two ways. On the one hand, he tells us that these years witnessed a "transformation of workers' consciousness," which seems to say quite a lot, but on the other hand his remark that the struggles merely "made it clear that the U.S. ruling class had failed to extend completely its hegemony over the working class," seems curiously mild. It is tantamount to saying that capitalist hegemony was almost complete.

It is worth considering the range of possibilities about worker consciousness. The phrase "transformation of workers' consciousness" comes from David Montgomery's seminal article in the *Journal of Social History* (1974) dealing with some of the same strikes as Foner. In using this phrase, Montgomery is obviously referring to class consciousness but what has forced it upon the workers, in his view, is the increasing loss of "control" over the work place as a result of the rise of scientific management. The result, according to Montgomery, was a series of strikes including the one by the machinists on the Harriman system and the series by immigrant semi-skilled workers in the garment trades. Although within the orbit of the AFL, these strikes really constituted a "syndicalist" upheaval, that is, their ultimate goal was control. On the other hand, the other possibility that Foner suggests, namely that these strikes are but small gaps in an otherwise complete capitalist hegemony over the workers, conjures visions of a society in which class consciousness, whether it existed or not, had little effect on American developments. Both of these schools of thought, it need scarcely be said, differ from that of the Commons' school.

With such vastly different interpretations open to us, and with such little specific guidance from the author as to where he stands, it is interesting to ask whether his narrative provides any clues. Here, I believe, two salient points are made.

First, in reading Foner's work one is constantly impressed by the formidable physical power of American capitalist power and state power. Both the law itself and law enforcers scarcely ever failed to act against organized labour, and strikes were usually defeated. It is the powerless-

ness of labour that is the most salient feature of American life.

But if capitalists waged the class struggle unrelentingly, this is not necessarily a guide to labour's actions. In fact, it is the limited nature of labour's demands that emerges in Foner's book. Organized labour at all levels associated itself with "progressivism" in its early stages and so showed a desire to participate in American politics. Even if there was a later drift to independent politics and to social democracy, this was still a means of ameliorating the system rather than of challenging it completely. At least, so it seemed to the extreme left at the time. As for the major "revolts" in industry, it is true that the prevalence of sympathy strikes shows a high degree of class consciousness. Nevertheless, what strikers persistently demanded was union recognition as a prelude to bargaining. Foner presents little evidence that the hard knocks labour received, in the pursuit of these limited demands, led to anything more revolutionary. It is thus ironic that the author who for long was regarded as *the* Marxist historian of the American labour movement, when compared with the new Marxist historians of the Montgomery school, should now appear to be so much closer to the Commons school he so long challenged.

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John W. Briggs, *An Italian Passage: Immigrants to Three American Cities, 1890-1930* (New Haven: Yale University Press 1978).

IN RECENT YEARS social historians have turned to the North American immigrant in a manner reminiscent of ward heelers, Protestant missionaries, and reformers in the Progressive Era. They use the immigrant to achieve specific goals pertinent to their research, and in their eagerness to abstract certain aspects of the immigrant experience, many social historians neglect issues more central to the immigrant's life. Students of labour history, modernization, woman's and family history, assimilation and social mobility have all been guilty of selecting elements of the immigrant experience without exploring their subject's complete universe — his/her socio-economy, social contacts, ethnicity, world view, and aspirations. Against this backdrop, it is refreshing to read a book such as John W. Briggs's *An Italian Passage* on Italian immigrants in Utica, Rochester, and Kansas City, a volume which makes the immigrant, rather than the union member, the mobile individual, or the mother, the central focus of its study.

Briggs adheres to Timothy Smith's admonition that the immigrant must be examined on both sides of the ocean, in the pre-industrial Old World setting and in the North American industrial, urban centre. Thus, the first section of the monograph examines the world the *contadini* left behind. In the early chapters as well as in the ensuing pages, Briggs presents the reader with an impressive array of information and data — among them, destination points on the passport application forms of a Sicilian town, membership statistics of mutual aid societies in a number of southern Italian communities, and rates of endogamy among immigrants in Utica from common provinces and regions in Italy. Yet, despite the enormous research, this volume is marred by questionable premises, occasional contradictions, and at times by data which say little about the immigrant experience and which do not clarify Brigg's main themes.

Central to Brigg's study is the thesis that Italian immigrants to American urban centres were not passive, apathetic individuals shaped by "external forces" but rather "active agents, capable of initiative as well as accommodation, and . . . possessors of a viable culture which shaped their perceptions of their American experience, in many ways giving them a sense of continuity between the past and the present." (272) This perspective, although

rare, is not novel and indeed was central to Rudolph Vecoli's work of the early 1960s on the Italians in Chicago. What is off-putting about Briggs's approach, though, are his gauges of the "viable culture" of the *contadini*. He devotes two chapters to a survey of formal education and mutual aid societies in southern Italy and two parallel chapters on those institutions among Italians in Utica, Rochester, and Kansas City. Briggs' purpose is to demonstrate that these people were not aimless illiterates, incapable of cooperation or void of any sense of future orientation. I question, however, the validity of demonstrating a general desire for formal education among a people to indicate a viable culture or future orientation. Is Briggs not employing American middle-class values to do this? Certainly there are other values, institutions, and social contacts — especially informal ones — more intrinsic to the culture of the late nineteenth-century *contadino*.

Another problem with Brigg's presentation is that we are not certain about how important or relevant formal education truly was to the average southern Italian peasant. We have virtually no statistics for specific communes on the rates of children enrolled in elementary schools, not to mention attendance records. Instead we are told about the number and conditions of schools, and about the eagerness of local officials to promote education. Likewise, Briggs's discussion of mutual aid societies is an institutional study rather than an attempt to understand the members. He admits that he found no written records for individual societies, so his major sources are the constitutions of the self-help institutions. Although I sympathize with him on the paucity of more detailed records, I disagree with Briggs's facile method of taking general intentions printed in the societies' constitutions at face value. He attempts to show that the societies not only provided for funeral and sick benefits, but also promoted upright, moral conduct and formal education. Yet, Briggs's sole sources are statements of intent; he has no evidence that any actions were ever effected to achieve prescribed goals. To make his case that Italians were future oriented and progressive in the hometown, he refers to the 1871 constitution of the *Società Filarmonica ed Operaia per Mutuo Soccorso* of the commune of Viggiano in which it was stated that members' children aged six to twelve years were to be kept in school. Yet Viggiano, along with Laurenzana, was the town in Basilicata most notorious for sending child street musicians — the "little slaves of the harp" — to New York, Chicago, and other American and European cities in the 1860s and 1870s.

This discrepancy between formal statement and actual practice appears in other chapters, especially when Briggs discusses the ethnicity of the immigrants. For example, his assertion that immigrants from Laurenzana, who defeated the townspeople from Lucca in a 1903 boccie tournament in Utica, were Italian "in self-concept" simply because the "Patriotism in their speeches referred to obligations to Italy" is too easily made. Examining in retrospect what Lawrence W. Levine calls the consciousness or the mind of a people involves more than taking statements at face value. Briggs should have examined more carefully the *Laurenzanesi's* claims to being Italian. Certainly the fact that each of the two opposing teams in the tournament represented an Old World hometown, places Briggs's assertion on shaky ground.

Indeed, Briggs's whole treatment of the theme of ethnicity or nationality among Italian immigrants invites skepticism. He rejects the idea that the peasant immigrants' most intensive contacts in the North American town or city were with Old World townsmen, but rather contends that the former peasants in Utica, Rochester, and Kansas City came to view themselves primarily as Italians. Unfortunately, Briggs's criteria for determining loyalties, especially *campanilismo* (essentially, strong allegiance to the hometown and its

people) in the New World are insufficient. His only measure of the latter phenomenon are rates of intramarriage in the American city among peasants from a common hometown or province: "Campanilismo might be said to be at work when endogamous marriages exceeded the rate expected if mating were to occur randomly throughout the colony." (81-84) This might be an important gauge, but certainly neither a central nor an exclusive one. I think that the problem with Briggs's perceptions of nationality and ethnicity go much deeper. When he states that if "a spirit of campanilismo had been prominent in Rochester, the colony would have been hopelessly divided and impotent," (87) Briggs betrays an inner belief that an immigrant cannot sustain loyalties to both hometown and Old Country. Perhaps it would be more useful to view the immigrant as a possessor of multiple, or expanding loyalties; such a view has been expounded by Fredrick Frey, Feliks Gross, and others. In fact, Briggs himself accepts this idea in his chapter "From Italian to Italian-American": "Patriotism was now associated with America; Italy and things Italian remained important, but in the nature of a cherished and romanticized past that had emotional significance and served as a determinant of group status to which they were now committed." (136) Briggs sees the dual identities of Italian and American in terms of accommodation yet he rejects the notion that a *paesano* (a townsman) can also be an Italian. He does not resolve the problem of dual identities convincingly and this is most obvious in his treatment of the mutual aid societies. Briggs does not account for the many societies in Utica which drew immigrants from a single Italian town; the presence of such societies hardly lends credence to his view that the immigrants forsook their hometown and regional loyalties for allegiances to Italy.

Ultimately, Briggs fails to enter the minds and to perceive the world views of the *contadini* of these three American cities. He posits that emigrants left the hometown "to keep from sliding into an undifferentiated proletarian mass" but he explores no farther. He does not indicate who in the family unit left and how the decision was made. He writes nothing about towns where emigration was not an alternative but a social fact so entrenched that to stay home was a sign of deviancy. The analysis of the immigrants' sense of space in the New World is lacking. Briggs devotes his efforts to estimating the ratio of Italian immigrants to the total population in the Italian wards of the three cities, but he makes no attempt to understand the cognitive maps or psychic neighbourhoods of the *contadini*. Social history from the bottom up should be more concerned with the immigrant's world view, informal social contacts, and cognitive world than with the political power of an ethnic group accruing from its relative strength in numbers in the population.

An Italian Passage is a smoothly-written book and Briggs deserves praise for his detailed research. It is unfortunate, however, for the historiography of Italian immigration that the author was apparently more anxious to find proofs for a number of *a priori* theories rather than in studying the inner consciousness of the immigrants.

As a postscript, I draw attention to Yale University Press's slipshod spelling of a number of names and place-names. Although I can accept an incorrect spelling of the small town of Viggiano or the ubiquitous "g" at the end of Irvin Child's first name, I find no excuse for misspelling such names as Columbus, Garibaldi, and Briggs's colleague, Mark Stolarik.

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Paul Avrich, *The Modern School Movement: Anarchism and Education in the United States* (Princeton, N.J.: Princeton University Press 1980).

IN THE 1960s, reformers and radicals sought to transform the American public school system, and revisionist historians set out to

rewrite its history. Largely overlooked by annalists and activists alike was a curious and remarkable chapter in the history of *private* education — the anarchist inspired "modern school movement." In a fascinating chronicle, Paul Avrich traces the development and demise of this libertarian experiment which pre-dated the turbulent 1960s, and from which "revolutionary" educators of that period, had they glanced over their own shoulders, might well have learned a great deal.

Few developments in the history of the American anarchist movement escaped the imprint of Emma Goldman and Alexander Berkman, and the modern school phenomenon was no exception. Inspired by the martyrdom of Francisco Ferrer, a Spanish educator and revolutionary anarchist, who was executed in his homeland in 1909, his American followers soon established the Ferrer Center in New York. This cultural and educational institution devoted itself to immortalizing Ferrer's name and spreading his educational philosophy.

Backed by a small, motley collection of socialists, liberals, and progressive philanthropists (in the days when free thinking leftists could submerge ideological differences for the common purpose of fighting capitalist plutocracy), American anarchists opened a dozen "modern schools" across the nation between 1910 and 1915. Though seldom able to recreate the spirit and optimism of these early years, the movement survived for the next four decades. Facing insurmountable obstacles, the modern school association finally disbanded in 1956.

Fervently, if naively, devoted to the belief that to free the nation they must first free its children, the libertarians invoked the catchwords and processes associated almost exclusively with the educational experiments of the 1960s. But long before then, thinkers like Rousseau, Godwin, Bakunin, Ferrer, Kropotkin, and Tolstoy had articulated, in the minds of the anarchists, an infinitely viable educational philosophy. In a "natural," unstructured environment, where children would learn by observing and doing; where teachers were companions instead of authority figures; where the individuality and dignity of the child were sacrosanct; and where discipline and punishment were as alien as exams and grades, the modern school advocates attempted to put into practice what their mentors had preached. It is jarring to realize that not a single educational innovation of the 1960s, from field trips to free schools, was original.

Yet in their promotion of unqualified individual liberty, the anarchists encountered enormous difficulties. Contradictions abounded in their movement and educational philosophy. Deifying individualism, some extreme libertarians, like Alexis and Elizabeth Ferm, sounded more like nineteenth-century liberals, as they argued that the government which governs least governs best. They opposed welfare, eschewed social action, and were hostile to all institutions (but their own school). Alexander Berkman would not have agreed with all of their ideas, but he was his own paradox. While he claimed that the modern schools should be forums for anti-capitalist propaganda, in the same breath he spoke against teaching children any "isms," and favoured the pursuit of knowledge for its own sake. Furthermore, the libertarian teachers wanted parents excluded from the schools, yet as rebels from the public system, most found it impossible to avoid imposing upon the educators and children frequently tearing the colonies and schools apart. In retrospect, Leonard Abbott (a movement pioneer) concluded that despite the good that flowed from the schools, the anarchist educators never did develop a workable philosophy, in part the result of the anti-intellectualism of those teachers who had little use for book learning.

Most destructive to the anarchist cause, apart from state harassment and deportation of many of its leaders, was the vicious conflict within colonies and schools

between anarchists and communists. While cooperation characterized early relations, the rise of Stalin turned them into irreconcilable enemies. Some anarchists even turned names of communists over to McCarthy's head hunters. Only too late did many discover that educational change in a vacuum produced only isolationism and ill-fated utopianism. But in fairness, if the anarchists insisted upon freedom without structure, the communists they fought generally had the reverse problem.

Yet the story and legacy are not all bleak. Avrich is at his best in describing the vibrant social and cultural life that revolved around the Ferrer Center in the years before World War I. There, nonconforming artists, writers, and actors spawned a dynamically creative community with some extraordinary results. For example, out of that environment emerged the realistic art of Robert Henri and his "ash can painters" who astounded observers with their graphic protrayal of ghetto life and the work of Alfred Stieglitz who introduced America to Matisse and the French modernists. Journalists hovered around the centre reporting on the activities of these free-loving, free-thinking, eccentric (and largely Jewish) radicals. Who could resist Hippolyte Havel, the mad Czech poet who waited on tables at Polly's, and denounced his customers as "bourgeois pigs?" Or the family of Ben Liber, who had a brother named Liberty, a son named Amour, and who convinced his friends, Jack and Molly Albert, to call their child "Freethought." Exalting free expression, the anarchist movement touched, if only briefly, the lives of many important figures in the cultural life of America.

While Avrich overwhelms with his meticulous research, the structure of the book leaves something to be desired. Whenever a new school opens, he feels compelled to repeat in painful, unnecessary detail the educational philosophy upon which it is based. He also provides so many brief biographies of the people in the movement, that at times his account reads like an anarchist year book, where no graduates escape mention, however ephemeral their contributions. Some creative and rigorous editing (preferably not by an anarchist) would have made this a more readable book.

But in the end Avrich succeeds in combining a tribute to some forgotten radicals with a sophisticated historical study of their activities. He is sympathetic without being sycophantic, and his conclusions, while judiciously critical, avoid cynicism and despair. His book should be the first consumed by the *next* generation of radical educators who seek to reinvent and idealize the wheel of educational reform. If they at least read their own history, perhaps they will not be condemned to re-living it.

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Harvey Schwartz. *The March Inland: Origins of the ILWU Warehouse Division, 1934-1938* (Los Angeles: Institute of Industrial Relations 1978).

THE MARCH INLAND relates the history of the formation in the 1930s of the warehouse division of the International Longshoremen's union in San Francisco. In a decade that was characterized by substantial unionization drives, a split within the American Federation of Labor which culminated in the formation of the rival CIO, and considerable labour violence, the establishment of the warehouse division of the longshoremen's union may appear to be a justly neglected footnote in the labour history of the era. Mr. Schwartz effectively establishes his contention, however, that the movement was a significant one and that it has tended to be overshadowed by historical concentration on both the colourful and volatile Harry Bridges, leader of the west coast longshoremen, and the 1934 Pacific coast strikes.

Schwartz sets his study within a larger consideration of the "causes and limits" of

unionization in the 1930s. He poses several questions in this regard: why workers organized; how employers responded; what effect if any New Deal legislation had on union activity and organization. The answers he provides to some of these issues tend to be fragmentary at times. At a time when labour historians have been fruitfully exploring the dimensions of working-class culture and its impact on labour organization, there is little consideration here of such factors as ethnicity, the social and economic background of workers, or of their residential and occupational mobility. There is some suggestion that a degree of ethnic fragmentation among Irish, Italian, and Swedish workers began to diminish in 1935 and play a reduced role in discouraging unionization efforts. Yet, one learns very little about these groups, even their numbers, and although interviews were conducted, there appears to have been no effort to determine the extent to which the interviews reflected a reasonable cross-section of workers' views.

Schwartz argues persuasively that New Deal legislation, notably Section 7(1) of the National Industrial Recovery Act and the Wagner Act, was a relatively minor factor in promoting unionization. Conversely, the evidence suggests that workers' experience under 7(1) contributed to their belief that unionizing efforts would be successful only through militant action, rather than reliance on New Deal encouragement of the voluntary cooperation of employers. Indeed, until 1934, such employers' associations as the Industrial Association, the San Francisco Chamber of Commerce, and, after 1936, the Wholesalers, Warehousemen's and Distributers' Committee provided the main opposition to the March Inland. Why employers' associations were unsuccessful in their efforts to contain the longshoremen is significant. Employers had the advantage of available agricultural workers to serve as scab labour in 1934-35 as well as substantial competition between the longshoremen and the teamsters over jurisdiction beyond the docks. Employers favoured the teamsters in this struggle rather than the type of democratic industrial unionism with which the longshoremen were associated. Yet, rank-and-file workers appeared capable of transcending the internecine rivalries of the labour aristocracy and demonstrated substantial solidarity, as for example during the September 1937 port blockade, when many Teamster pickets and ILWU men fraternized and discussed the confrontation. This incident ended the efforts of the Teamsters to break the power of the ILWU through direct confrontation during the decade, and by 1938 employers recognized the ability of the union to control its membership. The ILWU succeeded because its tactics were appropriate for the circumstances and for the structure of the warehousing industry, and because workers were driven to militancy by the appalling working conditions and wages in the depression years. Whether the New Deal contributed to their success or not, one wonders what the consequences of militancy would have been had there been a political climate and administration antagonistic to organized labour.

The study is a valuable contribution to our knowledge of the Bay area labour situation in the 1930s and casts light on some of the larger issues of the decade. One would hope that those who take the inquiry further will transcend the rather traditional industrial relations methodology of this study to incorporate the insights and techniques of E.P. Thompson, Herbert Gutman, Ira Berlin, and others working in the forefront of labour history.

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Athan Theoharis, *Spying on Americans: Political Surveillance from Hoover to the Huston Plan* (Philadelphia: Temple University Press 1978); Jane Sanders, *Cold War on the Campus: Academic Freedom at the University of Washington, 1946-64* (University of Washington Press 1979);

and Justin Goldstein, *Political Repression in Modern America from 1870 to the Present* (Cambridge, Mass: Schenkman 1978).

SINCE THE Viet Nam War era, there has been renewed interest in the varied forms of government repression of domestic opposition. It is undoubtedly an important theme in contemporary American history but historians have thus far been unable or disinterested in assessing its impact.

Athan Theoharis is a well known student of the McCarthy period. In *Spying on Americans* he extends his range back to F.D.R. and up to the present. His focus is on the FBI's activities as an internal security police and particularly on its relation to the Presidency. Unfortunately, the author takes a dramatic tale and fails to do it justice. This is not only a matter of style but also, perhaps fundamentally, of approach. Theoharis' interest is not so much in detailing FBI transgressions on basic liberties but in such matters as the success of the intelligency agency in exploiting the Cold War, the increasing claims of executive privilege, and the failure of Congress to check these developments. He believes that the critical Congressional failure came during the Eisenhower administration, when conservatives deserted their traditional, though fragile, libertarianism in favour of national security goals. Thus Theoharis prescribes a reassertion of Congressional authority through legislation and oversight as a check against the intelligence agencies' abuses.

The diagnosis and hence the antidote are debatable. Theoharis recognizes that American conservatism had a shaky allegiance to libertarianism. Other scholars, going further, have questioned whether the United States has ever had a truly conservative tradition. Certainly the failure of so-called Congressional conservatives to sustain their critique of executive power after the Republican return to power demonstrated that partisanship rather than principle controlled their behaviour. Today, Theoharis looks to Congressional liberals to reinstate checks and balances and do the job that in an earlier day they too had rejected. It is a rather mechanistic solution that issues from a questionable historical analysis. Yet *Spying on Americans* is, nevertheless, a valuable book. Theoharis has used the Freedom of Information Act well and his careful footnotes and bibliography constitute a useful guide through a thicket of materials that have often been purposefully obscured by the agencies involved. He has also given us a very able treatment of the interaction between the FBI and the Presidency.

Jane Sanders' *Cold War on the Campus* is the least ambitious of these works but not the least valuable. She gives us a straightforward chronicle of academic freedom at the University of Washington, concentrating on the post-war decade. Legislative investigation, administration compliance, the purge of communist professors, speakers bans, and faculty weakness, all form part of her narrative. It is an interesting case study of Cold War pressure on a public institution that was replicated elsewhere. Curiously, there is no indication that Sanders sought to use FBI files. If she had, it might have supplied a nice comparison to Professor Sigmund Diamond's recent revelations of cooperation with Mr. Hoover's boys by Harvard and Yale.

Finally, Robert Goldstein's *Political Repression in Modern America* is the most ambitious of the works but the least successful. His book is an almost encyclopedic account of government activities against labour and radical movements. It is based entirely on secondary sources, although newspapers, periodicals, and some congressional hearings are used for the more recent years. One would not expect that a volume which encompasses so much could rely on primary materials but the necessary corollary is our expectation of a broad new synthesis. Instead of analysis Foldstein presents us with assertions that represssion prevented the labour movement from achieving power until the 1930s, and destroyed radical labour and political movements in the United States. Certainly repression had its impact but the

author gives us no effective means of judging how important it was. The subject demands a comparative analysis that Goldstein does not supply and it is only through comparison that we can begin to weigh the impact of repression vis à vis other forces in American society. One cannot shrug off Louis Hartz's analysis of pervasive American liberalism, or David Potter's people graced by plenty, merely by the enumeration of cases of repression. Goldstein is so pessimistic in his view of increasing repression that he judges "it will not be possible to publish a book such as this in another fifty years." May that day be hastened.

Careful new studies of repression such as those by Theoharis and Sanders are useful and welcome. However, we sorely lack comparative treatments which may help us to understand not only the processes of repression but the impact of it on American society.

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Todd Gitlin, *"The Whole World Is Watching": Mass Media in the Making and Unmaking of the New Left* (Berkeley: University of California 1980).

OVER THE COURSE of the last two decades, we have witnessed the appearance of many social scientific studies concerned with "ideology" and "meaning," with the production and manipulation of "consciousness," with the determination of subjective life generally. Todd Gitlin's recent book on the role of the mass media in the "making and unmaking" of the New Left in the 1960s is one such study. According to the author, the rise and fall of the leading New Left organization in the United States, the Students for a Democratic Society (SDS), was shaped, directed, and otherwise influenced by its portrayal in and by the mass media. In turn, the SDS had a limited impact upon the latter. The movement-media "dance," as Gitlin terms it, is the focus of the book.

For Gitlin, "... the mass media have become core systems for the distribution of ideology;" they are capable of "orchestrating everyday consciousness — by virtue of their pervasiveness." (2) As such, they are a powerful force which ensures the continued existence of the established order. Content analysis of the coverage of the SDS by the *New York Times* and CBS News (1965-70) done by the author, reveals that stories about the radical student group were "framed" in such a way as to portray it either as well-meaning but utopian (in the early years), or as powerful but insidious (toward the end of the decade). Rarely were the issues raised by the SDS at the centre of media coverage. In part, this was due to the technical requirements of journalism and telecasting, to the need to find a "peg" on which to hang the story. But it was also the result of a political process which involved writers, reporters, correspondents, editors, cameramen, photographers, and in a few instances, the management of the media enterprise. An ideological front protecting the domination of the elite in society would always have to be maintained; or, in a situation involving intra-elite conflict, such as occured over the question of the prosecution of the Viet Nam War, the legitimacy of one elite fraction would be supported ideologically.

Gitlin has done his homework. He carefully documents his argument and successfully avoids the temptation of saying more than his evidence will bear. Yet, even if we give him full marks for producing a lucid and well-written book, we are forced to ask several questions which throw his entire project into relief.

Our first question concerns the treatment of "meaning," "ideology," "consciousness," as matters apart from the social relations out of which they arise, in which they are embedded, and of which they are an expression. The process of "making meanings" is seen by Gitlin to be a parallel process to "making value":

"Truly, the process of making meanings in the world of centralized commercial culture has become comparable to the process of making value in the world through labor. Just as people as workers have no voice in what they make, how they make it, or how the product is distributed and used, so do people as producers of meanings have no voice in what the media make of what they say or do. . . ." (3) Instead of saying that men produce the objects which satisfy their wants, the relations which organize and determine the historically specific character of this production, and the ideas, the consciousness of the foregoing, Gitlin makes these into separate processes, the production of meaning having assumed, in his eyes, an independent force. Meaning thus comes into conflict with meaning, ideas with ideas. Unlike Karl Marx, who viewed the ideas of liberty, equality, and fraternity as expressions of the specific character of bourgeois relations, the author of this study sees an "evolution" of these ideas in the following way: "Bourgeois ideology in all its incarnations has been from the first a contradiction in terms, affirming the once revolutionary ideals of 'life, liberty, and the pursuit of happiness,' or 'liberty, equality and fraternity,' as if these ideas were compatible, or even mutually dependent, at all times in all places. More recently, the dominant ideology has strained to enfold a second-generation set of contradictory values: liberty versus equality, democracy versus hierarchy, public rights versus property rights, rational claims to truth versus the arrogations of power." (256)

This brings us to our second question: although it is clear that the author of the book has a theory of the media in modern society, it is not at all clear that he has a theory of the society within which the media operate. There are a number of indefinite references to Marx made in passing, a lengthy discussion of Antonio Gramsci's theory of hegemony, and a few allusions to psychoanalysis. But the unmistakable theoretical beacon which guides the author to the end is that projected by the late C. Wright Mills, mentor of the New Left. (Gitlin's introductory paragraphs are constructed out of Millsian language: a "manufactured public world" stands opposite "private space" and "private crevices." Gitlin does not ask for a return to the true publics of a decentralized nineteenth-century America, which C. Wright Mills was wont to idealize; he asks the radical movement instead to free its mind of the images of itself created by the media: "A mass market culture, even with Bob Dylan records and *The Battle of Algiers*, is simply not a sustaining basis for oppositional political culture. A strategically minded political movement cannot afford to substitute the commodity process of news, fashion, and image for a grasp of its own situation, a suitable organizational form, and a working knowledge of social conditions, structures and interests." (238)

Third, we would like to ask how Gitlin understands the rise and fall of the New Left in general, since he admits that media coverage had only a partial effect upon the movement's trajectory. He astutely recognizes that the New Left was a movement of the relatively privileged whose narrow social base stood in sharp contrast to its vast political and social ambition. In addition, he underlines the radical or revolutionary will of its members. But he does not tell us *why* this particular group at that particular time developed the will to be radical." He mentions the movement's professional-managerial base but neither cites the *Radical America* article by the Ehrenreichs, nor offers an explanation of his own on the connection between the movement and its "class interest." (120) Yet, to be fair, this was not the problem which the author chose to investigate in this work.

Our final query concerns the message which the author of the study wishes to impart to his readers about the future. From our point of view this message is an alarming one insofar as it signifies that little has been learned from the past. In the author's words: "The radical project for the eighties is to regather the elements of

cultural revolt, to form of them a coherent political opposition, and to overcome the centrifugal tendencies of the seventies: tendencies which represented the playing out of the incomplete and self-contradictory results of the sixties." (292) The results of the 1960s were not incomplete and they were self-contradictory only in the sense of being a manifestation of a movement of privilege struggling against privilege. Even though Gitlin seems to have recognized the elitist elements within the New Left, he is not entirely clear on their significance. He can still ask the question: "The goods are delivered, true; but why do citizens agree to identify themselves and to behave as consumers, devoting themselves to labour in a deteriorating environment in order to acquire private possessions and services as emblems of satisfaction?" (9) The very posing of the question is an indication that the framer is cut off from the common mass of humanity. The real lesson of the 1960s, which not all have learned, is that history does not dance to the tune played by a relatively privileged few.

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BOOK NOTES/REFÉRENCES BIBLIOGRAPHIQUES

(Notice here does not preclude subsequent review.)

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