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Richard Pomfret, *The Economic Development of Canada* (Toronto: Methuen 1981).

THIS IS THE SECOND new Canadian economic history textbook to appear within a year, after a long span of more than 25 years without any direct challenge to the rein of Easterbrook and Aitken. What Pomfret has to offer is thinner and more narrowly focussed. His concerns are strictly the broad themes of Canadian economic development, essentially in the period since about 1850. His organization is mainly topical, like the other new textbook by Marr and Paterson. The volume has reasonably good coherence, even if the coverage is quite selective. The author explicitly recognizes that he has slighted both population and labour history. That may be because there is little on those topics in writings on Canada that would be classed as "new" economic history, and Pomfret certainly writes as an economist, from the perspective of the "new" economic history.

Pomfret fills in the pre-1850 background with a brief sketch that is very much in a conventional "staples" mould. He then outlines some alternative explanatory frameworks including both *the* "staples" approach and the Marxist. The idea is a good one but it unfortunately does not get carried off so well. He is not a very helpful or reliable guide to the "staples" approach and his discussion of the Marxist approach is confined to a review of Stanley Ryerson.

The chapter on "approaches" is followed by a chapter that offers a broad overview of quantitative evidence on Canadian economic growth in the long-run. Again, the idea is a lot better than the execution. There is a glaring lack of international comparison and some not altogether reliable discussion of data series and their sources. Mainly, though, the chapter proceeds without much sense of framework. The actual framework — the growth accounting approach — is

tacked on at the end as an appendix. It would have been much more effective to discuss that approach as one of the alternatives in the preceding chapter. The core of the book, however, lies in the next three chapters — on, respectively, government policies relating to economic development (tariffs, railway subsidization, and land policy), industrialization in Central Canada, and the Prairie Wheat Economy. These also turn out to be a mixed bag. They summarize the contributions of writers of the "new" economic history and offer some interesting insights into and comments on their work. These chapters would provide students with a good antidote to some of the traditional historical interpretations. They would not, however, leave them with an entirely clear understanding of what has been written. The material is intended to be self-contained but is really not. Too much attention is directed to who wrote what without explaining why he did it or what the analysis was really all about. Part of the problem may be that the author tries to compress too much. In the course of it his judgement and selection of topics become questionable. Cheese manufacturing is hardly a good example of Canadian industrialization, the large and important industries — cotton textiles, clothing, boots and shoes, and the variety of metal products manufacturing boom scarcely receive any mention. The Wheat Economy is dealt with as an on-going operation in the early twentieth century. Pomfret just passes over the important questions of why western settlement occurred when it did and how it interacted with the rest of the economy.

The chapter on the Wheat Economy has tucked away as an appendix a quite good and reasonable survey of the Chambers-Gordon assessment of the economic contributions of the wheat boom and the controversy they touched off. It is surely incongruent to tuck away in an appendix the discussion of the most substantial work on Canada which the "new"

economic history has contributed. It appears too much as a strained attempt not to be off-putting to the larger market of historians who might have qualms about assigning the book to their students. Three rather thin chapters on financial institutions, business cycles, and regional development round out the book. A short wrap-up fails as a statement of a re-interpretation of Canadian economic development but it does make the important point that attention ought to be redirected to the middle of the nineteenth century in seeking the origins of economic growth in Canada.

This is clearly not the successor to Easterbrook and Aitken as *the* textbook on Canadian economic history. Pomfret's is a limited effort with considerable shortcomings. Yet it is not a book that should be ignored. Students of Canadian economic history should be directed to it, if not as a main text and certainly not as a reference work, as a useful perspective on several of the main issues with which they have to contend and as a passable introduction to a literature they might otherwise miss.

R.M. McInnis  
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Jorge Niosi, *Canadian Capitalism, A Study of Power in the Canadian Business Establishment*, translated by Robert Chodos (Toronto: Lorimer 1981).

IS THIS THE LEFT-WING ANSWER to Peter Newman's *The Canadian Establishment*? Certainly it is better written, and there are some interesting bits of detail about business in Quebec, but unfortunately historians are not likely to find a great deal of value to them in Jorge Niosi's book. This translation of *La bourgeoisie canadienne* (1980) promises to depict "the economic anatomy of the Canadian bourgeoisie" (author's emphasis), but it proves more episodic than scientific. Niosi is almost as much concerned with reviewing the theoretical literature as

he is with analyzing "power in the Canadian business establishment." Those who want a quick primer on the views of various Marxists regarding "state monopoly capitalism" may be satisfied, but people interested in the role played by businessmen in contemporary Canadian society are likely to find the contents pretty superficial.

Who "controls" Canadian capitalism, asks Niosi? Not surprisingly his answer is the "wealthy controlling stockholders and senior corporate officers" who make up the Canadian bourgeoisie. Fair enough but are there no constraints on their behaviour? Even if one believes that the state in capitalist society is supposed to manage things in the interests of the ruling class, one ignores intra-class conflict at one's peril. If "power" is the subject of this book who has it in Canada today? Are all corporate chief executives equal? What roles do politicians and bureaucrats play? The picture given here seems much oversimplified.

A sociologist, Niosi devotes less than 20 pages to an historical account of the formation of the Canadian bourgeoisie. And the lack of penetration which this section achieves is perhaps best indicated by the first period which he treats, 1867 to 1914. The year of Confederation had no obvious long-term significance in the development of the Canadian business class. Indeed, H.C. Pentland, whom he quotes, suggests that the capitalistic labour market in Canada began to appear about 1850. The rest of the country's history is dealt with in three periods dating from 1914-1940, 1940-1970 and 1970 to the present, but the author is really only interested in the last of these. The brief narratives which he supplies contain little that is new.

Those who want to study Lenin's views on "state monopoly capitalism" as compared to those of Soviet economist Victor Cheprakov may find Niosi's chapter on state enterprise in Canada useful, but they are not likely to learn much that

is new from his accounts of the motives behind various public undertakings which have been created. The existence of a "comprador" bourgeoisie carrying out the orders of foreign investors may be becoming a Canadian commonplace, but Niosi's only conclusion after examining this group is that they sometimes cooperate with their indigenous counterparts and sometimes disagree with them. The idea that one person might be a member of both the indigenous and the comprador bourgeoisie he does not seem to consider seriously, except in the case of lawyers who are perhaps better at encapsulating conflicting interests than others.

One of the book's failings is that it does not live up to its promise to study Canada as a whole for it has a very strong focus on Quebec. One chapter deals with the New French Canadian bourgeoisie but there is no similar analysis of other parts of the country. Niosi argues interestingly that Quebec's francophone business leaders are not a "national" Quebecois bourgeoisie but rather have Canada-wide ties and ambitions, but his failure to look seriously at any other region of the country weakens his study. In fact, the book would be more useful had Niosi stuck simply to treating recent developments in the business community in Quebec. Beyond some of the detailed information on that which he supplies (including a list of lawyers on the boards of French Canadian controlled companies) there is not much here to interest the historian.

Christopher Armstrong  
York University

Donald H. Akenson, ed., *Canadian Papers in Rural History, Vol. III* (Gananoque, Ontario: Langdale Press 1982).

A GENERAL THEME RUNS through most of the essays in this volume: the crucial importance of access to affordable arable land, not only in the minds but in the lives of the majority of nineteenth-century

Canadians. If this is a truism, it is one whose ramifications are still far from being fully appreciated by our social scientists. Only recently has work by Dechêne, Harris, and Gagan, among others, indicated how the opening and closing of a frontier radically altered the functioning of class relations, community institutions, and family dynamics.

In this volume, Peter Russell's article essentially substantiates the image held by immigrants that Upper Canada was a good "poor man's country," where an abundance of cheap land promised independence, a certain sense of security, and a measure of material comfort, if not in one's own lifetime, at least for one's offspring. Gerald Block claims that the key to Robert Gourlay's career was his desire to settle British tenant farmers on freeholds, while Donald Akenson points out the obvious but largely ignored fact that the bulk of the Irish immigrants became farmers, not town dwellers and labourers. John Clarke and Bruce Batchelor briefly examine the man-made impediments to the goal of an independent homestead — Clarke by exploring the land-grabbing career of John Askin in Essex County, Upper Canada, and Batchelor by demonstrating the harmful effects of a land company's monopoly in what was to become central Alberta. R.M. McNis reviews the perennial "agricultural crisis" debate of Lower Canada, attempting unsuccessfully to prove that there was no shortage of accessible, good quality land, and therefore probably no crisis at all. Alan Skeoch's survey of the evolution of plows reminds us that even in regions where land was plentiful and inexpensive, available technology placed strict limitations on the size of the family farm. Finally, Darrell Norris and Victor Konrad demonstrate that nineteenth-century rural housing types often reflect not so much the date of construction as the socioeconomic status of the builders-owners. Thus later arrivals who were forced to choose more marginal land

tended to replicate outmoded but more affordable housing styles. The only essay which sheds no light on this "man-land" theme is that of John Mannion which measures shipping activities in an Irish port town, and is completely out of place in a volume on Canadian rural history.

Most of the articles in this volume are tentative explorations rather than definitive studies, but they do raise important questions for further research. Space limitations restrict me to an examination of the three papers which I found to be most thought-provoking. In his attempt to determine how good the opportunities were for immigrants to Upper Canada, Peter Russell finds conflicting evidence. Firstly, there was a dramatic decline in the number of absentee proprietors after the institution of settlement duties in 1818. In fact, between 1812 and 1822, most speculators were resident farmers. However, findings from the assessment records indicate that the true key to prosperity for most farmers was not the amount of land owned, but the ability to bring a reasonable number of acres under cultivation — one hundred acres in wheat ensured an average annual income of \$1500, comparable with the salaries of high governing officials. Very few farmers ever reached this lofty status, for Russell has also found that land clearing was a much more time-consuming task than contemporary observers and modern-day historians have led us to believe. Farmers cleared an average of one and a half acres per year, thereby requiring a lifetime to reach 50 acres and a position of modest material well-being. What Russell does not suggest is that observers probably based their apparently exaggerated estimates on the work of pioneers (he deliberately avoids examining frontier townships) who did not have the planting and harvesting of cash crops to slow their progress. Four to six acres were likely cleared during the first year or so in order to gain the minimum arable land needed for relative self-sufficiency.

While future prospects were strictly circumscribed for those who had to start from scratch in this "poor man's country," Russell calculates that between 1812 and 1842 at least 25 per cent of the immigrants brought the considerable capital needed to buy a cleared farm or to hire labourers to help them in the backwoods. And Donald Akenson's article indicates that the Irish were probably well represented among this fortunate minority. In contrast to the historical image of the typical Irish immigrant as a diseased Catholic pauper who arrived during the famine years of the late 1840s, Akenson finds that the Irish were already one-quarter of Upper Canada's population in 1842, with fully one-third of these being Catholic; that during the famine years it was not the landless paupers who crossed the ocean, but the tenant farmers of north-central Ireland (mostly Protestants) who had enough money to pay their passages and even buy land; and that at least three-quarters of the Irish did not live in urban centres. Akenson admits that many of his figures are still rather tentative given the inadequacies of statistical data — for example, his conclusion that more Protestants arrived during the famine migration is based on the 1871 ratio of Irish Protestants to Irish Catholics, but a higher proportion of the latter probably had moved on to the United States by this late date.

His revisionist findings lead Akenson to condemn as racist stereotyping most of what little work has been done on the Irish. But perhaps he is being a little harsh, for Irish labourers did dominate canal and railroad construction as well as much of the logging industry, and Akenson himself demonstrates that in 1851 and 1861 an Irishman was twice as likely as any other Upper Canadian to find himself in the city. By 1871 this ratio had equalized, but the Irish Catholics still had twice the proportion of the Protestants in urban areas. Akenson is no doubt correct in stating that it was natural for a newly-arrived population to start off at the bot-

tom of the ladder, but he provides no evidence for his suggestion that there was a steady progression from urban to rural places. It is more likely that *vis-à-vis* the Catholics, Irish Protestants were under-represented in the cities in 1851 and 1861 as well as 1871, and that this trend continued after 1871 when Ontario's rural frontier had long since been closed. Furthermore, we still do not know what proportion of those Irish living in the countryside were prosperous commercial farmers as opposed to being members of the rural proletariat. In other words, the Irish Catholics were probably not as "invisible" as Akenson suggests, which helps to explain through not condone the unfortunate stereotyping they have been subjected to.

R.M. McNnis' paper also dismisses the previous historical interpretations of its subject, the state of agriculture in Lower Canada during the first half of the nineteenth century. He tests and rejects the three main explanations for Lower Canada's agricultural crisis — that the French Canadians' slovenly agricultural practices exhausted the soil, that there was an absence of markets for their produce, and that the land had become overcrowded. This leaves him with the conclusion that perhaps there was no agricultural crisis after all, only problems to be met and adjustments to be made. The disastrous crop failures of the 1830s and 1840s he dismisses as a short-term anomaly. One man's crisis may simply be another man's problem, but there is simply too much evidence to the contrary to deny the impoverishment of the habitants prior to and after the rebellions, or the difficulty of the adjustments after mid-century.

McNnis does have a certain amount of sympathy for the nationalists' argument that the major problem was the lack of a good market rather than the ability of French Canadians to produce a surplus. Prior to the early 1840s, Britain simply was not a dependable client even for wheat, making the internal market cru-

cial, and therefore causing prices to be depressed by Upper Canadian and American competition. McNnis claims that Lower Canada's wheat yield in 1851 was nine bushels per acre, on a par with that of certain eastern American states, but far below the twenty bushels per acre that Russell claims was a modest yield for Upper Canada. Upper Canada's soil and climate were clearly better-suited to wheat culture, forcing Lower Canadian farmers to turn increasingly toward alternatives. Presumably they would have held the advantage in the local market for harder crops and livestock products, and McNnis points out that other North American regions with healthy agricultural economies depended upon internal markets of a similar magnitude. This returns us to the possibility that Lower Canadian farmers were simply incapable of taking full advantage of the demand that did exist for their produce.

The reasons could only be low crop yields and/or small amounts of improved land and cattle. McNnis gives few agricultural statistics, but Serge Courville's recent study of the St. Lawrence Valley reveals a decline in crop yields and in livestock per farm between 1831 and 1851. Of course, if farms were reasonably large, low yields would not necessarily have been disastrous. McNnis finds that Lower Canada's farms were similar in size to those of Upper Canada in 1851. Unfortunately he does not compare acreages under improvement, though he claims that Lower Canada's farmers were not restricted by a shortage of arable land. Even in the seigneurial counties, less than half the "cultivable" land (that is, land that would ever be improved in the future) was actually being exploited in 1851. McNnis' map does reveal, however, that in the major grain-growing area around Montreal and in the Richelieu Valley, over 70 per cent of the cultivable land was already improved. McNnis' calculation also assumes that seigneurs were not withholding land for speculative purposes, as

was very widely claimed at the time. Finally, it is simply untenable to assume that land which was eventually "improved" or "cultivated" as pasture, when Quebec adopted a livestock-oriented agriculture, was also suitable for growing wheat.

McInnis feels that the vast extent of still unopened territory remaining outside the seigneuries clinches his argument that Lower Canadian farmers had ample room, and therefore could have suffered no inherent crisis. As an example, he points to the fertile yet undeveloped Bois-Francs area, but he ignores the barriers posed by speculators and almost-impenetrable swamps parallel to the St. Lawrence. Not until the arrival of the railroad and a municipal system which taxed absentee proprietors could the south-shore settlers flood into this promising area. And if so much good land was left in the seigneuries and townships during the 1850s, why did so many habitants move to the complete isolation of the upper Saguenay, to the barren soil of Wolfe County, and to other areas in the Eastern Townships which were shunned by the region's English-speaking farmers? McInnis even argues that French Canadians went to the New England mill towns because of pull rather than push factors, but why, if factory jobs were so attractive, did the Americans leave them to immigrants?

In short, McInnis has failed to demonstrate that the French-Canadian farmers had not reached the limits of the commercially-viable land available to them before mid-century, or that they were able to meet the competition from more advantageously-situated Upper Canadian and American farmers. French-Canadian farmers may very well have been as efficient as their English-speaking counterparts, but they were certainly not as prosperous, and that was the critical issue for the socio-economic development of Quebec.

We must await the appearance of representative and comprehensive local

studies before we can provide more definitive answers to those questions concerning man's relationship to the land that are raised by McInnis, Akenson, and most of the other contributors to this stimulating volume.

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Carol M. Judd and Arthur J. Ray, eds.  
*Old Trails and New Directions: Papers of the Third North American Fur Trade Conference* (Toronto: University of Toronto Press 1980).

THE THIRD NORTH AMERICAN fur trade conference ended on a glorious note in the banquet hall of the Fort Garry Hotel at the forks of the Red and Assiniboine. Several hundred assorted academics, public service historians, anthropologists, archivists, and fur trade hobbyists and enthusiasts had been treated to a supremely elegant after dinner address. In a virtuoso display of the historical imagination and discipline, Professor Glyndwr Williams, Queen Mary College, London traced the content and authorship of Anthony Henday's journal. It was one of those seemingly effortless British academic performances — no vulgar typed notes to be seen, no jargon, no quantification — an almost languid delivery and a genially impressive intellect. This could only have been topped by the spontaneous votes of congratulations to the conference committee that arose from the banquet floor, spearheaded by an impromptu speech from Irene Spry and Lewis G. Thomas. For, in spite of the few murmurs raised about the lack of native participation, it had been a highly successful conference; genuinely interdisciplinary, properly reflective of established and new scholars, international in scope, in fact almost a celebration of the state of the art.

The 19 essays in this published version of the conference proceedings are



divided into six sections — "Social History, Native Societies, Maps, Personalities, the Pacific Coast, and Economic Aspects" — with topics as diverse as agriculture, women in the fur trade, French attitudes to native society, the Russian fur trade, and the intellectual contribution of company men to international cartography. As W.L. Morton suggests in his foreword, the fur trade's concerns were those of an entire way of life "as complete in itself as most human existence." The selection of conference papers satisfactorily mirrors this perception of the discipline.

Placed on the shelf next to the slim volumes of papers of the earlier conferences (St. Paul, Minnesota 1965 and Winnipeg 1970), these essays demonstrate some of the more overt changes in fur trade studies in the last decade. Like most academic fields it has become increasingly professional, more inclined to interdisciplinary approaches, more commonly Canadian, dominated by a variety of younger scholars, more concerned with the native role in the trade, and conscious of new directions elsewhere in historical study — particularly in material history and in labour, women, and ethnic studies, all of which are represented here. No longer is it the pursuit of amateur voyageurs, the odd armchair anthropologist, or the privileged few academics with access to London's Beaver House archives where the leisurely pursuit of research was legendary. The expansion of the Historic Sites service, the growth of Canadian archaeology and ethnology, and most important, the transfer of the H.B.C. Archives to Winnipeg where they have been made increasingly accessible to researchers have all contributed to the major changes in the field.

Labour historians will find several papers of interest here. Carol Judd's discussion of the importance of ethnicity, or at least ethnic stereotyping, in the hiring and deployment practices of the H.B.C. is a solidly researched and modestly argued

paper. "Mixed Bands of Many Nations: 1821-1870" demonstrates that although there was a brief labour surplus at the time of union 1821, the H.B.C. soon experienced labour shortages and attempted to fill its needs as in the past with Orkneymen deemed to be slow but hard-working, and Canadians, the better boatmen, voyageurs and woodsmen. As wages in Lower Canada began to rise in the 1840s, the Iroquois became more prominent as "excellent men for hard labour" but lacking the "dash, vivacity and the song" which apparently so entranced their master, Sir George Simpson.

In a separate article Trudy Nicks analyses clearly and competently the role of those Iroquois labourers, while John Nicks discusses the recruitment of Oradians by an analysis of the family relationships and hiring patterns in the agricultural parish of Orphir. The observations he makes on the savings rates of these frugal company servants and the impact of their return on the parish economy pushes the boundaries of fur trade research further back. Often these sons of small tenant farmers who left their country wives and children in fur trade society were able to outbid others for small holdings in Orkney and thus enter the middle ranks of society. Is it any wonder, concludes Nicks, that they should come to be regarded as men with "a low level of commitment to the interests of the Company, but shrewdly attentive to their own welfare." Such a conclusion, although not incorrect, is typical of much of the tone of fur trade research where the viewpoint of the Hudson's Bay Company still tends to dominate its new scholar servants.

The most marked change in fur trade studies is the recognition of the role of native people. Malcolm Lewis' "Indian Maps" records the cognitive world of the Indians of the North West, an article which is paralleled by Calvin Martin's stimulating and fundamental consideration of the spiritual links between man

and animal, broken by the fur trade. Toby Marantz' discussion of the "Cree of James Bay" is an excellent example of the new literature emphasizing Indian control of the fur trade. According to this ethnohistorical analysis, the eastern James Bay Cree spent little on alcohol or tobacco, were slow to adopt steel traps, and after a century of fur trade contact had experienced little disruption of their social or material culture.

In examining the role of the "Indian as Consumer," A. J. Ray takes up a similar refrain of the Indian as an aggressive trading partner taking the best advantage of competitive conditions and demanding goods suited to his environment and way of life. The other side of this equation, of course, the Indian as labour, is not represented here, and to a large extent is still to be written. Readers interested in such questions might turn to the analysis of native apprenticeships and seasonal wage labour available in Philip Goldring's innovative, impressive *Papers on the Labour System of the Hudson's Bay Company*. (Parks Canada, Manuscript Report Series, no. 362)

The general reader would find Sylvia Van Kirk's "Fur Trade Social History: Some Recent Trends" and the substantial bibliography included at the end of the book helpful as introduction to the field. A suitable seminar selection might add both Charles Bishop's fascinating biography of "Kwah, A Carrier Chief," an article which would interest any level of student and Mary Cullen's "Outfitting New Caledonia 1821-1858." In spite of a deceptively dull title this is a model article which can illuminate for students the range of historical enquiry in fur trade studies. Starting with an administrative problem of transport and supply common to much of Canadian history, Cullen summarizes technological change, political and diplomatic concerns, management interests, economic returns, social life, and in particular labour conditions where

deviancy or desertion was punished by "simply knocking the men down, kicking them until they got up, and knocking them down again until they could not get up any more when they finished them off with a few more kicks." (239)

A final paper of note is that of Irene Spry, Professor Emeritus of Economics, University of Ottawa, on "Innis, the Fur Trade and Modern Economic Problems." A former research associate and colleague of Harold Innis, Spry notes that the study of the fur trade was the foundation for Innis' work and argues that the basis he laid for Canadian political economy is still relevant to the problems facing Canada today. Innis recognized that the fur trade ended native dominance on Canada's frontiers. He emphasized the role a staple exporting economy played in developing political dependency and in making instability and loss of local control a perpetual condition of hinterland life. And finally the study of three centuries of H.B.C. activity as a multi-national corporation brought Innis to some of his conclusions about the impulses towards centralization, economic inflexibility, and the threats to political democracy inherent in such organizations. Professor Spry's own role in fur trade studies deserves particular comment for it goes beyond that of the economic critic of the corporate fur trade. She is herself a forceful articulate woman who has done much to foster an informal sense of collectivity amongst fur trade scholars in Canada.

The fur trade was once the domain of those seeking only a story of manly vigour and entrepreneurial success in the wilderness. As Glyndwr Williams acknowledges in his "Epilogue," it continues to attract us because as a subject it offers "within manageable compass, all the ingredients of human motivation and accomplishment which historians and their public seek."

Jean Friesen  
University of Manitoba

Graeme Wynn, *Timber Colony. A Historical Geography of Early Nineteenth Century New Brunswick* (Toronto: University of Toronto Press 1981).

THIS BOOK IS both a pleasure and a disappointment. It is a very great pleasure for geographers and historians to have available in published form Graeme Wynn's meticulous research on the timber trade in early New Brunswick. A major study of this important staple trade has been badly needed for a long time and I, for one, am greatly relieved to be able to abandon my microfilm reader and work at last with this compact and pleasant-looking volume.

Wynn provides us with a detailed and knowledgeable description of the growth of the trade to 1850 in a work whose insights into the structure and day-by-day nature of logging in early and developing New Brunswick are unparalleled in the literature to date. Beginning with a picture of New Brunswick in 1800 in terms of settlement, forest cover, and the early timber trade ("a backwater of empire, peripheral to, and poorly connected with, the North Atlantic world"), the book goes on to look briefly at the transatlantic economic context, the rise of the timber trade, and the creation of a dependent economy in New Brunswick. This is followed by two valuable chapters detailing the "assault on the forest" and the rise of sawmilling, both examples of Wynn's careful research and fine-grained knowledge of this staple industry *in situ*. Thereafter, there are two chapters on the structure of the trade, and the book concludes with another review of New Brunswick, now at mid-century, when the full flood of the export staple industry has passed and its impact can begin to be assessed.

This book is a valuable addition to the existing literature on Canadian staples. In particular, the chapter on the structural organization of the trade ("The Role of the Entrepreneur") is masterly and the maps and diagrams attractive, informative, and a good portrayal of process. This

chapter excepted, however, the book is not the theoretical work that the back cover would lead us to believe. Since the study does not go beyond 1850, we do not have a full model of the "rise and fall" of the New Brunswick timber trade; since it crosses the Atlantic only very briefly in Chapter 2, we do not have a balanced transatlantic perspective on a transatlantic trade; although the book is about an export staple, we have no theoretical discussion of how dependency develops in staple economies. What we do have is a discussion of tariffs, changing markets, and entrepreneurial response — this last being the chapter that comes closest to theory, using Vance's wholesaling model very usefully to elucidate the structure of the North American production end of the trade and using the concept of evolving dependency to document entrepreneurial response. I hasten to add that this is not a criticism of the study itself so much as of the exaggerated claims made in descriptions of the book by both author (in his Introduction) and publisher (back cover). It is not a theoretical book, nor do I find the horizons of the study "far wider than the province itself" (10) except in Chapter 2, which relies heavily on secondary material, and in the recognition that exogenous elements such as tariffs were vital to colonial development. Such claims do the author a disservice, as does the occasional production problem: a case in point being the use of photographs from the 1880s and 1900s in a study which stops in 1850.

Taken overall, this book should be judged for what it is rather than what it is claimed to be. It is a meticulous, detailed, informative, and invaluable description of New Brunswick's timber trade to 1850, with some very useful modelling of the evolving structures of the extraction and processing end of that staple trade. It is not a treatise on staple theory, dependency theory, or merchant capitalism. What Graeme Wynn has done, and done superbly well, is to document for us the evolu-

tion of Canada's timber colony to mid-century. His contribution to our knowledge of that trade is important, and it is in this context that the book is essential reading for all students of the history and historical geography of Atlantic Canada.

Rosemary E. Ommer

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H. Clare Pentland, *Capital and Labour in Canada 1650-1860* (Toronto: James Lorimer 1981).

GONE ARE THE DAYS when conscientious students strained their eyes reading microfilm versions of perhaps the most celebrated doctoral dissertation in the social sciences in English Canada. Thanks to the efforts of Paul Phillips, James Lorimer, and an SSHRC publishing grant, the monumental work of H. Clare Pentland has surfaced after a 20 year life as a subterranean classic. And what a *tour de force* it is. Its impact is even greater in book form.

Neo-classical economists have created assumptions about the workings of a capitalist economy that allow them to ignore the societal and cultural changes and the inequality between people that underlie capitalist industrialization. It makes for a neat, static world to which mathematical models can be applied. But it is a fantasy world that deletes human history. Pentland does not share these easy assumptions. In the tradition of Marx, Weber, Dobb, Hobsbawm, Thompson, Gerschenkron, and Landes, the great encyclopaedic thinkers about modern society, Pentland is intrigued by the metamorphosis of people and society in the industrial revolution.

Pentland begins with an examination of how a capitalist labour market was created in Canada and his work branches out from there. It ranges into questions about technological change, the vast migrations of people, the transformation

of Canadians from a world of tradition, camaraderie, self-sufficiency, and an attitude in "which existence was a sufficient justification for existence" into the "grimmer discipline" of modern industrial society where the emphasis is on progress, hard and regular work, sobriety, individualism, and future acquisition as the rationale for life. These aspects of his work began to receive the proper attention they deserved from labour historians and Marxist-oriented economic historians in the 1970s. But Pentland's influence is much narrower than it should be. He was not simply a labour historian but a great synthesizer, an historian of social change. Many of his greatest contributions are still ignored.

He developed a complex and dynamic approach to ethnic relations in Canada that break free from ahistorical, functionalist assumptions. Pentland examined the economic basis for emigration and the specific conditions in Canada into which the immigrants came. Time of entry strongly affected their class position, but social customs, cultural ideas, and skill levels also determined an ethnic group's economic place in society and led to competition and conflict with other ethnic groups. In looking at the connections between a variety of historical trends, Clare Pentland never lost sight of the people who experienced the changes he wrote about. He was a humane historian.

Perhaps his best work has been a modification of the staples approach to Canadian economic development. But this contribution remains largely ignored. He deftly sidesteps the subsequent sterile debate about merchants versus industry in late nineteenth century Canada. The merchants of Montreal inadvertently created the conditions necessary for the broadening of the Canadian economy by the 1850s, despite their distaste for fixed investment and their lack of concern for the colony's progress. In their competition with New York for the continent's

interior trade, Montreal merchants were forced to construct canals and railways — fixed investments — that created the basis for diversified industry and for Canada's first sizeable wage labour force. "Transportation facilities constructed in the last desperate attempt to hold the interior for the St. Lawrence system, failed to accomplish that purpose; hence, their incidental function of creating a Canadian economy also became their only function."

It is ironic that the nationalistic staples approach and the apologists for Canada's current branch plant economy both leave the impression that Canadian manufacturing is a recent phenomenon. Pentland provides an antidote to this misconception. He traces the development of some of Canada's early industries and the political movements supporting industrialization. Given the myths we are reared on, it may come as a surprise that the St. Maurice Forges, begun in the last days of New France, and continuing until after Confederation, produced iron that "was usually declared better than American or English, and sometimes was held to be as good as Swedish." A wide range of finished iron products also was considered superior to English imports. Pentland explores the new political ideas and the social groups that backed the campaign for a more independent and diversified industrial economy beginning in the 1820s and 1830s. These early economic developments and political movements explain why Canada achieved the rank of eighth largest manufacturing country in the world by 1867, easily surpassing Japan and Sweden. Contemporary Canadians are astonished at this because our history has been presented in a one-sided way. Pentland's book ends in 1860 on the eve of industrialization. The subsequent reversion of the Canadian economy to the ranks of the semi-industrialized, resource exporting economies, like Australia, thus lay outside his purview.

Pentland has received due recognition for outlining the ways an industrial work-

ing class was created in Canada. Before the middle of the nineteenth century, employers had a difficult time finding labour of any type when they needed it. The employers' predicament demonstrates that the operation of a capitalist labour market is not the automatic working out of an abstract economic law, but rests on a social construct peculiar to capitalism. It assumes "the creation and continuance of a large enough labour force that employers could always hire the numbers wanted, whenever they were wanted. . . . These conditions . . . permitted employers to force workmen to bear their overhead costs, and yet to be available for use, even for very short periods." Thus a capitalist labour market assumes a social system that benefits employers and shifts the cost of large scale unemployment to others.

Sometimes it is held that new settler societies were born capitalist. But this is not true. They had the peculiar problem of an abundance of land that inhibited the creation of a wage-earning class. Why would people want to assume their own maintenance costs while risking unemployment in working for someone else, when they had the alternative of independent farming? But these obstacles to the creation of a capitalist labour force were nothing compared to the problems faced by the late industrializing countries of Europe, with which Canada shared striking economic similarities in other respects. In a long drawn out conflict, filled with misery, serfs had to be thrown off the land before many would embrace the uncertainties of working-class existence. In Canada it was different. It was not the surplus children of Canadian farmers, but the cast-offs of Europe, that formed the basis for Canada's original working class.

In pre-industrial Canada, employers devised a number of means to tie labourers to employers for short periods. Pentland reviews the forcible methods such as slavery, indentured servitude, and convict

labour that were tried. They could not produce eager or productive workers. Positive incentives, usually involving the promise of land in the future and economic support when no work was required, were needed to induce a labour force even for short periods. The problem for employers was a shortage of labour that created the positive conditions for "personal labour relations" with similarities to serfdom. "The workman had to be kept when not wanted so that he would be available when wanted." As Pentland points out, the employers' costs of providing for the maintenance of labourers even when not needed, created a barrier to industrialization. But this obstacle must be kept in perspective. It was overcome in the 1850s in Canada, on the eve of industrialization, whereas other late industrializing countries had to simultaneously sever the ties of a sufficient number of people from the land while at the same time establish the first factories.

The Irish provided the basis for Canada's initial working class. Fleeing from the collapse of the potato economy in the 1840s, they were the first group of people that did not hanker after a return to the land and were willing to devote their lives to wage labour. Pentland's work centres upon the Irish migration: why they left their native country, their economic position in Canada, the role of the Orange Lodge in working-class culture and politics, and the early labour disputes involving conflicts with employers, amongst themselves, and with French Canadian workers. It is this work on Irish labourers that has received most attention and sparked several controversies and a growing body of solid historical research.

Pentland never considered this manuscript a completed work, so it is perhaps unfair to look at aspects of Canada's transformation to an industrial society that he failed to explore. It must be pointed out, however, that there is a systematic omission in Pentland's work. For someone who was obviously concerned about

the lot of the ordinary person, it is surprising that he neglected the political role of farmers and workers in his discussion of the formulation of Canadian economic strategy. Pentland notes that "the exceptionally strong political position which merchants occupied between 1765 and 1835" was due to the "weakness of Canada's agricultural base" and he subjects the actions and world view of British officials and early manufacturers to considerable scrutiny. But the most numerous class, the farmers of Upper Canada, are hardly mentioned in the movements to diversify the economy. By 1848 responsible government had given the farmers the possibility of political control. Why did they not seize the chance? The crisis in French Canadian agriculture that gave rise to massive emigration to New England and protectionist campaigns to promote industry in Canada are ably presented, but the effects of subsistence agriculture on the existence, or rather the absence, of farmers' political movements in French Canada are not explored. Similarly, Pentland stresses the processes by which a capitalist labour force was created and largely neglects the history of early union formation and the development of working-class ideas in Canada.

While it is easy to list sins of omission from a work of such scope, it is more difficult to fault Pentland for sins of commission. Several of his interpretations can be challenged, but *Capital and Labour in Canada 1650-1860* remains a masterpiece. May its influence grow in years to come.

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Ian MacPherson, *Matters of Loyalty: The Buells of Brockville, 1830-1850* (Belleville: Mika Publishing 1981).

IN A THEN PROVOCATIVE PAPER delivered to the Canadian Historical Association in 1965, Alan Wilson urged a return

to biography and specifically to biographies of figures of the second rank. The workings of society would be revealed by the study of the less than famous who made social institutions operate. About that time Ian MacPherson was working on a M.A. thesis concerned with some of those second rank men, the Buells of Brockville. A decade and a half later MacPherson's results have been published and they partially justify Wilson's contention. As a book, *Matters of Loyalty* is not successful. But it does offer some useful material on the local society of Upper Canada and on the plodding progress of local élites.

The Buells were Loyalists and liberals. The first characteristic, joined with shrewd land dealings, gave them prominence in Leeds County and the town of Brockville. The second gave them a difficult political field to hoe in an area dominated by Tory Loyalists, the Sherwoods and the Joneses. Buttressed by their ownership of the district's most important newspaper, the *Brockville Recorder*, and their links to provincial figures such as Mackenzie and Baldwin, the Buells did gain a measure of political success. Their patient service eventually was rewarded when the Reform Great Ministry achieved power in 1848. Important patronage flowed their way and a scion of the family, William Buell Richards, would become attorney general in the Hincks-Morin government of 1851. Richards was the most distinguished member of the clan, becoming first Chief Justice of the Supreme Court of Canada and, in 1877, Sir William.

Founding fathers of Brockville and important promoters of their district, William and Andrew Norton Buell are chiefly of interest as examples of a type, the local élite members who retained positions of prominence because of their social utility. MacPherson furnishes all of their credentials of community leadership and, lest we get lost in the tangle, summarizes their careers in a genealogy conveniently pre-

sented as an appendix. This is a more detailed picture of an élite family than any we have had. Although the Brockville leadership already was familiar from Elva Richards' article on the Joneses (*Ontario History*, 1968), it is useful to have the complete story. Or, rather, the complete story of certain aspects and of some of the time. MacPherson's book is even more limited than its title would suggest. He is chiefly interested in the pre-Union period and the vital 1840s get short shrift. He also is a partisan, anxious to present his heroes in the best light. As a result some important parts of the story are untold.

One tantalizing aspect is the relationship of A.N. Buell with William Lyon Mackenzie. MacPherson attempts to exonerate Buell from any part in the rebellion but his ties to Mackenzie were strong throughout the rebellion period and for many years thereafter, as both the Buell and Mackenzie papers testify; MacPherson relegates the correspondence to a footnote. That Andrew's brother William was loyal is beyond doubt but this contretemps illustrates a general problem in MacPherson's book, an attempt to homogenize the Buells. He quite correctly lays stress on the importance of family in building business and political success. However, this often leads him to underemphasize the differences within the family. A more familiar illustration of the point would be the Baldwins. Family ties were very important in creating their power base and, for Robert Baldwin at least, family was more important than any other consideration. Yet the bonds of family affection could stretch across wide political differences with Robert's Tory uncle, Augustus, his erratic cousin, R.B. Sullivan, and even his treacherous cousin Edmund Murney who used Orange troops to defeat Robert in the Hastings by-election of 1843. So, perhaps, with the Buells, William was more conservative than Andrew. And both, as members of a mercantile and agricultural élite, were clearly distinguishable from their nephew,

W.B. Richards, who was a member of what might fairly be distinguished as the first modern and capitalist government, that of Francis Hincks, which ruled Canada between 1851 and 1854.

Andrew Norton Buell also is interesting because of his seeking after patronage, an activity which receives little notice here. Despite their prominence, the Buells had suffered from the one-party rule at the provincial capital. While the provincial élite of Upper Canada would have argued that public offices were distributed on the basis of merit, in fact the Tory party always had first pickings. One of the accomplishments of the Reform governments of the 1840s was to open up patronage to the politically dispossessed, to bring new blood into government and to end, at both the provincial and local level, patronage by divine right. A.N. Buell was one who felt that Baldwin did not go nearly far enough in the good work. He wrote to his leader in July 1849 that too many old enemies were still in office while those who had fought the fight for the party "ought to reap the spoils of victory." Andrew himself did reap the spoils in 1849, an episode which is passed over quickly and inaccurately by MacPherson. Buell mounted a campaign within the party to win the clerkship of the Crown in the Court of Common Pleas since he was tired of waiting, as he advised his brother, "for some of the loaves and fishes of a Reform Government to fall to my share." He was bitter when given an office, Registrar of the Chancery Court, which did not satisfy his ambitions and was only mollified when, in December 1849, Baldwin finally delivered the Common Pleas post. The Buell interests were disaffected on a larger scale, as well, because of the monopoly on patronage in eastern Upper Canada maintained by Francis Hincks and what W.B. Richards called "a certain clique about Montreal." The Buell connection, in fact, is a good microcosm of the patronage question and of the democratization of government in the

1840s and 1850s. It is a pity that MacPherson in his portrayal of the gentry and its public service has not taken the opportunity to cast some much needed light on this important development.

The book leaves a good many dark corners for it is more interested in genealogy than ideology. It does amplify what we already understand about the gentry code of Upper Canada. However, its discussion is episodic and uneven and often difficult to read. The fast skim over the 1840s is particularly frustrating because MacPherson does have some provocative suggestions about the decline of the old élite and the retreat from a gentry-style interest in business progress into agrarianism in the forties. Again, this suggests profound differences within the family. If William and A.N. were cool to the capitalist progress of the Union and unenthused about railways, as MacPherson contends, it must have marked them off sharply from W.B. Richards, member of the Hincks' government whose chief rationale was the railway.

MacPherson has given us a good deal more of what we already know about the gentry but has missed the chance to tell us about what we see only dimly, the fate of that gentry in the age of democratic politics and capitalist economics. Still, so little of substance is written about Upper Canada that we must be grateful for even slim pickings.

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Doug Owsam, *Promise of Eden: The Canadian Expansionist Movement and the Idea of the West 1856-1900* (Toronto: University of Toronto Press 1980).

THERE ARE DECIDED ADVANTAGES to reviewing a book two years after its initial appearance. The reviewer gets a chance at a second reading and needs not worry about making a wrong call. By now the jury is in and so the critic can merely echo



the wise pronouncements of his colleagues. In the case of Doug Owrām's *Promise of Eden*, the verdict is highly favourable and in that judgment this reader readily concurs.

The reviewers generally accept Owrām's thesis that the changing ideals and aspirations of Canadian expansionists during the late nineteenth century shaped their perceptions of the Northwest. Traditionally, central Canadians dismissed the prairies as an inhospitable wilderness good only for fur trading, but in the 1850s, Owrām claims, they began to use the reports of explorers and scientists to create a concept of fertile plains rich in resources and ripe for exploitation. Because they wanted Canada to expand its commercial empire, they figuratively transformed an inhabitable landscape into a pleasant environment. The botanist John Macoun epitomizes this process: in describing Palliser's Triangle, this scientist painted a picture of a lush garden where others had seen a desert. After Macoun, when reality shattered the illusion, expansionists blamed everyone but themselves for the disillusionment, and thus created a regional identity based on discontent.

Good proof of Owrām's theme — that preconceptions even influence observations of the geographical environment — is his consistent misspelling of Hudson Bay. Believing the spelling of the company to be derived directly from that of the bay, Owrām unwittingly adds the apostrophe 's' to the bay in quotations and book titles alike. Even a trained historian will see only what he wants to see.

Although the general thesis is beyond dispute, the means used to prove it sometimes raises suspicions. Owrām's use of short paragraphs and cryptic quotations sometimes creates an unfair appreciation of an author. A good case in point is George Grant who emerges from *Promise of Eden* as a crass, materialistic expansionist who distorted reality with unfair hyperboles. Owrām writes, for example,

that Grant's only criticism of Red River soil "was that it was too rich, leaving the farmer with nothing to do with his manure except dump it in the nearest river." (108) Grant utters no such reservation on the pages cited, but only recounts his visit to a lush garden where no manure was used. To be fair, Grant does refer to manure dumping elsewhere in *Ocean to Ocean* but not in the tone presented by Owrām. Similarly, the failure to discuss more sober judgments, as of the Hudson Bay land commissioner C.J. Brydges, taints *Promise of Eden* with the sins of its subjects. In short, the eclectic quotations do at times take on the appearance of proof-texts in an evangelist's sermon.

Even though *Promise of Eden* does not need the same liberal dose of salt that the writings of its subjects require, it has its limitations. The work is a study of an extreme view among certain promoters of the Northwest, most of whom were Ontario-based. Their claims are a remarkable illustration of the length to which some expansionists would go to sell western Canada in face of the increasing competition for settlers. Owrām has written a highly readable and fascinating account of these miniature imperialists. It is an amazing tale.

A. A. den Otter

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Patrick A. Dunae, *Gentlemen Emigrants: From the British Public Schools to the Canadian Frontier* (Vancouver: Douglas & McIntyre 1981).

THE CONVENTIONAL STORY of nineteenth- and early twentieth-century emigrants to Canada presents a picture of Europe's huddled masses, victims of industrialization or drought, to whom any change meant improvement, even change so drastic as to mean tearing up familial and cultural roots forever. The story continues with accounts of long days in steerage, disembarkation in the fog at Halifax, St. John, or Quebec City, an equally

uncomfortable trip in the infamous colonist car, to be let off at a CPR station in a strange community full of hostile or indifferent people concerned only with relieving the newcomer of his last few pennies. Patrick Dunae, in his book *Gentlemen Emigrants*, tells us a different story — that of the influential and well-to-do whose reasons for leaving were every bit as personally pressing but who travelled first-class and who expected their names and connections to open doors to the opportunities the colonies were supposed to possess.

Dunae describes two groups of gentlemen emigrants to Canada. The first came to Ontario after the Napoleonic Wars and there joined members of the frontier élite to help shape Compact attitudes and institutions towards continued loyalty to Queen, Church, and Empire. These people, of whom the Moodies, the Stricklands, and Laytons receive special mention for their visibility, are only background to the much larger group, 45,000 in all, which crossed the Atlantic later in the century, many of them bound for Western Canada. While some left the "Old Country" voluntarily in search of adventure and wealth, most were simply refugees from changing circumstances which had deflected their lives from the path to stature and comfort at home for which their public school educations had prepared them. They were the "younger sons" of landed gentry who earlier would have been as secure as their elders but who became a much-discussed problem when agricultural depression made them redundant at home. Their futures darkened further when increased competition in the church, the legal profession, and in medicine, and the reduction of patronage in the civil service meant that breeding and contacts were no longer enough to guarantee a ticket to security. Their only recourse was to emigrate to Canada or to one of the other Dominions since they knew that they would not be leaving Britain.

The naïveté of these earnest young men was reinforced by misinformation about what to expect and was exploited by provisioners and "educators" at home and in Canada who promised them the goods and training necessary to conquer the wilderness which awaited them. They also found upon arrival that their elegant letters of introduction meant little in the search for positions of wealth and prestige. Nevertheless, they found themselves much in demand in certain circles not only for the money and culture they could provide but also because they could strengthen the Anglo-Saxon grip on Canadian affairs.

Most gentlemen emigrants adapted to their new conditions, some more gracefully than others, although few of them sought refuge in occupations unworthy of gentlemen, like homesteading. Therefore they could be found in later years breeding livestock and fruit trees in British Columbia, ranching in Alberta, and occupying mid-range civil service positions throughout the region. Only a minority became serious playboys like those who frequented Cannington Manor in its heyday, or the famous remittance men who insulated themselves from their harsh and hostile environment with booze and debauchery bought with the periodic payments from indulgent parents. Nevertheless, the visibility of their antics meant that in time all upper class British emigrants became identified with them in the public mind.

*Gentlemen Emigrants* results from Dunae's perceived need to provide his own people with an image face-lift and "to rescue bourgeois and aristocratic emigrants from the shadows" created by the recent emphasis placed on labour and multicultural history. While he dwells on playboys and remittance men for the good stories they provide for his narrative, he argues that overall the products of British public schools contributed a great deal to Western Canada. Not only did they provide much-needed cultural life and sports

activities but in the process they also "made the Canadian West different from the American West" by their presence. This British Columbia historian and archivist, however, fails to admit that their motives were more self-serving than altruistic as they sought to duplicate as closely as possible the society, the amenities, and the privileges of British society that they had enjoyed before coming to Canada. Their "contributions" were not meant for the indulgence of all since the working class had neither the leisure time, nor the surplus capital to participate. What is more, they did not possess the necessary social credentials to belong, even if some had desired to crack the class barrier and to join the hunt, polo, dinner, and horticultural clubs the "better class" launched to create a civilized environment for themselves.

Dunae also attributes much of the political leadership available to the frontier West before World War I to that elite aggregation. Perhaps so, but in their positions of influence they also hardened attitudes towards continental European immigrants and towards those less well-endowed than themselves. In their continuing desire to maintain old boy ties and in their inability to concede that Canada was not Britain as perceived by their fathers, their legacy must also include the ethnic and class animosity which divided Western Canadian society so seriously between the wars.

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Laura Goodman Salverson, *Confessions of an Immigrant's Daughter* (Toronto: University of Toronto Press 1981).

*CONFESSIONS OF AN IMMIGRANT'S Daughter* by Laura Goodman Salverson, reissued by the University of Toronto Press in their *Social History Series*, is an important cultural document about immigrant sensibilities in Canada in the 1930s.

The immigrant communities at that time were inward looking and defensive, cherishing the parallel institutions they had developed in part as a response to Anglo-Canadian hostility towards the "foreign born." Inevitably racial and ethnic tensions between the dominant and subordinate groups were heightened by the trauma of the Depression. While inside the ethnic enclaves it was possible to deal with the hardships of resettlement with both humour and pathos, the outward stance of ethnic writers and intellectuals was to spring to the defense of their community, to bear witness to the nobility of its traditions and the heroism of the individual members, to tell the dominant culture in no uncertain terms that civilization did not stop at the British Isles, but extended both north, east, and south of the Channel.

Salverson — to be closely followed by Philip Grove — was the first "ethnic" voice in Canadian literature. Her *Viking Heart*, published in 1923, drew a vivid collective portrait of early Icelandic settlers in the west. This novel, like several subsequent ones, was essentially a historical romance, interspersed with realistic details about the lives of pioneer farmers, artisans, and their families. A strong sense of continuity with Icelandic ancestors, and the pre-industrial values of courage, honour, and loyalty they stood for, was shown to give meaning to the suffering that was the price for successful resettlement in Canada.

The novels seem somewhat overwrought and melodramatic today, but the autobiographic *Confessions* has withstood the ravishes of time considerably better. It chronicles the life of the author until the momentous event of getting her first novel published at the age of 23. Basically it is an account of the heightened moments of her past combined with comments, analysis, and reflection about Canadian realities from the perspective of someone who was poor, female, and a cultural outsider.

Salverson was born in Winnipeg in 1890, at the beginning of the mass immigration to the west. Her restless, romantic father, an impoverished gentleman farmer in Iceland, kept the family on the move for a good part of her growing years, seeking to find the place that would make good the promises of the immigration agents who had enticed him to leave his home land. Culturally the family never left the Icelandic community. Lars Goodman was a contributor to Icelandic periodicals and active in ethnic self-help organizations. The calm and emotionally sturdy Ingiborg Goodman had the thankless task of improvising homes in many foreign and inhospitable settings, between countless pregnancies and tragic infant deaths. Laura, a frail and dreamy youngster, was one of the few surviving children. She was fiercely guarded by her mother, who did not allow her to attend school until she was ten years old.

Laura grew up with two dreams: to become a teacher and to tell the story of the embattled Icelandic immigrants to a Canadian audience. Poverty and ill-health combined to make it impossible for her to attend Normal College, but she did succeed in her other major goal. After marrying another restless immigrant, the Norwegian George Salverson — between *keeping house and looking after children* and boarders in almost as many new settings as her mother — she began to write stories about her countrymen. These stories found a response among a socially conscious Canadian audience, sensitized to the plight of immigrants by such works as Woodsworth's *Strangers Within Our Gates*.

From the point of view of historical research, memoirs are about as accurate accounts of past events as novels and oral history records. They are, however, authentic and trustworthy cultural documents about the values, attitudes, and responses of individuals and their social milieu at the time they were written. *Confessions*, which received the Governor

General's Award in 1939, expresses the romantic nationalism which was one characteristic immigrant response before World War II. At best in a work like Salverson's this idealization of a mythic past went together with a sturdy sense of cultural self-worth and a refusal to buy into any easy notions of Canadianization through assimilation. (In it are the seeds of the multiculturalism of today.) But *Confessions* also reflects the bridges that Salverson like many immigrant intellectuals built with liberal and progressive Canadians. Salverson was a personal friend of Nellie McClung and feminism was also a strong ideological component in *Confessions*. She had been touched by the social protest movements in the west and when she describes the sweatshop where her father worked and her own experience as a domestic, she does it from the perspective of someone who was also powerfully drawn towards a socialist vision.

All these ideas coexist somewhat uneasily in her autobiography as they no doubt did in her life. For the modern reader, painfully self-conscious of the ambivalences and contradictions in her own psyche, the intellectual tensions in *Confessions* make it more trustworthy as a personal document. Laura Goodman Salverson seems like a spiritual ancestor a lot of us can relate to.

Satu Repo  
Toronto

Sally F. Zerker, *The Rise and Fall of the Toronto Typographical Union 1832-1972: A Case Study of Foreign Domination* (Toronto: University of Toronto Press 1982).

IN DISCUSSING INFLUENCES on Canadian labour historians writing in the 1970s, Kenneth McNaught in his 1981 *Canadian Historical Review* article suggested that there were roughly two polarized approaches to the subject as epitomized by historians E.P. Thompson and Harold Logan. In so far as Zerker's

book is institutional labour history, her work is akin to Logan's. As indeed is her style — for while her research is thorough and impeccable, her writing style is clear but can be tedious particularly when her historical analysis becomes shrouded in paragraphs of excessive detail. Nevertheless, the book is a valuable case study, if not a definitive history.

On 12 October 1832, 24 journeymen printers met at the Royal York Hotel on King Street East in the "frontier village" of York to found Canada's earliest, still-continuing trade union. This early germination of unionism resulted from a combination of skill, literacy, the high status of printers as "professionals," and a political environment which had spawned many newspapers and therefore a sizeable workforce. There were also defensive reasons for creating a union. After providing the reader with useful information about the changing printing processes, Zerker indicates that even in the earliest days, there was anxiety about the effect of technological change, which was already creating "specialists" within the trade. There was also the problem of competition from half-trained workmen employed often in small shops established by entrepreneurial printers. From the beginning then, the union was organized primarily to deal with industrial problems and less to service social and benevolent needs, although it did that too. Its major function as a craft union of skilled workers (from which flowed its power) was always to try to control the labour supply by regulating the employment of apprentices and the ratio of apprentices to journeymen.

As the union in the 1830s was weak and lost its first strike, it finally folded in 1837. Nevertheless, this first organization was useful in that it taught the printers the fundamentals of organization and trained some leaders. The Toronto Typographical Union (TTU) was reorganized in 1844, as the result of an employer combination organized to reduce wages. In this early period, George Brown, "Canada's lead-

ing nineteenth-century liberal reformer" (33) as master printer of the *Globe* became the union's greatest adversary. Zerker describes Brown as a "zealous anti-unionist" who despite his strenuous efforts, failed to unite the employers against the union in 1844, 1847, and 1854 but continued to run a non-union shop. Much of this information will be familiar to historians from earlier accounts, but Zerker's investigation proves that "Brown's record as an employer is replete with evidence that his overriding objective was destruction of the union." (86)

Zerker traces the development of the union from its beginnings as a weak local, to its affiliation to the International Typographical Union (ITU) in 1866, to its "victory" in the 1872 printers' strike and so on. In doing so — and this is what is valuable about institutional labour history — we learn how aspects of unionism which are now common practice, evolved and we are therefore given a rationale for established union practices. For example, she traces the gradual change in the printing industry from the custom of the unilateral imposition of a wage scale to a later trend toward cooperation or consultation to the still later practice of collective bargaining. From the union's point of view, the imposed method of wage settlement was far from satisfactory even when it was the union which dictated the terms. Union gains through unilateral imposition (workers' control?) could last only so long as the employer was weak. Without consultation or an ongoing relationship, the union could not depend on the employers' continued goodwill to hire union members. Consequently the union became more interested in more cooperative methods and union-management consultation occurred more frequently, although as Zerker cautions "the trend toward bilateral bargaining was not a continuous straight line progression." (47) Zerker describes how other concepts like a centralized strike fund evolved; how early unions without check-off provisions faced

constant financial problems; how the concept of the closed shop persisted as a union goal, and how techniques like boycotts and the union label were developed. All of this information is useful in understanding the role of trade unions both historically and in the present.

The Zerker study is not complete in three respects. First: not all time periods are given equal treatment. After a detailed account of the union until 1910, the book begins to fade. The 1920s to the 1960s period is dealt with cursorily. Does this mean nothing of significance occurred then? The last disastrous strike in the Toronto newspaper industry is then dealt with in great detail but in an investigative and balanced manner. Second: one gets little sense of the printers themselves — of the personalities of the leaders, of the role which pride of craft played in cementing the union. Yet their solidarity allowed the printers to discipline by ostracism, expel members for "scabbing," and insist on controlling workshop conditions. Another aspect of the ITU which is mentioned only briefly is the party system within the union. This is inexplicable — for the ITU is unique in that it is the one union which has developed almost from the beginning a recognized two-party system. Some of the Toronto local's positions which it took over the years would have been more comprehensible to the reader had they been placed in their proper internal union "political" context. Third: gradually, the theme of the international connection with the ITU comes to dominate the second half of the book. While it is an important theme, other aspects are thus necessarily neglected. Also Zerker's preoccupation with local autonomy verges on being parochial and occasionally even ahistorical.

One would think from the author's analysis that the ITU was an extremely hierarchical and dictatorial union. It is true that over time it did become increasingly centralized, but this had to do with its craft structure. While becoming more

centralized, it also became more effective in carrying out its duties. The basic power of the printers' union lay in its control of a relatively scarce skill together with the strong sense of group solidarity and both factors naturally contributed to the development of an international union. Repeatedly it becomes clear that the ITU did not question this centralization, but endorsed it, saw it as necessary, and consistently supported the administration. While Zerker bemoans this behaviour and in certain circumstances finds it difficult to understand, the local union did not. Surely the historian's job is to reflect and explain the local's perspective and then if necessary analyze the pros and cons of its actions. Of course not all international unions are as centralized as the ITU; it is no accident that the industrial international unions have always been more autonomous at the local level but again trade union structure is related to its functions.

The author implies that local autonomy was completely overridden by the international at all times. While tensions between the local and international (or national) levels are always apparent in any union, and especially in large locals which are jealous of their prerogatives and powers, the ITU has managed to maintain a system of democratic self-government for many years. S.M. Lipset's study of the ITU confirmed the development of a more centralized union with increasing power vested in the hands of the international; but to control the international there evolved a division of powers between the different levels of the union. Despite the development of an ITU bureaucracy, the locals remained in almost complete control of the collective bargaining process. They handled day-to-day grievance procedures, the administration of apprenticeship regulations, disputes over the operation of seniority rules, and discharge cases. Zerker consistently minimizes this local participation and yet it was this factor which explained the

TTU's consistent support of the international union, even when there were disagreements. In the absence of a national or regional bargaining pattern, the locals also had control over their demands. Periodically there were union-wide struggles to force employers to accept certain minimum standards throughout the industry in which the TTU participated. For example, in 1906 the ITU tried to establish the 8-hour day. All locals were assessed to aid strikes pushing for this standard in cities where it did not exist. Some Canadian locals, like Hamilton and Ottawa balked at the assessment. While the TTU went along with the assessment, the author interprets this crisis as being an occasion where the TTU lost wages in a trade-off for the eight-hour day. She recognizes that "the eight hour movement was the first round in a difficult and critical undertaking by the ITU: to lessen the wearing, often crushing effects of long hours of work" (177) and it was successful. Yet she regrets the TTU members' short-term monetary loss and even more importantly sees that "the TTU was leaving behind the practice of autonomous decision-making." (177) She suggests that the TTU might have been better off on its own. Local autonomy is all very well but it was surely not a serious incursion for the international to insist on the principle of solidarity to ensure that *all* printers henceforth would work eight hours.

In exchange for the strength and benefits of the larger connection, locals cannot always have their own way. In 1906, those locals which worked longer than eight hours benefitted most from the ITU campaign. At other times, other locals including the TTU would benefit by belonging to the larger organization. Similarly, Zerker's discussion of the 1960s seven-year newspaper strike is portrayed as proof of the disadvantages of the international connection. Her analysis of this dispute is well researched so much so that one could conclude that there was a good deal of poor leadership, bad timing,

and even incompetence at both levels of the union. When it became a debacle, the author finds it regrettable that the local was so loyal to the ITU that it did not make its own settlement (which at that point would have meant giving up all control over the workplace and technological change) rather than lose the strike. One can sympathize with the printers who were the victims, condemn the union-busting attitude of the employers, and regret the poor tactical sense of the union leadership at both levels (which Zerker does implicitly in her comparison with the bargaining tactics of the Toronto and New York locals), yet one can also understand why the international union was opposed to the resolution of the dispute in this way. It would have set a terrible precedent for other locals in the union, also battling employers over the crippling issue of technological change. The TTU and its membership understood this. One local's actions can affect what happens in the union as a whole.

Finally Zerker is critical of what she sees as institutional conservatism, rigidity, and atrophy in the face of technological change. The pace and scope of such change in the printing industry in recent years had the effect of making the skilled printer obsolete. That dehumanizing process is a problem with which even a more flexible, progressive union might not have been able to deal. The resistance of the ITU to industrial unionism and the persistence of divisiveness amongst the printing trades might have been more explicable in the Toronto situation if the union politics at the international level had been outlined. From the 1950s the printing trades unions did agree to a no-raiding pact. This jurisdictional peace was difficult to maintain amongst the industry's unions and reflected technology's dimming of craft outlines. Nevertheless, the printing trades opposed the one big union solution which the AFL-CIO (hardly a far-seeing organization) was urging for practical and quite understandable reasons. Such a develop-

ment would have completely upset the unions' pension schemes and trade regulations which the unions affected had built up for years and spent millions to defend. While the decision to opt for printing trades cooperation over industrial unionism was perhaps short-sighted, it was not without foundation.

Eugene Forsey has noted in his new book on early Canadian unions that there are few well-written, thoroughly researched specialized studies of particular unions and consequently the study of Canadian labour history is made more difficult. Zerker's book is such a worthwhile study and goes a long way in remedying this lack of material in the printing trades. Hopefully, in the near future, more such specific studies will be completed.

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Sally F. Zerker, *The Rise and Fall of the Toronto Typographical Union, 1832-1972: A Case Study of Foreign Domination* (Toronto: University of Toronto Press 1982).

ORDINARILY THE HISTORY of one trade-union local will generate little more than antiquarian interest. But for at least two reasons that is not true for the Toronto Typographical Union (TTU); its voluminous records have survived remarkably well, and Toronto printers have played a key role in the Canadian labour movement. According to Sally Zerker the history of the TTU is a microcosm of organized labour's travails in Canada. She argues that "the Toronto printers' union was essentially a successful autonomous operation growing well in its native Canadian environment" (316) when the American-based International Typographical Union (ITU) took control of it in 1890. Since then ITU leaders have been "bludgeoning the Toronto subordinate, effectively whipping away every spark of ingenuity and originality, so that what

remains is almost total conformity to headquarter's demands." (320) As we shall see, the vehemence of the author's rhetoric is sustained by neither her evidence nor her reasoning.

The book's thirteen chapters are divided into four unequal parts. In the first of the three shorter sections Zerker cites most of the traditional reasons for the founding of a local trade union. Although the role of George Brown is duly noted, she is not concerned with struggles for control over the workplace, or with the printers' artisan traditions, but rather with the evolution of collective bargaining. Because her focus is on this aspect rather than on the culture of work, she is astonished to discover that the first national printers' union in the U.S. was borrowed from the Odd Fellows. The second part chronicles the growth of the TTU and its affiliation with the ITU. She argues

persuasively that printer mobility was the chief reason for this linkage, although the TTU continued to derive more aid from local Canadian unionists than from its new American "parent." Retracing the famed Nine-Hours' Strike of 1872, she does not resolve some of the remaining contradictions in the work of Kealey, Careless, or Beatty on this important episode. Instead she is intent upon establishing the TTU's victory over the master printers, for it made them, she says, pioneers in the continental movement for a shorter workday. That sets the stage for Part III, detailing the TTU's loss of autonomy and initiative after the ITU takeover. The American organization "subjugated the wishes of the Toronto local in the interest of broader ITU considerations." (111)

At this point Zerker's argument collides head-on with her evidence. She complains that the TTU lost "the right and power to choose its own course over its own destiny." (127) But so did every other ITU local in the United States and Canada, and she offers no evidence that ITU leaders systematically discriminated



against the Toronto printers. Indeed, she grudgingly acknowledges the importance of ITU strike support to the TTU just two years after the latter's "subjugation." As a result of this help "the *News* strike was far less burdensome for the Toronto local than earlier experiences with strikes had been." (123) Her problem here is compounded by a failure to note the degree of bureaucratic rationalization and integration taking place in both trade-union and business circles at the turn of the century. Certainly artisans of that era could see which ways the winds of change were blowing, and Zerker admits that she can "find no suggestion anywhere in the records... that members [of the TTU] were concerned about the possibility of parent union invasion of local autonomy." (100) Furthermore, her argument clearly implies that "foreign domination" did not start until the middle of the 140-year period her sub-title presumes to label. Given these contradictions and the absence of sufficient evidence, Zerker's conclusions rest largely upon rhetorical assertions: "It was typical of the older, larger, and *more arrogant* ITU..." (134, italics added); "One cannot help suspecting that it was a result of the ITU influence that..." (137); "one suspects that the proposal for 4 June had an ulterior motive" (172). These suspicions may persuade some readers, but in my view they merely convert the book into a tract for the times.

The fourth part, entitled the "ITU in Control," commands about half the entire book and clearly reflects the author's interest in the contemporary period. Her conviction that the TTU would have been better off without the ITU affiliation rests upon two distinct arguments: that "losses" resulting from affiliation outweighed "gains," and that the ITU failed to account for differences between the Canadian and American environments. To substantiate the first point, she condemns the ITU for throwing its support behind striking Pittsburgh printers (166ff.) at the

expense of indigenous TTU goals. The Toronto printers, she says, had already won a shorter workday and ITU officials failed to take this fact into account. But she admits that the ITU spent more money (\$1,610,939.35) to support Toronto printers during a four-year struggle for the 44-hour week than on any American local. Her argument is undermined further by evidence showing ITU sensitivity to local conditions at other times such as during the Depression (211) and in negotiations by Toronto printers in 1945 (236). She even describes an extraordinary willingness by the ITU "to shut its eyes to [a] unique deal" (236) negotiated by Toronto printers that was contrary to ITU laws. Yet Zerker is annoyed that Toronto printers remained unwaveringly loyal to the ITU. So much for "foreign domination!"

If the author cannot prove ITU discrimination against the TTU, she might have retrieved her argument by demonstrating that ITU leaders systematically failed to take unique Canadian economic, social, or political conditions into account when formulating ITU policies. Most of those reading this assessment will know that I am sympathetic to such an approach. But Zerker devotes only four pages (200-4) to a woefully undeveloped and unconvincing assertion that a printers' strike in the 1920s was inappropriate for Toronto to bear because the Canadian wheat economy lagged behind American prosperity. Nowhere does she show how Toronto printers were vulnerable to international wheat prices; instead she contradicts herself a few pages later (227) with an observation that the newspaper industry did not follow business cycle fluctuations. Thus while asserting that Canadian trade-union locals were inevitably exposed to national constraints and opportunities, distinct and different from American conditions and needs, (222) Zerker fails to show what these differences were or how they put Toronto printers at a "serious disadvantage."

Although the book does contain

worthwhile descriptions of the printers' trade at various times during the past decades, its argument is fundamentally ahistorical. The author concocts an artificial battle between the ITU and TTU and ignores the real struggles between printers and publishers. She reveals little understanding of the extraordinary control which printers exercised over their workplace, or that the ITU's authority (in Greg Kealey's words) "flowed from the shop floor up." Thus the real meaning of the 1962 strike stems from the printers' loss of this control over the latest computer-driven machinery. Over the years Toronto printers understood that waving the flag did not put more bread on the table, and the author never satisfactorily explains how they could have been better off by doing so.

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Paul-André Linteau, *Maisonneuve — Comment des promoteurs fabriquent une ville*. (Montreal: Boreal Express 1981).

NOT MANY QUEBECERS — let alone Canadians — are aware that between 1883 and 1918 a city by the name of Maisonneuve rose and grew just to the East of Montreal. Still fewer people know that while at the peak of its development, Maisonneuve was one of the most important industrial centres in Canada. Now, thanks to Paul-André Linteau, we not only have a history of this "vanished city," we also have an excellent piece of scholarship which sheds important light on the urbanization and economic growth of the larger Montreal region. Linteau takes us step by step through the various stages of the city's short history — from the successful attempt to charter a semi-rural territory into a separate municipality, to the rapid unfolding of a strategy of urban and industrial expansion, and on to the city's golden period when the local

elite undertook a programme of beautification which has left lasting marks on the architectural physiognomy of the area. The story ends, as one might expect, with the city's extinction as a separate municipal entity, when in 1918 Maisonneuve was annexed to Montreal.

In a way, it should come as no surprise that in a period of rapid industrial and urban change the administrative map of the Montreal region — one of Canada's key poles of development — would be continually remade so as to adjust to those changes. In this sense, the territory of Maisonneuve, just as that of other villages surrounding Montreal, was destined to be encompassed by a wider process whose nerve center resided in the Quebecois metropolis.

Linteau's great merit is that of having rendered intelligible what normally appears as a highly impersonal process. He shows, for instance, that if Maisonneuve rose when it did, and if it took the urban and economic course that it did, it was largely due to the actions of a group of economic operators who knew how to profit from the conversion of semi-rural space into prime commercial and residential land. These are, in fact, the major protagonists of the book. Linteau identifies them, traces their social and economic backgrounds, and tries to situate them within the class structure of the time. The analysis of these historical actors unfolds along two well-integrated lines. We have a functional examination of the precise role of urban promoters and *capitalistes fonciers* within the larger framework of capitalist growth; at the same time, the analysis takes into account the ethnic make-up of this group — in short, the fact that they were French Canadians. The conclusion at which Linteau arrives goes a long way to challenge traditional assumptions concerning the place of the French Canadian bourgeoisie in the economic development of Quebec. The author in fact demonstrates, quite convincingly, that the promoters and landed capitalists

who "made" Maisonneuve belonged to a remarkably dynamic and progressive sector of the bourgeoisie, and as such they were perfectly in tune with the modernizing trends which were sweeping through most of urban America at the time. They knew how to use the political machinery in order to push through their strategy; they saw the advantage of allying themselves with industrial capital in their attempt to turn their city into the Pittsburgh of Canada; they knew how to promote and provide public services which responded to the dual need of efficiency and private accumulation. They could even assume a pro-working class image, advertising Maisonneuve as "a city of workers, governed by the workers and for the workers." (176)

At the same time, Linteau adds a nice touch of cultural history when, after analyzing the architectural style of the various works of beautification undertaken in Maisonneuve, he comes to the conclusion that their authors were *parvenus*, trying to "show off their recently acquired wealth. Their cultural vision was very limited, unable to go beyond the importation of foreign models, mostly American." (220) This the author sees as a particular trait of the French Canadian bourgeoisie of the "*Belle Epoque*" — the trait of a class which saw its ranks swell rapidly at a time of major economic expansion.

But if *Maisonneuve* pivots around its leading protagonists, with their aspirations, schemes, and political manipulations, it also unfolds within a framework made out of impersonal forces and elements. For urbanization and industrialization can hardly be understood in all their complexity and interrelatedness unless the physical and geographic characteristics of the space are duly taken into account and linked — both conceptually and historically — to the wider transformations occurring in the region. In the case of Maisonneuve, in fact, Linteau

shows to what extent the potential for urban and industrial growth rested on its particular territorial location in relation to Montreal's economic activity. The construction of shipping facilities on its southern shore, the attraction of industrial plants, the extension of urban transportation networks, were all moves which could be conceived and concretized on account of the physical features of Maisonneuve's territory. These features are skillfully dissected with the help of maps, charts, and photographs, making it possible to gain a visual understanding of urbanization and, perhaps more importantly, a spatial perception of land-based accumulation.

Whether or not Maisonneuve deserved the title of "the Pittsburgh of Canada," the fact remains that by the beginning of the 1910s the city had built a solid and diversified industrial base, and attracted a large residential population. Maisonneuve was far from being "governed by the workers and for the workers" — but it certainly was a working-class city. In one chapter entirely devoted to this aspect, Linteau examines the social and economic characteristics of the city's labouring population, and demonstrates that wage-workers were certainly not the ones benefiting from the industrial growth their city was undergoing. He also provides statistical evidence to show that a large majority of Maisonneuve's labour force was made up of French Canadians who had moved to the city from other parts of Quebec. Unfortunately, the lack of adequate labour sources has prevented the author from infusing more life into these pages. One is not sure whether a workers' movement came into being in Quebec's second largest industrial city and how it might have been affected by the particular pattern of urbanization and industrialization which developed there. Yet, the emerging outline of Maisonneuve's social structure will certainly serve as a basic starting point for labour historians interested in pursuing further this line of

analysis. Also, for this reason *Maison-neuve* may be viewed as a pioneering work in Canadian urban history.

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Hugh Johnston, *The Voyage of the Komagata Maru: The Sikh Challenge to Canada's Colour Bar* (Delhi: Oxford University Press 1979).

DRAWING ON THIS narrative, a talented script writer could prepare an exciting documentary. Before the file on the voyage of the *Komagata Maru* from Hong Kong to Vancouver and back to India was closed, at least 23 people had died in a riot in India, four members of Vancouver's Sikh community had been murdered, and W.C. Hopkinson, a Canadian immigration officer (who was also on the payrolls of the United States Immigration Service and the India Office), had been assassinated in the Vancouver Court House. The central drama of the *Komagata Maru*, however, took place in Vancouver harbor where, for six weeks during June and July 1914, most of its 376 passengers waited while Canadian authorities succeeded in preventing them from landing as immigrants.

The voyage of this chartered vessel of Japanese registry and its passengers — of whom about 340 were Sikhs — was of more than usual interest since it challenged Canadian immigration regulations and embarrassed the British government of India. The chief character, a Sikh entrepreneur, Gurdit Singh, apparently saw the voyage "as an act of patriotism which, win or lose, would win him recognition among nationalists in India," (25) while he demonstrated the rights of Indians, as British subjects, to visit any part of the Empire. An important part of Johnston's story rests on the existence of a secret revolutionary party, the Ghadr (Mutiny), in India and among Indians overseas. While

the role of the Ghadr and the reasons for the nationalist movement in India may be well known to Indian readers of this book, Professor Johnston does not fully explain them for those who are less familiar with the history of India. Nor does he reconcile this theme with Gurdit Singh's later and equally plausible claim that the voyage "had been a commercial venture, not a political one." (124)

In the short run, the motives of Gurdit Singh's paying passengers are clear; they hoped to reach Canada before the Canadian government plugged a loophole in the 1908 Orders-in-Council which had effectively prohibited immigration from India. They failed; Canadian officials were determined not to admit immigrants from India. Johnston is sharply critical of the officials concerned, especially Malcolm J. Reid, the immigration agent at Vancouver. Reid feared conspiracies against himself (sometimes with justification), paid little attention to the basic needs of the *Komagata Maru* passengers for food and water, and deliberately delayed the taking of a test case to court. Reid owed his position chiefly to his political friendship with H.H. Stevens, the Conservative MP for Vancouver and "a rabid opponent of Indian immigration." (19) Johnston argues that Stevens "was the man making the decisions" (50) but presents scant evidence of this. Moreover, Johnston's sketch of anti-Asian prejudice in British Columbia is almost too brief to be useful<sup>1</sup> and, by concentrating on the official record, he underestimates the intensity of the anti-Indian feeling aroused in British Columbia by the *Komagata*

<sup>1</sup> Readers will find a more useful sketch of the British Columbia to which the *Komagata Maru* sailed in W.P. Ward, *White Canada Forever: Popular Attitudes and Public Policy Towards Orientals in British Columbia* (Montreal 1978), Ch. 5. A fuller but less generally available source is Brij Lal, "East Indians in British Columbia: An Historical Study in Growth and Integration, 1908-1914." MA Thesis, University of British Columbia, 1976.

*Maru*. Thus, he is unnecessarily harsh about Reid's fear that Vancouver whites might riot if the *Komagata Maru* passengers were allowed ashore for any purpose other than immigration hearings. Reid undoubtedly suffered from racial prejudice but he could also remember Vancouver's 1907 anti-Asian riots. In retrospect, Reid was perhaps using the possibility of a riot as an argument against allowing the passengers ashore (42) but, given Vancouver's history, he was prudent to remind Ottawa of the danger.

Despite his reluctance to provide the background material necessary to draw definite conclusions from his evidence, Johnston has usefully recalled that while the *Komagata Maru* sailed out of Canadian history in July 1914, its voyage had dramatic repercussions in India and may have contributed to "a renewal of unrest." Curiously, Gurdit Singh had difficulty in getting himself recognized as the hero of the *Komagata Maru* until shortly before his death in 1954 at the age of 95. Johnston also demonstrates the severe factionalism in the Sikh community of British Columbia. A small group of westernized Sikhs such as Husain Rahim, a trader and real estate agent, desired to make money but Rahim was "an activist first." (9) Other westernized Sikhs, notably Bela Sikh who earned \$62.50 per month as Hopkinson's "chief tipster" (125) became informers. Bela Sikh narrowly escaped assassination in Vancouver but, some years after returning to India where he was also an informer, old Ghadrite terrorists trapped him outside his village and "chopped [him] to pieces." (133)

The majority of Sikhs in British Columbia apparently had no great interest in political matters in India or Canada. They were sojourners who lived frugally while seeking their fortunes as workers in sawmills and other industries where they could earn ten to fifteen times more than they could in India. (Although Johnston does not mention it, Indians in British

Columbia were often paid less than white men for similar work.)

That such discrepancies still exist between wages in India and in Canada is evident from the price of this book. For \$7.50, the Canadian purchaser receives a hard bound book of 172 pages that has been well copy-edited, almost flawlessly proofread, clearly printed on fair quality paper, with a simple but attractive and sturdy dustjacket and four well reproduced plates. The secret, of course, is that the book was published not by the Toronto, London, or New York offices of the Oxford University Press but by its Delhi branch.

A book can be judged on two standards — how well the author accomplished his objective and whether or not that objective was the best possible one for his subject. Judged by the first standard — one must assume in the absence of contrary evidence that Johnston set out merely to recount the story of the *Komagata Maru* and its passengers — the author has succeeded well. He has reported in detail — sometimes to excess — on what happened to the passengers. Yet, surely the historian must do more than merely relate the story; he must interpret, he must explain why it occurred and he must evaluate its significance. Thus, on the second criterion, Johnston's work is not entirely satisfactory. His unexplained unwillingness to present an overall thesis is especially unfortunate because he was obviously in an excellent position to present a clear argument. He has used archival material in Canada, the United States, Great Britain, and India (including some material translated from Hindi and Gurmukhi, the script used by Sikhs) and even interviewed one passenger. This is the most thorough study of the *Komagata Maru* yet published by a Canadian. Alas, by choosing a narrative rather than an analytical framework, Professor Johnston may have made life easier for some future script writer but he has sold his own scholarship short, narrowed

his focus unnecessarily, and produced a less than definitive work.

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Ian Angus, *Canadian Bolsheviks: The Early Years of the Communist Party of Canada* (Montreal: Vanguard Publications 1981).

IN A 1968 REVIEW of *Soldiers of the International*, Victor Hoar suggested that as an institutional examination of the beginnings of the Communist Part of Canada (CPC) William Rodney's book "left little to be desired" and would remain "the definitive history." Hoar began his review, as Rodney began his book, with the words and identity of Tim Buck, claiming this place of prominence for the National Chairman because of his long-standing visibility as a Communist and his presence, on 23 May 1921, at the founding meeting of the Party. Lest he appear too uncritical Hoar suggested that though important, Buck was perhaps not the sum total of the making of Canadian communism. What, he asked, of Beckie Buhay, Maurice Spector, and others?

Thirteen years is not a long time in historiographical circles, but it is time enough to shunt more than one definitive history aside. Ian Angus's *Canadian Bolsheviks* now takes its place beside Rodney's *Soldiers of the International*, a book that it draws heavily upon to go beyond. Writing not from the professorial chair, but from the unaffiliated backrooms of the Trotskyist movement, publishing his work not with the University of Toronto Press, but with the Fourth International-connected Vanguard-Pathfinder, Angus takes a fresh, politically-poised look at the degeneration of Canada's first Bolshevik Party in the years leading up to the 1931 Plenum. As a political statement, *Canadian Bolsheviks* deserves political reply. But it is hoped that its scholarly merits will not go unnoticed and something should be said of them here.

First, Angus provides an exciting, if somewhat schematic, charting of the radical currents that flowed together toward the founding and consolidation of the CPC, offering insights on the Socialist Party of North America, the Socialist Party of Canada, the left-wing Ukrainians in the Social Democratic Party, and the radical, skilled trade unionists who led labour's revolt in the immediate post-war period. While all of this is not new, some of it is, and as a package it presents one of the best short introductions to radicalism and militancy in the 1917 - 21 years. Second, on the underground years of the early CPC and its obscure founding, as well as the counterrevolutionary tide that swept over the Party in the post-1923 years, casting Maurice Spector upon the Trotskyist beach and sinking Jack MacDonald in a cesspool of compromise from which he would eventually escape, Angus is exceptionally acute. We could perhaps have been spared the 20 pages of elementary political commentary on the Stalinization of the international communist movement in the mid-to-late 1920s, a tumid tale told far better in an extended and accessible literature. But quibbles such as this cannot detract from Angus's third contribution. Notable communists previously neglected — the brother-sister duo, Beckie and Mike Buhay; Canada's leading communist theoretician/intellectual, Maurice Spector, whose Trotskyist endeavours are not, unfortunately, traced into the 1930s, when they were of greatest significance; and the archetypal proletarian, patternmaker "Moscow Jack" Macdonald — get a part of their due in this book. Rodney and others have given us the skeletons of these and other early leaders of the communist movement. Angus puts some flesh on the bones, and if the figures still remain shadowy they now have, at least, some substance. One communist, however, has managed to perpetuate a historical image that is larger than life.

Tim Buck, a Stalinist's Stalinist, has been at the very centre of the history of the

CPC, official and academic, for over half-a-century. Theoretically wooden, politically opportunistic, loyal to the administration, unscrupulous but outwardly affable, Buck may well have missed his time and his calling: he had the qualities contemporary academics are made of; in the university, too, he would have been a chairman, if not a dean. As it was he earned the distinction of outlasting other western communist leaders, none of whom had the staying power of Buck or the capacity to fashion a mythology in which their personalities were inextricably entwined with every facet of party life and development.

Angus explores this "making of Tim Buck's Party" more fully than other sources, and in the process he takes this superman of the left and cuts him off — at the knees and at the neck. We learn that Buck was not, in fact, a part of the historic initiatives leading to that much-cited founding convention of the CPC in 1921. Furthermore, Angus suggests, Buck was not present at that spring meeting that formally created the Communist Party, a view that was also propounded by Spector and MacDonald who, with a score of others, were there. Buck's visibility and prominence as a communist actually date from the formation of the legal Workers Party in February 1922. Before that he had an honourable history as a trade union militant, but the evidence suggests that his involvement with Marxism was peripheral. But in the post-1929 years, when Buck wrestled control of the Party, finally and unequivocally, away from its founders, this history was insufficiently heroic. As a consequence a new history had to be written, marking the rise of Canada's preeminent Marxist revolutionary, falsifying and fabricating with each new text. In the process, as Angus reveals, Buck and his "official" Marxist followers get so much wrong that it is impossible to trust a word they have written. Buck's historical credibility, always questionable, hits a new low with Angus's relentless review of

the contradictions, mistakes, and slanders that permeate his writings on Canadian communist development.

Having accomplished all of this Angus gives us more to chew on than many a scholarly study. But our chewing, on this book, is more than an academic exercise. *Canadian Bolsheviks* was written to fight against the stream, "against the overwhelming influence of Stalinism and Social Democracy in the workers' movement." Ultimately it should be applauded and evaluated on this eminently political ground. And while there is much to cheer there are moments that cause one concern.

The curt dismissal of the Socialist Party of Canada's "impossiblism" and "sectarian" refusal to join the Second International, even though, two pages later, we read of the collapse of the International in social democracy's orgiastic warmongering and abdication of socialist responsibility in 1914, is just such a moment. So, too, is Angus's brief discussion of the Third Period (1929-35). He correctly argues that the CPC, following Comintern directives, entered into years of adventurist ultraleftism that led the workers' movement in suicidal directions. Along the way he takes a swipe at the CPC's trade union work, and presents an entirely negative view of the Workers Unity League. "The only lasting achievement of the WUL," claims Angus, "was the isolation of thousands of left-wing labour militants from the mainstream of Canadian labour." So intent is Angus to locate all positive accomplishment in the years of the early 1920s, that he cannot concede any achievements in the 1930s. The WUL, for all its flaws (and these were many), did indeed help to pioneer in industrial unionism, the organization of the unemployed, and new forms of workplace resistance. It heightened militancy in a period of labour quiescence, and while much of this ended in defeat we cannot embrace the stand of the cynic. Somewhere between the CPC's manipulative use of the working class, whom it

would have to fight regardless of the possibilities of victory, and Angus's omniscient assertion that the WUL "had no life other than that which the Party gave to it," lies an appreciation of the grievances and material realities that moved workers and communists towards dual unionism, recognition of the courage and heroism of the rank-and-file communists, and balanced assessment of the experience of struggle that severed the ties that bound many to the capitalist order.

There are other questions one might raise of this study — the lack of analysis of MacDonald's futile attempts to preserve party unity as Trotskyism and the Left Opposition were being hammered into retreat around the world, or the ahistorical and idealist attempt to locate part of the explanation of labour's defeat in 1920 in the innate conservatism of human nature — but these are calls to clarify and expand. The real moments of disagreement here, and they are rare indeed, are political, moments like the hurried distancing from so-called "impossibilism" or the quick rejection of the practice of dual unionism in the Third Period. Here one detects a willingness to gravitate towards a reformist stand, pushing analysis of parts of radical and communist success and failure into the background. These are moments read against the counterstream of Angus's narrative. They do not carry us so much towards scholarly critique as they pull us into a formulation of political strategy and practice. It is a welcomed book that achieves this, and *Canadian Bolsheviks* demands attention from all of those professing interest in the history and nature of Canadian communism.

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Rolf Knight, *Along the No. 20 Line: Reminiscences of the Vancouver Waterfront* (Vancouver: New Star 1980).

KNIGHT HAS GIVEN US a picture of Vancouver as experienced by a variety of

working people of all ages some 30 to 40 years ago, when he himself was in his early teens. This is a very visual book. We start with a ride on the Number 20 streetcar from the Highland District of Vancouver East right down to the terminal dock. We are treated to vignettes of sections of the city and their inhabitants. We see foundries, refineries, and slaughterhouses. We see the loggers' area around Carrall and Cordova. We see the old men living without pensions in bunkhouse-like buildings along Pender Street. The author introduces us to some of these men and comments that "they were of a generation which had been part of the great transformation, of the construction of the modern world. Many had participated in the final phases of the great migration to the New World; their muscle and brain had helped lay railways which bound this continent together." He takes us down to the railway yards and spur lines. We hear parents say, "Don't climb on the box cars." Then the car comes to the 200 boathouses scattered around Burrard Inlet. These houses, built over the tidal foreshore, were not under the jurisdiction of local government. Inhabitants held no titles, paid no taxes, and received no water, sewer, or other services.

The author also has us spend time with the Gainer family and their neighbours there. He describes how people collected water, scavenged firewood, built terraced gardens, lived, and raised their children. Of course, in the 1950s there was a newspaper campaign emphasizing the unsanitary nature of this settlement, and the houses were soon demolished.

Part II is less visual and less analytical, but full of the physical sensations of work. Knight gives excerpts from the lives of nine people. Frank White talks of early trucking between the Fraser Valley and the city. Jean Trebett talks of her days as a plate fitter in the Shipyards during World War II and her dismissal at the end of the war. Ken Barker was desperately seasick on his first trip to sea. Phyllis Knight, the



author's mother, describes how in her late 40s she worked in a sausage plant: "Always wet and cold and drafty and with your hands covered in brine; walking in and out of cold lockers." Some of the excerpts in Part II have been published before in Knight's works. Nevertheless, there is a nice balance of different kinds of experience here.

Knight also has a strong sense of the world and its history as made up of the experiences of all people. Of the Oriental hospital, he writes: "On a hot summer day you can see the patients crowded around the open windows or on a large balcony in the rear. It's a warehouse of memories totally unknown to most of us."

Part II of the book looks at the historical developments that produced the city that the author and his friends knew in the 1940s.

Apart from the labour of a few hundred thousand working people, Vancouver was the creation of the CPR. Locating the western terminus of that railway on the outer harbour of Burrard Inlet changed a sawmill hamlet into a booming boosterville and then into the major Canadian port on the Pacific.

Unlike a number of the smaller industrial and mining towns of the B.C. coast and interior, Vancouver did not develop as a solidly working-class city. Its waterfront sections and campworker district were, in part, an extension of the resource industries stretching throughout the province.

That Vancouver was not a series of "urban villages"; it was not like the stereotyped folksy and colourful working-class districts portrayed in some accounts. . . . Many of those who lived and worked along the waterfront and those who passed through from camps came from a wide variety of backgrounds. It was a polyglot population. Local allegiances often went hand in hand with extra-national loyalties. If there were often contradictions in these enthusiasms, they were certainly more vital and more outward looking than the colonial patriotism officially promulgated.

The book also includes a dozen or so photographs of Vancouver and its working people in the 1930s and 1940s.

Part I, to my mind the best part of the book, is full of sociological insights. My favourite section is Knight's account of the Vancouver sojourn of the logger. The author describes the hierarchy of loggers' hotels, the hotel-keepers holding mail, messages, and a suitcase containing the moth-balled city suit for the visitor. Knight describes the restaurants, the bars, and the very necessary exchanges of experiences and tips between people who had been in different camps. As Knight points out, all this has changed. Conditions have conspired to make it easier for families to live in the interior. Now most loggers live where they work with their families, and those hotels, restaurants, and bars, that served that particular clientele are gone or changed. This is part of the broad phenomenon of the decline of the family-less migrant worker in North America. Factors such as unionization, job security, the automobile, and greater affluence have allowed more men both to form families and to take them with them where they work.

There are many touches of humour. For example, Knight shows how the CPR's dominance as a major landholder in Vancouver was mirrored in the schoolyard joke. Question: "What's the difference between the CNR and the CPR?" Answer: "Canada owns the CNR and the CPR owns Canada." There are also poetic touches. I particularly liked the descriptions of the different varieties of smoke produced by the different kinds of coal used in Vancouver.

My one criticism is the lack of background material provided. Although Knight says explicitly towards the end of the book that *this is the task of others, I do feel that he should have provided a little.* As it stands, the reader is presumed to understand B.C.'s dependence on forest products and the isolation of the interior. The reader is also presumed to understand a great deal about the growth of the city. A summary with a few short tables in an appendix would have given the reader

from elsewhere a clear picture of the economy and the changes Knight has described so well. More data might also have been illuminating for Vancouver people. People who live in the midst of change are not always aware of the magnitude of what has happened. They just know that things are not as they were. Knight does provide maps, but maps are not enough.

Knight does not argue about the importance of the working-class experience. Simply by presenting people's accounts of their lives and his own distillation of his childhood experiences he shows how working people live and how they are treated in this society. Though he scoffs gently at his history teacher's "curious (largely mythological) pass at Captain Vancouver, . . . Colonel Moody of the Royal Engineers and General Douglas of the HBC," he clearly has a great love for and hope for a different kind of history.

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Craig Heron, Shea Hoffmitz, Wayne Roberts, Robert Storey, comps. *All That Our Hands Have Done: A Pictorial History of the Hamilton Workers* (Oakville, Ont.: Mosaic Press 1981). (In conjunction with the Office of Labour Studies, McMaster University).

IF "ALL THAT OUR HANDS HAVE DONE" had been written in the traditional mould, no doubt we should have had a story of the growth of collective labour action in the Hamilton area from the Nine-Hours League to the steel strike of 1946. Sprinkled amongst the pages, or more likely grouped formally into "Illustrations," there would have been a handful of selected photographs of union leaders, a foundry interior, a couple of captains of industry perhaps, and a strike picket-line or street march panorama. We would have digested data derived from government enquiries, assessed the content and pre-

judices of press reports, measured the interplay of worker-employer relations from committee records, political correspondence, and the odd personal journal (most probably belonging to an employer's wife), sifted through union and association broadsheets and pamphlets, examined the utterances and resolutions of the city fathers and, if the authors had been exceptionally assiduous, even read some transcript from a radio programme or taped interview. The result most likely would have been a cool academic study or at least a balanced account attempting to get close to reality.

In fact, *All That Our Hands Have Done* employs none of these traditional sources, save newspapers, and concentrates instead on two largely unused and often abused sources — photographs and taped recollections. They constitute part of the "new methods, new sources, new questions, and new mutual relations between researchers and the subjects" platform which heralds the "new field" of labour history. Well and good, even if claims of such novelty ring a trifle hollow in realization. Labour historians are at the forefront of attempts to document more closely the lives of working people through innovative research and both the photograph and the taped recollections offer exciting research possibilities. "Pictures," the authors observe (and presumably they are using the umbrella word for photographs), "can add a new dimension to our understanding of Canadian history." And indeed they can as long as no greater virtues are attached to the photographic record than to any other forms and as long as the context and method of its creation is properly researched and understood. On this score, the four-page introduction is quite exemplary: "only when we understand how and why the camera was used can we come to terms with the range and focus, the limits and perspective, and potential of this book." No previous study on a broad historical theme has been quite as considerate in its cautions on

the interpretation of the photographic record as this one. The same plaudit cannot be delivered on the use of taped interviews which have their own peculiar characteristics and complexities. About this record, although it was used fairly widely throughout the work, the authors are silent. That is unfortunate because this absence of scrutiny diminishes the impact of the fulsome comments on photography.

A further detraction on this score is the total elimination of source citation, even of the most primitive variety. Despite a listing of institutions, companies, and individuals from which photographs were copied for publication, and a note that taped interviews were lodged in the McMaster University Archives, the reader and more particularly the researcher has absolutely no means of considering the creator of the record, the time sequence of its creation, nor the context in which it was conceived. As with any other documentary form, it must always be possible to check and re-examine sources. This is even more true of a volume officially presented by a major university programme as an "historical study on work, workers and work organizations in Hamilton." Presumably such omission is an oversight, though it does rather suggest that the authors are not practicing exactly what they preached in the introduction.

Linked to these concerns is another, slightly more perverse, matter. "*All That Our Hands Have Done*" is unabashedly full of missionary zeal, most of which is well-placed and promotes the most human of responses. Few would argue that working people's lives were (and often still are) controlled and manipulated by titanic technological and economic forces, few would not stand shoulder-to-shoulder with any working man, woman or child who struggled to retain or gain dignity, respect, and equity. The workers and dependants of Hamilton's foundries and factories are as entitled to as much of all such qualities as any other worker, urban and rural. Nevertheless, running throughout this book is a celebratory tone focus-

ing sharply on the union victories emerging from the 1946 strike. The authors contend that the episodes they have selected for part two, "The Making of a Union Town," are a natural evolution from the recognition of workers' hardships within day-to-day experience in the plants and were little affected by external influences. And yet on two major episodes alone, the introduction of profit-sharing at Dofasco in 1938 to counter union activity after the Depression and the drawing of women into industry during wartime, not only were working people and unions directly affected by outside forces but were often divided amongst themselves. Again, there is only bare mention of the terrible union conflicts between the craftsmen and industrial unionists, which were tearing apart the American labour movement and had inevitably affected Canadian organizations in the 1930s. Perhaps these observations are carping but they do reflect a feeling of uncertainty engendered by a volume which deals with the Hamilton labour movement in such a crusading, progressive fashion and exhorts the reader "to develop a better idea of what remains to be overcome in the future." The question that still needs asking, notwithstanding the best of intentions, is: has the use of unattributed photographs and taped interviews led to less of an historical study and rather more of a polemic? As a study, a much more rigorous organization of material is needed — one which certainly avoids such broad categorization of "Life and Labour." It is probably doubtful also that Hamilton's labour movement could be presented as an historical study using photographic and oral records primarily. If Hamilton and its work-force can be rated ambitious by the authors of "*All That Our Hands Have Done*," however unselfishly, their book rather deserves the same label.

There are nonetheless some fine photographic images: a wrecking crew on the top of the James Street Incline Railway building, a group of stone-cutters posing as if for J.A. Martin, an overhead

view of the time and motion man at Westinghouse, a bevy of cardplayers chuckling ruefully over a twist, men dwarfed by the ovens at Dofasco, women levering rail sections, peeling tomatoes in the back yard or standing in the sweat shops, and so on. Most are not only evocative, a few provocative, but are highly instructive too. The accompanying text attempts to address each photograph, though not always directly. For much of the time, the text amplifies what the eye encounters by sketching in further detail or providing contemporary and hindsight commentary. Observations are generally lively and full of interest but there is less care than there should be in noting time contexts. A later image is sometimes used to allow comment on an earlier situation without explanation. The costs of printing so many photographs, usually in near actual size or better, are formidable so the loss of quality and rather black pancake effect throughout this book cannot be unduly faulted. Towards the end, prose takes over from imagery and design falters alarmingly in the final burst of photographs from the 1946 strike.

"*All That Our Hands Have Done*" is an eminently engaging and passionate book. It is easy to feel waves of anger, of comradeship, of impotence, of respect as images pass by. And yet, reflecting on the more ambitious claims made for the volume, an uneasy thought lingers on. This book is a collection of photographs on a broad theme in a specific city, it is not a historical study of the Hamilton labour movement, the history of work, or the relationship of "the details of daily, working-life to the processes of industrial change."

Gordon Dodds  
Manitoba Provincial Archives

Meg Luxton, *More Than A Labour of Love: Three Generations of Women's Work in the Home* (Toronto: The Women's Press 1980).

"HOUSEWORK IS NOT A respectable subject for study," stated Ann Oakley in 1974.

But even as she wrote, her statement was becoming less and less true, at least in some circles. Under the impact of the second wave of feminism, the work of women in the home became the subject of considerable serious and often contentious study. Indeed once the left began to interest itself in the Women's Liberation Movement, housework was elevated to "domestic labour" and a daunting "domestic labour debate" ensued as socialist theorists attempted to adapt the concepts of Marxist social and economic analysis to the work of women, particularly working-class women, in the home.

Three main questions are generated by an investigation of housewifery: 1) what does the job involve; 2) why is it overwhelmingly done by women; and 3) how does it relate to the larger economy. Ann Oakley's classic studies, *The Sociology of Housework* (1974) and *Women's Work: The Housewife, Past and Present* (1974), concentrated on the first two questions while arguing for the historicity of the modern housewife as a role which emerged first in a housewife-supervisor form among the urban middle classes in England during the nineteenth century and then changed into an almost society-wide housewife-worker form as the primacy of domesticity for women spread also to the urban working class. In contrast to Oakley, the contributors to the "domestic labour debate" have addressed themselves principally to the third question, concerned as they are with determining the relationship of women's work in the home to industrial capitalism. With a flurry of distinction-chopping worthy of the subtlest thirteenth-century scholastics, Marxist thinkers have worried such bones of contention as whether the labour of housewives is productive or not, whether it produces use value or surplus value, and

whether it constitutes a mode of production all its own.<sup>1</sup>

Bonnie Fox's *Hidden in the Household* (1980), one of two books on the subject of housework recently published by the Toronto Women's Press, contains a collection of Canadian contributions to furthering the domestic labour discussion. Descending from their high level of abstraction, Meg Luxton's *More Than A Labour of Love: Three Generations of Women's Work in the Home*, the second volume in the Women's Press Domestic Labour series, is more accessible, seeking as it does to anchor the theoretical in an empirical world of real women in real kitchens and real bedrooms. Luxton describes her book as a general analysis of the work women do in the home "based on a case study of three generations of working-class housewives" in the northern Manitoba mining town of Flin Flon. (12)

Between October 1976 and December 1977 Luxton gathered the data for her study from questionnaires, participant observation, and interviews of 100 women grouped according to the generation in which they had moved to and settled in Flin Flon or married and set up house there. She made sure that the 100 included women from all three generations (Settlement, 1927-39; Growth and Development, 1940-59; and Stabilization, 1960-77), some women who had mothers in the first generation and/or daughters in the second or third, and 20 "who had at least one pre-schooler." (39) She also interviewed five men among whom the three generations were covered.

Luxton set for herself two goals: 1) "to locate domestic labour within the development of industrial capitalism in North America to show how it has

changed throughout the period;" and 2) "to illustrate the actual work process as women experience it to reveal the impact of those changes on women's lives." (24) In carrying out the first, Luxton works within a Marxist framework, not arguing fine points of philosophical distinction so much as using her empirical data to demonstrate certain hypotheses, particularly those Wally Secombe has developed in his two contributions to *Hidden in the Household*. In carrying out the second goal, Luxton's work is more in the tradition of Ann Oakley.

Luxton conceptualizes domestic labour, in terms of the Marxist analysis she favours, as "productive" and "reproductive," and identifies its "product" variously as "labour power" (reproduced on a daily basis), "human beings" (reproduced on a generational basis), and "life itself" (reproduced presumably on both a daily and generational basis). (19, 21, 203)<sup>2</sup> So defined, domestic labour acquires stature as "one of the most important and necessary labour processes of industrial capitalist society," (13) "the necessary complement of the wage labour/capital relationship." (17) Although they did not use the same terminology, some of the women Luxton interviewed certainly indicated an awareness of the importance of their domestic work. As one said, "But when I think about all those thousands of other women all doing the same thing, then I realize I'm not just making porridge. I'm part of a whole army of women who are feeding the country." (13)

What Luxton's analysis fails to address is why women are stuck with the domestic jobs. It does, however, encourage a 24-hour conception of the economy of the working-class household, a perspective which helps to draw out into the open the existing sexual division of labour

<sup>1</sup> For an excellent summary and assessment of some of the major contributions to this debate, see Eva Kaluzynska's witty and incisive "Wiping the Floor with Theory — a survey of writing on housework," *Feminist Review*, 6 (1980), 27-54.

<sup>2</sup> For Luxton, then on p. 117, to speak of "Housework" as "the most tangible 'product'" of the housewife's labour is confusing.

between wage work and domestic work, the endlessness of the housewife's labouring day, and the economic importance of her contributions to family subsistence. From this viewpoint, for instance, as Ruth Milkman has argued with respect to U.S. women in the Depression,<sup>3</sup> women can be seen as "intensifying their domestic labour" to make ends meet during economic hard times by such methods as taking in boarders, remaking clothes, canning, and shopping more thriftily.

Luxton's analysis combines especially well with her empirical data to show how the husband's position as primary breadwinner and "owner" of his wage, structures a "petty tyranny" into household relations. (65) His economic responsibility and her economic dependency obstruct their meeting on a basis of freedom and equality. He orders and demands, she manipulates and nags. His letting off steam can escalate to wife beating; out of self-defence and protection of the children, her job expands to include tension management.

In her descriptive analysis of the "work processes" of domestic labour, Luxton, following Oakley, has found that basic components of the housewife's job, like housekeeping and childrearing, are often incompatible. Furthermore, according to Luxton, intensified modern mothering is fraught with contradictions. Luxton uses the distinction between "production time" (a measure of "the duration of a task from start to finish") and "labour time" (a measure of "the specific period during which a worker is actually expending labour") to give new meaning to the proverbial knowledge that a woman's work is never done. (20) As long as the wife is primarily responsible for a household's domestic labour, the 24-hour pro-  
<sup>3</sup> Ruth Milkman, "Women's Work and the Economic Crisis: Some Lessons from the Great Depression," in *A Heritage of Her Own: Toward a New Social History of American Women*, ed. by Nancy F. Cott and Elizabeth H. Pleck (New York: Simon and Schuster, 1979), 507-541.

duction time of many of its components (such as child rearing, for instance, or general management) guarantees that, in contrast to the sharp demarcation between work and leisure experienced by the husband, the wife's work is continuous and endless. And that remained true among the women Luxton studied even when they took on paid jobs outside the home.

Despite Luxton's stated intention to study development and change over time, her account is essentially static. When the past is considered, it is presented, not as dynamic process, but rather as a series of fixed moments in time, like snapshots, from the three different generations. Nevertheless, these juxtapositions sometimes bring out striking contrasts, such as that between the weekly schedule of housewives of the first generation (Monday — Washday, Tuesday — Ironing) and the incessantly repeating daily schedule of housewives of the third generation. Luxton's time budget studies indicate that, while technology has removed some of the back-breaking labour, it has also effected the "deskilling" and the "despecializing" of housework, thus making it all the more fragmented and repetitive. (131)

Luxton in a subsequent article in *Atlantis* (1981) claims that the working class households she studied in Flin Flon "are typical and representative of working-class households elsewhere in Canada." This could well be so at a certain level of generalization, such as when she concludes that "The high level of responsibility, the isolation, the economic dependency and the endless tension managing all combine to make domestic labour a high stress job." (197) But could one not draw the same conclusion about middle-class housewives? Furthermore, some aspects of the situation of Flin Flon working-class housewives would appear to be peculiar to a primary resource company town; and others, such as the women's "dominant complaint... that they could not control their own fertility" (94), could be attributable to special cul-

tural or religious features of the community. While the "underdevelopment of secondary industry and social services" and the "rigid sexual division of labour within the community" might indeed provide an archetype of "the relationship of capital, wage labour and domestic labour," (25) the archetypal is not necessarily representative. Indeed Luxton might have advanced our understanding of the special oppression of housewives all the more by paying greater attention precisely to those oppressive aspects of the relationship between capital and domestic labour characteristics of or exaggerated in a one-company mining town. Take, for instance, the hiring policies of Hudson Bay Mining and Smelting Company. Luxton points out that the restricted employment opportunities facing Flin Flon housewives result not only from the "underdevelopment of secondary industry and social services" but also from the Company's refusal to hire women. She also points out that the Company has from the beginning sought a primary corps of labourers made up of married, family men. The connection between those two policies and the implications of that connection for women could have been more fully examined, for the latter policy is clearly predicated not only on women's labour in the home, including the labour of keeping the man at work by giving him the responsibility of a family, but also on the availability and willingness of women for a particular kind of marriage.

Socialist feminist historians, such as Gail Braybon,<sup>4</sup> would argue that that hiring policy is also premised on the collusion of male workers and male-dominated unions. But that dimension of working-class women's oppression is largely missing from Luxton's study. While she sometimes shows working-class men as the direct instruments of the oppression of working-class women, particularly in her

discussions of sexuality and family violence, and while she acknowledges that "the male contempt for women... expressed through physical violence" cannot all be traced to "the various power struggles that occur between men and their work," (67) the concept of patriarchy does not appear in her book. Instead the emphasis is on class solidarity. Although she states that "men lose some of their privileges and services when their wives go out to work for wages," (197) she tends to interpret the male resistance to change as based in economic structures rather than in a male supremacism which crosses class lines. Similarly when she is listing the fronts on which "the militancy of working-class women proceeds," (225) and includes fighting to end sex segregation in the paid labour force and the unequal sexual division of labour within the home, she does not speak of working-class women's struggle as in any way in opposition to working-class men but rather as leading to a healing of the rifts between the sexes.

The fact of women's oppression is a fundamental assumption of Luxton's work. But that assumption did not always square with the women's perceptions of their own lives, as sometimes revealed in the words of the women themselves which Luxton liberally quotes throughout the text. She confesses that through her study she came to understand "how painful and humiliating it is for women to admit that they are oppressed." (205) It is clear from her final chapter "Bread and Roses" that she puts faith in knowledge as the first step to liberation. And in her view such a step for housewives must be "to recognize that their domestic labour is more than a labour of love," that it is in fact a labour fraught with distinctly "oppressive qualities." (204) Although placing too exclusive a burden of explanation on industrial capitalism, *More Than A Labour of Love* goes some distance toward facilitating that recognition through its sensitive dissection of the working conditions as well

<sup>4</sup> Gail Braybon, *Women Workers in the First World War: The British Experience* (London: Croom Helm 1981).

as the tedium, self-abnegation, guilt, and frustration experienced by Flin Flon working-class housewives.

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Jonathan F. Wagner, *Brothers Beyond the Sea: National Socialism in Canada* (Waterloo, Ontario: Wilfrid Laurier University Press 1982).

THE 1930s. THE AUTHOR OF this first scholarly account of Canadian Nazism points out, spawned not only a number of parties such as the CCF, Social Credit, and the Union Nationale that were eventually integrated into this country's system of government, but also the fascist movements of Adrien Arcand and William Whittaker as well as the *Deutscher Bund Canada* (German-Canadian Bund) which "represented an extreme protest against the economic chaos, the intense social insecurity, and the pervasive political uncertainty of the Depression" (144) and which more or less expired at the end of that decade. Professor Wagner's grounds for thus characterizing one of the principal mainsprings behind the emergence of National Socialism in Canada after 1933, the year of the conquest of power by Hitler in Germany, lie in the occupational composition of the Bund. Utilizing admittedly fragmentary lists of its members — for the most part from among those interned at the outbreak of the war — as compiled by the RCMP, which otherwise declines to open its comprehensive records on the Bund to researchers, a "composite picture" of the organization's adherents can be drawn. (69 ff.) These were overwhelmingly farmers, artisans, white-collar employees, and small businessmen, in short lower-middle class elements whose socio-economic status was especially endangered during the Depression. Conspicuously absent were industrial workers, even more so than had been the case in the nascent NSDAP. But like

the latter's early following, the Bundists in the main were relatively young men and also either new immigrants or else first-generation Canadian Germans; for example, all 20 members of the Loon River local unit whom the author has been able to examine in some detail arrived in Saskatchewan in 1929. They had therefore been exposed to the same racist-nationalist ideological influences as their contemporaries in central and eastern Europe (many of them were actually so-called "Volksdeutsche," that is persons born and raised beyond Germany's political frontiers who had acquired a heightened sense both of linguistic homogeneity and of economic deprivation among alien majorities). Although the largest single number of Canadian Nazis resided in Montreal, the great majority of them were to be found across the prairies, often in remote agricultural communities made up predominantly of Germans. Nonetheless they never counted more than 2,000 followers among the half-million of their compatriots in the total population.

This pronounced marginality goes far towards explaining why the Bund was allowed to pursue its activities almost unhindered until 1938. These consisted largely of disseminating propaganda on behalf of the "new Germany" through the medium of the ethnic press (only two of the seven German-language newspapers with substantial circulations remained outspokenly anti-Nazi) and at the annual summer "Tage," or folk-fairs, staged by German groups in several provinces. In this they encountered substantial opposition from longer-established cultural clubs whose older members knew nothing of Hitlerism first hand and moreover were irreversibly assimilationist in outlook. The small Ontario-based German-Canadian communist party, the *Deutscher Arbeiter- und Farmerverband*, regularly attacked the Bund in its paper as a Nazi-inspired and controlled organization, a fact its leaders went to great lengths to conceal in order to avoid the fate of paral-



lel American phenomena banned by the authorities there. Neither communist efforts, which culminated in the noisy disruption by over 600 of their number of a Bund meeting in Windsor in April 1935, nor the more conventional attempts of the Canadian Jewish Congress and Liberal MP Samuel W. Jacobs to expose the blatantly anti-Semitic nature of native Nazi publications could shake the indifference of the Canadian government and people towards the movement and its sponsors. It took *German overtures to purchase Anticosti Island* in the mouth of the St. Lawrence River and the strategic implications of this to arouse Conservative politicians, led by R.B. Bennett, to voice the first official concern about the Nazi threat to Canada, including that posed by the Bund. From then until their most prominent members were rounded up beginning in fall 1939 and placed in detention camps, German-Canadian Nazis along with their supreme Führer became the object of increasingly hostile public attention — one Toronto alderman proposed confining Hitler in a cage at the city zoo! Even Ottawa admitted confidentially, however, that there was no indication any Bundists had engaged in acts of espionage or planned to do so in the event of war between Germany and Canada. In all 840 internees, a great many of them naturalized citizens, by war's end or shortly thereafter had been allowed to return to their homes; the idea of deporting a handful of the "most obnoxious" Nazis back to Germany was never implemented.

Professor Wagner has accorded this byeway to Canadian (and German) history of the 1930s the degree of attention it deserves. Hitler's almost complete lack of knowledge of or even interest in Canada, which he thought the United States should and could absorb without much Canadian resistance (23-4), condemned any pro-German movement in this country to ultimate insignificance — unlike similar groups in Czechoslovakia or Poland whose activities directly facilitated the dictator's ambitions. Of course, this did

nothing to discourage the myriad of Party and other agencies in the Reich concerned with their co-nationals overseas from involving themselves in the affairs of the Bund; in carefully delineating this tangled side of the story, the author furnishes another illustration that bureaucratic Darwinism, rather than totalitarian uniformity, was the hallmark of National Socialism. His analysis is based on a thorough examination of available archival evidence in West Germany and Canada. His claim for the study — that it is not intended to cover the entire range of domestic fascism during the interwar period, only its German variant — is as appropriately modest as his conclusion: "the pro-Nazi forces in Canada were neither as strong as their leaders believed nor as dangerous as some of their opponents contended." (143) Until or unless the Mounted Police make available the documentation in their possession, this book tells us as much as we can and probably need to know about Canadian Nazism — at least before 1945.

Lawrence D. Stokes  
Dalhousie University

Christopher Armstrong, *The Politics of Federalism: Ontario's Relations with the Federal Government 1867-1942* (Toronto: University of Toronto Press 1981).

IN HIS OUTSTANDING BOOK, Christopher Armstrong, an historian at York University, analyzes the frequently fractious relationships between the governments of Ontario and Canada. By focusing on economic development and resource policy, "fiscal federalism," and constitutional questions, Armstrong demonstrates conclusively that successive Ontario governments formed the vanguard of the "provincial rights" movement, steadfastly opposed the extension of federal power, and pursued a relatively coherent strategy in intergovernmental relations. The author's careful use of histori-

cal documentation results in a readable and lively interpretation of an important, yet seldom rigorously examined, aspect of Canadian political economy. An obvious virtue of Armstrong's book is his ability to point to broader lessons while also recognizing and conveying the importance of dominant personalities.

Armstrong shows how Queen's Park, under Oliver Mowat's shrewd direction, successfully challenged Macdonald's centralized conception of the Canadian state. In fact, shortly after Confederation, Ontario's political elite recognized and began to exploit the province's increasingly important powers over natural resource development. But business interests also played a prominent role in the expansion of the Ontario state's economic and political influence. For capitalists adapted to the complexities of federalism and quickly learned how to exercise influence at both Ottawa and Queen's Park. Indeed, the heart of Armstrong's volume is a series of carefully documented accounts of how inter-governmental conflict flowed from a complex confluence of competing business interests, an unclear division of power under federalism, and the complications of Canadian-American relations. Vociferous disputes about the "manufacturing condition" and the development of hydro-electric power are explained in these terms. Some intergovernmental conflict was also rooted in the tendency of competing bureaucracies to maintain and expand their influence. Armstrong notes, moreover, that the Ontario government's expanding role in the economy led to federal-provincial conflict over taxation as Queen's Park sought maximum control over the course of provincial economic development. In constitutional matters, Ontario governments in the era examined are portrayed as stubborn and self-interested defenders of the status-quo.

A couple of criticisms of Armstrong's book are in order. First, his account of the very bitter conflicts between the federal

and Ontario governments in King-Hepburn period is not complete. Armstrong correctly points to the personal animosities between the two leaders and dismisses as unproven the view that some of Hepburn's power policies reflect his alliance with power capitalists in Quebec who feared competition if the St. Lawrence was developed. But ultimately he goes no further in analyzing the Hepburn-King feuds. At the very least, Armstrong's account should be read in the context of Reg Whitaker's assessment of federal-provincial conflict in this period. For Whitaker, in his excellent book, *The Government Party*, adds a different dimension by portraying Hepburn as a staunch ally of reactionary mining interests in northern Ontario. Mackenzie King's deep fear was that Hepburn's virulently anti-labour policies, undertaken on behalf of Hepburn's mining cronies, and the premier's pursuit of a Liberal-Conservative coalition would polarize Ontario politics to the long-term detriment of the Liberal party's fortunes. Second, Armstrong's assessment of the events reported on is a bit too antiseptic for my liking. The author admonishes readers to take seriously powerful provincial governments, argues that Ontario and federal politicians were responsible for the constitutional impasse of the depression, and notes that it is impossible to gauge accurately the degree of public support for Queen's Park's positions. But Armstrong makes few other explicit judgements about the complex politics involved.

*The Politics of Federalism* should be read widely, particularly by those interested in, or hostile to, Ontario's "mission" in Canadian federalism. Armstrong's excellent scholarship also points to many areas where further research is required. In particular, studies of the intergovernmental politics surrounding the development of Ontario's manufacturing industry and the welfare state would be very valuable. For scholars contemplating such studies, Armstrong's

book provides an admirable model and one worthy of careful and reflective study.

Allan Tupper  
University of Alberta

Walter Stewart, ed., *Canadian Newspapers: The Inside Story* (Edmonton: Hurtig Publishers 1980); and John D. Stevens and Hazel Dicken Garcia, *Communication History* (Beverly Hills: Sage Publications 1980).

IT IS ONLY BY A KIND OF administrative fiat that labour history and cultural studies are kept apart. When one thinks of E.P. Thompson one's thoughts often run on to the work of Raymond Williams, even if one is not quite sure of all the connections. This kind of linking becomes almost impossible if instead of culture as a practice or an ongoing response, one substitutes "mass culture" understood as something to which *others* are subjected. When understood in this way it is no wonder that labour historians choose to stick to their own field of interest.

One way of making the connections can be found in Walter Stewart's collection of pieces by journalists writing about their work on the major Canadian newspapers. The idea of journalism as work tends to strain the labour history paradigm of the "classical" working-class occupations. We tend to imagine work in physical terms or in terms of skill, rather than as work that is spoken. But of course, all work is spoken in various ways and to this extent is not exempt from a hermeneutic of the prose of the world which is in turn the foundation of the communication industries.

Some of these connections are now being made by a sociolinguistics of work situations and by studies in industrial folklore. In the sociology of literature and cultural studies there are investigations of the ways workers are represented in novels, plays, and TV news programmes.

But of course, it is much too simple to say that workers are "represented" as if they were being discovered for the first time. In a society such as ours there are long oral and written traditions that make work known to us. More often, in the dominant traditions, work in any real sense is unknown to the forms of expression that are the residue of the past. For example, few novels deal seriously with work. Emergent forms such as the new industrial poetry are significant because they challenge this long tradition of neglect.

We should hardly be surprised that the journalists who contribute to Stewart's book do not restrict themselves to describing hours at the typewriter or video display terminal. The journalists quite properly understand the prose of the world as their work.

Stewart kicks off with a marvellous piece called "No, Virginia, there is no Lou Grant" which deals mainly with the takeover of the newspaper business by the two corporate giants of Southam Inc. and Thomson Newspapers Ltd. The book treks across Canada from east to west and the first chapters make sorry reading. So many factors conspire to produce such *bad* newspapers in Eastern Canada. By comparison the Montreal *Gazette* sparkles, but its triumph over the now defunct *Star* was, as Glen Allen puts it, "a triumph of accountants over journalists, a triumph of mediocrity." (83) One of the pieces is Michael Enright's on *The Globe and Mail*. Heather Robertson writes superbly on "the end of the terrible men" and the beginning of corporate journalism in Winnipeg. The journey ends with the Vancouver *Sun* and then there are good critical pieces on the parliamentary press gallery, French journalism in Quebec, Canadian foreign correspondents (there are far too few of them), and the Canadian Press news agency.

Some sense of different regions does emerge from the book, but the main emphasis is repeatedly on the "rational-

ization" of the newspaper business. An important subplot is the journalists' struggles to unionize. The articles are without exception extremely critical of the poor standards of Canadian newspapers.

*Communication History* is written in quite a different style than the snappy lead paragraphs of *Canadian Newspapers*. Quite simply, it is a professor's book: "Students, unexposed to the demands of precision, remain unchallenged to exercise the necessary discipline for advancing research in the profession." (49) The book may be a useful reference for a labour historian interested in the implications of communications — both information and transport — defined broadly to include travellers' gossip, post riders, letters, handbills, newspapers, books, or waterway and postal workers, the use of roads and land vehicles. The first three chapters attempt a historiographical clearing operation and this is followed by case studies of communications on the Kentucky frontier, the effects of public opinion on the media, and the distribution of printed and electronic media. All of the examples refer to the United States. *Communication History* is supposed to be a textbook, but if one had a student or a friend who was interested in combining labour history and cultural studies it would be far better to give him or her David Meakin's *Man and Work: Literature and Culture in Industrial Society* (1976); and two books by Raymond Williams, *The Country and the City* (1973) and *Culture* (1981) — if for no other reason than for Williams' meditative style of prose which seems always to remind the reader of how much is necessarily left unsaid.

Alan O'Connor  
York University

Denis Moniere, *Ideologies in Quebec: The Historical Development* (Toronto: University of Toronto Press 1981).

WHEN DENIS MONIERE'S BOOK first appeared in 1977, it was hailed as synthetic work that brought the tools of Marxism to bear on the historical development of ideologies in Quebec and, more particularly, of nationalism. While there had been earlier works such as Gilles Bourque's and Stanley Ryerson's on the post-Conquest period and the nineteenth century, there was no study that spanned the centuries from the beginning in New France to the rise of the P.Q. Moniere's book, written with a certain verve and polish, had a critical success well beyond the universities, earning its author a Governor-General's award and establishing him in the forefront of the younger generation of Quebec intellectuals.

University of Toronto Press deserves praise, therefore, for having brought out an English-language version of the book, albeit four years later. Richard Howard's translation is a competent one, even if the rhetoric of abstraction rings better in the French original than in any English version one could devise. There are also passages one could have rendered differently, for example, Moniere's "humaniser le capitalisme" might have become "capitalism with a human face" of Czech spring fame rather than the rather limp "to bring humanity into the capitalist system" (266) of the present version. Still, by and large, the translation succeeds.

What then of the status of Moniere's book five years later? I can make passing mention of a controversy that has sprung up regarding the originality of at least some passages of the book. Nichole Gagnon, in a detailed review in *Recherches Sociographiques* in early 1980, argued that Moniere, while giving references to various secondary studies, had in fact lifted whole paragraphs and pages without acknowledging the full extent of his debt. For a while it seemed there might be a veritable scandal in the making, though Moniere was able to argue, convincingly I believe, that his had been a work of vulgarization that had never purported to be

entirely original. I don't think Gagnon's charges detract from the synthetic skills Moniere displays in this book, though they may have dimmed the lustre of its author in Quebec's intellectual firmament.

But it is the substance of Moniere's approach that must concern us here, an approach that I would characterize as left-nationalist in character. Moniere's is not the nationalism of an earlier period, and it leads to some fairly critical comments on the clerical-corporatist-capitalist elements in the past. Moniere's main barbs, however, are reserved for the anti-nationalists, the federalist compromisers from Lafontaine through Henri Bourassa and, of course, Pierre Elliott Trudeau. He sees Quebec as a colonized nation that can only realize itself through independence, and has praise for the progressive nationalism exemplified by the *Patriotes*, the *Rouges*, the trade union movement, the PQ, and independence forces to the left of it.

To this nationalist lexicon is appended a Marxian approach to the development of ideologies. Moniere talks about the petty producers' mode of production in New France, about competitive and monopoly capitalism in the nineteenth century (already monopoly capitalism?), with an essentially non-French-Canadian bourgeoisie, and French Canadian petty bourgeoisie, farmers, and workers. More messy and ultimately unsatisfactory is his treatment of the Church. It is supposed to be the great beneficiary of the suppression of the 1837 revolts. (120) It is the spearhead of opposition to certain features of capitalism by the beginning of this century, heading off any socialist movement. (203) Yet the Church is not convincingly explained in strictly class terms. If it is petty bourgeois, then curiously its institutional ideological moorings go back well before the capitalist era in Europe to feudalism and the Dark Ages. Clerical domination over civil society and the political system evokes an anti-secular

force whose strength both precedes the modern state and, in some ways, bars its development. A Marxist approach to this question might gain from the recent spate of historical works on the origins of the modern state and from a less class-reductionist model of ideology.

There are other problems with Moniere's version of Quebec history. He takes note of the *Rouges'* support for annexation to the U.S. circa 1850, but fails to explain what this does to the predominantly nationalist interpretation he would offer of this movement. He praises the Abbé Groulx for having "raised the idea of the French Canadian national state to a level of grandeur in the collective consciousness of the province of Quebec," (197) only to take his distance from Groulx's more fascist utterings of the 1930s. (221-3) Can one so simply separate the "good" Groulx from the "bad?" He claims that, strictly speaking, there is no French-Canadian bourgeoisie today, (232) though there is evidence that since the Quiet Revolution this class has begun to come into its own (Desmarais, Bombardier, the Caisse Populaires Desjardins). He would deny any authenticity to the Trudeau version of federalism, (242) but is forced to acknowledge, in the aftermath of the referendum defeat of sovereignty-association, Trudeau's success in dividing Quebec society. (305-6) Perhaps unfortunately, not every one in French Canada shares Moniere's idea of authenticity.

Moniere concludes with the following exhortation: "We need a lucid nationalist philosophy if Quebec is to be more than a country of the imagination. We need a nationalism that begins by exposing the patterns of domination." (312) For about 20 years this has been the *leitmotif* of the Quebec left and I think it has served the purposes of getting left analysis and organization off the ground. But is this enough in 1982? Is nationalism the *sine qua non* of any Quebec socialism as Moniere, despite his passing criticisms of

the PQ, implies? Is a Marxist interpretation of Quebec history, now that Marxism has come of age in Quebec, the same thing as the nationalist/anti-nationalist odyssey that Moniere offers in his study?

I certainly would not argue for the opposite position, that is, to throw nationalism overboard, embracing a federalist ideology which is ultimately even more compatible with the development of monopoly capitalism in this country than the weaker, more petty bourgeois nationalist line of the PQ. But I do think the Quebec left, like the Canadian left, must stop treating nationalism as the necessary short-cut to socialism, must put more emphasis on the specifically socialist elements in its position — collective ownership of the means of production, workers' control, the flowering of society as against the Moloch-state. This may seem like gratuitous advice coming from an English Canadian living in British Columbia. If there is to be a socialist post-script to *Ideologies in Quebec*, however, Moniere and others like him will have to move beyond the positions of five and fifteen years ago.

All of this is not to detract from the undoubted merits of this book. It does bring between two covers a great deal of historical information and uses Marxism creatively in its analysis of political ideology. The book deserves a large readership in English Canada, and should help to familiarize the left in particular with what is still the dominant current of analysis within the left in Quebec.

Philip Resnick  
University of British Columbia

Emile Bouvier, S.J., *Les Relations du Travail au Québec* (Montréal: Guérin Éditeur Limitée 1980).

**THE OBJECTIVE OF THE AUTHOR** is to introduce beginners to the study of labour issues, unions, and labour relations with emphasis on the Quebec context. The

book is divided into four major sections: 1) the history of unionism; 2) components of the union structure; 3) the code of union policies; and 4) labour conflicts.

The first two chapters of the book introduce the study of labour relations. The author develops a theoretical framework, that is, a "system of labour relations." Terms are defined and some statistics on the evolution of the labour movement are presented. The author's intention is sound and the topics are appropriate. However, much is lost with an approach that tends to be more prescriptive than descriptive. The first part of the book on the history of the union movement is probably the weakest. The author attempts to describe the history of the labour movement and the history of unions as the same. What results is a presentation that lacks both clarity and a sequential approach essential for beginners in the study of labour relations.

Reference to the North American context in which the union movement has evolved is also weak. For example, in the history of the CNTU the bias of the Catholic Church against industrial American unions is hardly presented. The reasons for the Church's establishment of Catholic unions are not well explained. There is hardly any mention of the protective role played by the Church. International unions were Protestant, English, and most of them favoured industrialization. Hence, French-Canadian workers had to be protected. Furthermore, the author presents the advent of the QFL almost as a Quebec phenomenon. Not enough emphasis is placed on the fusion of the AFL and CIO in the United States. The historical description of the four provincial centrals (CNTU, QFL, CEQ and CSD) lacks uniformity. The different approaches seem to be based more on a bias than on fact. Finally, the negative image reserved for the CNTU is unnecessary in an introductory text.

The second part of the book provides some useful insights on the internal struc-

ture and decision-making of unions. The unionization of "cadres" is well introduced. Also, the author presents a good analysis of the relationship between the monopolistic tendencies of employers and unions. The author arrives at conclusions, however, which seem to lack perspective and reality. For example, referring to unions the author claims: "Ils peuvent développer une puissance de monopole" and "Ils peuvent déclencher facilement une grève générale." In short, the author credits the unions with too much power. The presentation confuses the power that unions could have with what they "really" have. This confusion is common in Quebec — the impact of unions in the public sector has been considerable during the last two decades. This is not, however, the case in the private sector. Finally, it would be preferable to separate the role of unions at the work place from their socio-economic and political influence in society.

The third part of the book describes several relevant issues such as union policies, union security, seniority, layoffs, managerial rights, co-management and from the Quebec experience, sectorial bargaining, and sub-contracting. In general this section is well done. However, the presentation lacks a balanced approach. To what extent have union practices (job-security, seniority, union security, etc.) been provoked by employers or by the socio-economic insecurity of workers in a capitalistic industrial society? Very little is offered by the author in terms of explanations or hypotheses. Furthermore, the author gives the impression that unions decide how the production process is organized when in practice they can only react. The division of labour and specialization in the production process are the result of industrialization, the bureaucratic model of organizations, and the scientific method of management. It is illusory to suggest, as the author does, that unions have proposed or caused the essence of the industrial approach.

The fourth section of the book presents a good summary of mechanisms of dispute settlement, the administration of collective agreements, the evolution of the Quebec Labour Code, and the legislation concerning minimum wages, minimum family income, and minimum working conditions. Although the summary is quite concise, the major issues are well identified. However, some of the factual information is not accurate. For example, teachers in Quebec did not acquire the right to strike in 1964. Furthermore, the evolution of the Labour Code presented does not deal adequately with the amendments (Bill 55 and Bill 59 in 1978) adopted for the public sector. Finally, the author makes three timely suggestions to improve labour relations in Quebec: a greater accessibility to unionization, sectorial bargaining, and a complete revision of all labour legislation.

In summary, the author has achieved the objective of identifying for beginning students in labour relations many appropriate issues. However, weaknesses in the content, approach, and organization of the material need serious attention. Concerning the content, the author excludes the social and political context from his analyses. Hardly any attention is given to the impact of the Duplessis Régime, the Catholic Church, and the Quiet Revolution on the evolution of labour relations in Quebec. The absence of the socio-political context results in a classical economic (Dunlopian) approach. Furthermore, the book is weak on the evolution of labour relations in the public sector. The construction sector is also neglected.

Regarding the approach and the organization the following improvements are suggested: the author should clearly identify his opinions and separate them from the content, a bibliography should be prepared for every chapter, more credit should be given in the text to other authors, sources should be quoted for all facts and statistical data, important recent works (e.g. Jean Boivin, Noël Mallette,

Fernand Harvey, Gérard Hébert) should be considered, and the accuracy of some statements should be verified.

Antimo Papale  
McGill University

Thomas R. Berger, *Fragile Freedoms: Human Rights and Dissent in Canada* (Toronto: Clarke, Irwin 1981).

CANADA, AS THOMAS R. BERGER conceives it, is that country which, rejecting both the ethnically defined nation-states which gave it birth and the melting-pot uniformity of its sibling to the south, attempts to unify itself through the vocation of diversity. "Diversity, as he says, is the essence of the Canadian experience." (xiii) The history of "Human Rights and Dissent in Canada," as *Fragile Freedoms* is subtitled, is therefore the history of Canada. Each chapter is an episode in which the majority is challenged by a minority which attempts to preserve its identity in the face of a strident movement of assimilation.

Berger presents, in turn, the expulsion of the Acadians, Riel's New Nation, the fight for separate schools, the banishment of Japanese Canadians, the suppression of Communists and Jehovah Witnesses, the October Crisis, and Aboriginal Rights. Each historical chapter, complete unto itself, describes an injustice to a minority group, an injustice which Canadians have more or less successfully overcome, an injustice against which the new Constitution and Charter of Rights and Freedoms provides a safeguard. (255) The book indeed is unified and dominated by its continual reference to the new Charter, for which the work can be seen as a sustained defence.

While most of Berger's material is not original, his selection of themes and the historical interpretation he places on them make *Fragile Freedoms* an important and timely work. The easy, popular style, illustrated by sensitive stories of human

events, should make this approach to the history of Canada accessible to many who would be unlikely to read the original sources. As such, it is to be highly recommended.

There are nonetheless a number of serious limitations to the work. The most obvious is that the draft of the Charter of Rights which Berger reproduces in the book, and on which he has obviously based his work, is outdated. After the work went to press, negotiations between the federal government and the provinces led to the addition of Section 33 which permits a legislature to override almost any part of the Charter for a five-year period simply by stating explicitly that it is doing so. It is one of Berger's basic theses that parliaments represent majorities and that therefore minorities can only be protected by placing limitations on the powers of parliaments, a thesis well substantiated by the case studies presented by this book. (261) (See also his statement reported in the *Globe and Mail* 8 April 1982, 12.) His interpretation of the new Charter as the instrument which will place human rights and fundamental freedoms "beyond the reach of Parliament and the provinces," (262) once and for all protecting minorities from the tyranny of legislative majorities, is presupposed by the whole book. "Of course, the entrenchment of minority rights will limit the powers of Parliament and the provinces. This is the whole point. These rights should never be subjected to the will of the majority. They are *minority rights*." (262) This presupposition is seriously undermined by the insertion of the override clause. In at least five of the eight episodes he describes, Berger's own description of the situation and attitudes would lead one to expect that the override clause would have been invoked if the Charter had been in effect at those times. Perhaps as a result of this development, Judge Berger seems to have adopted a more sceptical position recently, if the report of the *Globe and Mail* of 14 August



1982 is accurate: he now says that the new Charter is an improvement, but cannot be expected to end all controversy.

Apart from this difficulty of being overtaken by events, Berger's book is locked into history in another way. He describes the Charter as "a document expressing that decent respect which the present owes the past; it is, at the same time, a document addressed to future generations." (xiv) Yet the first of these themes is emphasized to the detriment of the second. He understands the Charter as the denouement of Canadian history, not primarily as a blueprint for the future. As a result, there is a certain lack of realism in some of the analyses.

For example, the battle over the rights of religion and language in education is presented as culminating in the new Constitution. (92) He states that the right to education in either English or French is "vital" for the preservation of the separate identity of the two founding nations; it is one of the main pillars of the family-church-school structure which, according to Berger, supports society. While this analysis may have been realistic in the last century, the ignoring of other factors is a serious impediment to understanding contemporary society. One could well maintain that at the end of the twentieth century the mass media, technology, and the working environment are at least as powerful as the school in effecting social change or stability. If a Charter of Rights is to protect a minority culture successfully, must it not grant to the minority a right to control cultural, artistic, and lifestyle influences, even perhaps to the point of protective censorship? English language advertising, television productions, and rock-music fashions now threaten the survival of French culture as much as restrictions on French language schools outside Quebec did in the last century. The danger is even more evident in the case of the Inuit who will soon be receiving hundreds of channels of international television from satellites. Measures to pro-

tect a way of life from such influences, of course, violate the rights of individuals even within the minority culture. That is precisely the real dilemma of the present and the future: the Charter, and Berger's defence of it, in this as in many other examples, are dealing with a nineteenth-century problem and supply a nineteenth-century solution. Problems based on the technological or economic infrastructure are ignored.

Why does Berger not see this? In his chapter on the Metis he is quite clear that what is at issue is the right to a way of life, a way of life that is no longer possible for technological reasons. The Metis cannot, as he says, become buffalo hunters again. (57) Berger asks, "Is there a way in which, in a modern, industrial country, a minority such as the Metis can be themselves?" His response is that the new Constitution will force us to renegotiate on aboriginal rights. (56) But how can the Constitution help? Berger himself points out in the chapter that the granting of land, such as in a lands claim settlement, does not address the real issue: the Metis way of life has vanished, dissolved by a change in the technological infrastructure. Rights to language, land, schools, and so on cannot solve the problem.

I would suggest that some of Berger's (and the Charter's) problems are due to the assumption of a "liberal" interpretation of rights. The liberal position assumes that once equality, or any other right, is written into the legal system it is assured. Consider the example of the Ontario nurse who was recently cleared of charges of murdering babies. Her right to a fair trial required her to accumulate legal fees of some \$200,000, which few could afford. Yet the Charter does not guarantee the availability of the resources necessary in practice to assure equality within the legal system. The Charter nowhere mentions economic realities. Economic issues are excluded, as it were, from the realm of rights. So there is no right to a minimum

level of wealth, no right to employment, no right to strike, no right to a minimum level of shelter, or even of food. Yet there is many an impoverished pensioner who would willingly sacrifice her right to use English or French in Parliament for the right to a square meal and decent shelter! Section 15 of the Charter prohibits discrimination on the basis of race, sex, etc., but significantly neglects to mention poverty, employment status, or class. These exclusions from the realm of rights are not arbitrary; they reflect an overall political philosophy shared by both the drafters of the Charter and by Berger.

Ironically, one of the first attempts to violate the new Charter was the move to remove Judge Berger from office for his public comments on political affairs, specifically his defence of the position adopted in *Fragile Freedoms*. The case itself is interesting in that no one used legal means to try to prevent Berger from expressing his opinion; rather it was proposed that he could no longer keep his employment as a judge if he wished to make political comments. The attempt failed; but the exact grounds for the failure are legally intricate and leave no grounds for a claim to victory for freedom of expression. As is evident also from the recent case of a public servant who was fired for opposing metricization, the Charter prohibits only the passing of laws restricting freedom of speech; it does not prevent restrictions or pressures based on economic threats, such as a threat to one's employment. Berger's own problems, I

think, help to illustrate some of the limitations of his liberalism.

Berger's selection of historical events, and his view of the Charter as a natural outcome of these events, are tightly determined by his liberal philosophy. Individuals are to be categorized as male or female, black or white, Anglophone or Francophone, but never as rich or poor, working or ruling class, employed, unemployed or striking. The conceptual scheme permitted by this philosophy prevents the inclusion of questions of economics or of technological infrastructure under the heading of rights.

Once these limitations of basic political philosophy, backward-looking historical perspective, and reliance on an outdated version of the Charter are recognized, *Fragile Freedoms* can be read with great profit by almost anyone. The book illustrates forcefully the fragility even of liberal rights in Canada and it places the new Charter in an historical context which, for the most part, was lacking during the public debate about it. It is written in an imaginative style, full of human sympathy; a very moving plea for the rights of minorities.

David L. Thompson  
Memorial University of Newfoundland

Paul Weiler, *Reconcilable Differences* (Toronto: Carswell, Metheun 1980); H.W. Arthurs, D.D. Carter, and H.J. Glasbeek, *Labour Law and Industrial Relations in Canada* (Deventer, Toronto: Kluwer, Butterworths 1981); and George

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W. Adams, *Grievance Arbitration of Discharge Cases*, (Kingston: Industrial Relations Centre, Queen's University 1978).

COLLECTIVE BARGAINING IN Canada has been regulated by comprehensive legislative schemes since the 1940s. In addition to the substance of regulation, the law created new processes for its own administration by labour relations boards and labour arbitrators. Those aspects of the collective bargaining process not governed by the legislation such as, in most of the country, picketing, are subject to the control of the courts. Bluntly put, it is a system of pervasive legal intervention and regulation. As a result, knowledge of the law and labour lawyers have become parts of our system of industrial relations. (Readers will, no doubt, draw their own conclusions about the wisdom of such a turn of events.) It is therefore remarkable that prior to 1977 only two books and one set of teaching materials designed for law students could lay any serious claim to the attention of those with a need to know the Canadian law of labour relations. The books were Carrothers, *Collective Bargaining Law in Canada* (Toronto: Butterworths 1965), and Sack and Levinson, *Ontario Labour Relations Board Practice* (Toronto: Butterworths 1973 and 1977 supplement). Carrothers' book, while comprehensive in legal scope and national in geographic sweep, is simply hopelessly out of date. Sack and Levinson, on the other hand, while reasonably up to date, is limited geographically to Ontario, and otherwise by its (intentionally) nuts and bolts approach. The other pre-1977 volume was *Labour Relations Law* (Kingston, Ontario: Industrial Relations Centre, Queen's University 1971, 2nd edition 1974, 3rd edition 1981), a set of teaching materials used in law schools across the country. Its obvious limitation is its equally obvious strength — that it is a teaching and learning device, and thus not an easily accessible compendium of our labour law.

Since 1977, something of a revolution has occurred in labour law publishing in this country. Three volumes have appeared covering the evolving law of labour arbitration — Brown and Beatty, *Canadian Labour Arbitration* (Toronto: Canada Law Book 1977); Palmer, *Collective Agreement Arbitration in Canada* (Toronto: Butterworths 1978 and 1981 supplement) and Gorsky, *Evidence and Procedure in Canadian Labour Arbitration* (Toronto: Deboo 1981). The field of non-collective bargaining labour law is now comprehensively dealt with in Christie, *Employment Law in Canada* (Toronto: Butterworths 1981) and in a more specialized way in Harris, *Wrongful Dismissal* (Toronto: Deboo 1st ed. 1978, 2nd ed. 1980). In addition, the three volumes under review have appeared. These are three works of radically different scope and purpose written by leading labour academics, labour board chairmen, and labour arbitrators. All three are intelligent, innovative, and excellent additions to our literature of labour relations law.

Phil Weiler is certainly one of the most dominant influences upon labour law in this country. He has been a labour law professor at Osgoode Hall Law School, nationally known labour arbitrator, labour law scholar, architect of the new labour code of British Columbia, and first chairman of the new British Columbia Labour Relations Board. His arbitration awards, scholarly writings, and labour board decisions have established the framework for our discussion of many issues and much of his writing has achieved an almost gospel-like status. While under his chairmanship the British Columbia Labour Relations Board became the leading board in the country. Now a professor at Harvard Law School, Weiler has produced a marvelous volume which is in part a story of his experience as chairman of the Labour Relations Board of British Columbia and in part a survey of many recurring and troubling

issues of basic labour law policy. These two themes are intertwined because Weiler has chosen to speak to many of the major legal issues through the medium of a concrete dispute which he had confronted while labour board chairman in British Columbia. This, while providing instances of *déjà vu* for those who have read the original labour board decision in question is, I think, an enormously successful technique. Issues such as bargaining unit determination, first contract arbitration, refusal to cross picket lines, the "right to work," reform of construction industry bargaining structures, public sector strikes, and wage and price controls are canvassed in light of real pragmatic disputes with which Professor Weiler is intimately familiar. This fundamental technique of concentrating upon a few cases in detail reflects the flavour of the book. It is written in the deceptively simple, almost conversational style which has become the trade mark of Professor Weiler's decisions. This is not a volume committed to the grand footnote or other academic paraphernalia. A very few notes for each chapter are contained at the back of the volume. The references to the literature that are noted obviously have been chosen with great care and when taken as a package constitute an excellent bibliography of the classics in North American labour law writings.

Professor Weiler's book makes no attempt to be a comprehensive survey of labour law in this country. It is not a text book and it is not a book for the beginning student to take up as a preview of the topic. Omitting the introductory, concluding, and first chapter, the book deals with industrial conflict in Canada, the labour arbitrator and the labour agreement, the individual employee and the trade union, the structure of collective bargaining, *labour relations in building construction*, strikes by essential public employees, and collective bargaining under wage controls. I would single out the chapter on the individual and the trade union as a classic.

Running through the text are several deep currents. Those familiar with Professor Weiler's career and writings will find them familiar. First, a commitment to the values of *collective bargaining*. Second, a profound belief in the virtues of a system of *free collective bargaining* by employers and employee representatives. Third, open acknowledgement of the public interest in the collective bargaining process. Finally, an overtly enthusiastic embracing of a comprehensive administrative (as opposed to judicial) model as the means for advancing and reconciling these objectives. For this reviewer, the book's central tenet is a commitment to the view that one cannot make or administer law without a clear view of one's purposes combined with substantial knowledge about what goes on in the real world. If only all law makers and administrators could achieve such understanding and articulate it so well.

Much of what Professor Weiler's book is and is not is put into starker relief by comparing it with *Labour Law and Industrial Relations in Canada* by Arthurs, Carter, and Glasbeek. All three of the authors are labour law teachers in Ontario, two of them prominent arbitrators and one of them a former chairman of the Ontario Labour Relations Board. Professor Arthurs has been a dominant force in Canadian labour law since the early 1960s. They have written a very different sort of book. This is a book which provides a comprehensive overview of our system of labour relations. It is *not* a book which dwells upon the deep troubling issues which Professor Weiler explores. It is a book with numbered paragraphs and it is a book which I would recommend to students looking for a brief, concise preview of the topic. It is, however, far too easy for lawyers to label this volume *incorrectly*. Lawyers are too familiar with legal texts which, in numbered paragraphs, list every decided case in a mindless litany of self contradiction and internal inconsistency. Those volumes are gen-

erally very large and breathtakingly anti-intellectual in their refusal to come to grips with difficult and confused areas of the law. Arthurs, Carter, and Glasbeek, as one would expect, have written a much better book than that. This is a concise, complete, no nonsense volume. It uses a minimum number of case citations and other references, which unfailingly take the reader to the leading cases and commentary. While its format and style do not permit extensive digressions upon troubling issues, the book is clear at the level of principle and tenacious in its exploration of the tensions in the working out of our substantive law and the proper institutional arrangements for its enforcement. I think this book is a remarkable achievement. There are, however, other aspects of this book which deserve comment. The book is a reprint of the entry on Canada which appears in the *International Encyclopedia for Labour Law and Industrial Relations* (Deventer: Kluwer 1981). As such it is meant to provide for non-Canadian readers an overview of our system of labour law and industrial relations. To that end the volume leads off with 50 pages of background information about Canada and its government, definitions of basic North American labour law and labour relations concepts (conciliation, bargaining units, arbitration, unfair labour practices), the Canadian Labour Movement, institutions, and sources of law. It is obvious that the book makes no assumptions and is, for the beginning student of labour law, a manageable starting point. This, however, is not the book's major innovation. This is a comprehensive book about the whole of our labour relations law, not merely as it applies to organized workers. That is, the survey undertaken here is not limited to our law of collective bargaining. The book is divided into three sections, the first covering the introductory material already mentioned. The second is entitled "The Individual Employment Relationship" and it deals with the fundamental issues such as

the rights of employers and employees under the common law contract of employment, the definition of employee, the action for wrongful dismissal, and the common law of occupational health and safety. Given the limited coverage of the collective bargaining regime in Canada (see 119-23) the inclusion of this material on the law for the unorganized is nothing but logically necessary in a volume purporting to cover labour law and labour relations in Canada. The treatment of this material is again to the point. It avoids no difficult conceptual issues (the role of the contract in employment) and pulls no punches (how employees fared under the common law regime). The third section of the book deals with the collective bargaining relationship.

*Grievance Arbitration of Discharge Cases* by George Adams is again a book by a prominent Canadian labour lawyer. Mr. Adams is a former labour law professor, former Ontario Assistant Deputy Minister of Labour, and currently chairman of the Ontario Labour Relations Board, as well as a prominent arbitrator. Unlike the volumes already noted his brief volume (108 pages) concentrates upon one single labour law issue — the arbitration of employee grievances of dismissal without "just cause." This book does two things with this single topic and does them very well. The first 37 pages contain a concise and invaluable elucidation of the concepts of discipline and disciplinary action, the defective common law approach to those issues, and the evolution of our modern arbitral jurisprudence created by Canadian labour arbitrators administering the familiar collective agreement standard of "just cause" for dismissal. The common law and the arbitration law are compared along three dimensions — substantive, procedural, and remedial. The overview of the remedial arbitration law leads into a review of competing theories of the purposes of punishment with a balanced discussion of employee and employer interests. This is

by far the best discussion of these large and difficult issues in arbitration law that I have yet located. The book is worthwhile for this alone, but Adams does not stop there. The second part of the book is an empirical study of 654 disciplinary discharge cases in Ontario between 1970 and 1974. There is precious little empirical work undertaken by lawyers and this is a valuable addition to our understanding of how labour law really works. Adams' objective, of course, is to seek empirical answers to questions generated by a discussion of the arbitral law of discharge. Do the theories being applied by arbitrators work or not? Is reinstatement of employees working? Anyone involved in the arbitration process will be intrigued by the many questions to which Adams' data offer some surprising answers. Who reinstates more often — tripartite boards or sole arbitrators? What is the effect of delay in the holding of a hearing upon the award of back pay? On reinstatement? Which industrial offences lead to reinstatement and which do not? How many of those reinstated actually return to work? How many of these remain at work? Does the age or seniority of the employee have an effect? Those involved in the grievance arbitration process will have views on many of these questions. Discovering whether one's intuitions are congruent with the data makes, in my view, for a compelling, if brief, read.

My enthusiasm for all three of these volumes is evident. These books by leading Canadian labour lawyers are to be welcomed.

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Charles E. Reasons, Lois L. Ross and Craig Paterson, *Assault on the Worker: Occupational Health and Safety in Canada* (Toronto: Butterworths 1981).

THE LITERATURE ON occupational health and safety has been steadily expanding

over the last ten years and can roughly be divided into two categories. First, there is a body of work which has sought to demonstrate that occupationally related injuries, disease, and deaths have reached unacceptably high levels and that some action must be taken to stop this workplace carnage. One strategy is to change the conventional wisdom about the range of injuries and diseases which can be attributed to occupational exposures, by shifting away from a narrow focus on traumatic injuries toward a concern with overall health effects. Another strategy is to characterize the problem as one of controlling criminal behaviour by employers, as is exhibited by instances in which employers have either ignored or suppressed evidence that continued exposure to some substance in the workplace was likely to have an adverse impact on workers' health. The primary goal of much of this literature is to inform those at risk of the dangers they face and to mobilize public support for state intervention.

A second category of literature tends to be of a more academic variety and grows out of the debates surrounding the desirability and effectiveness of regulatory responses to the problem of occupational health and safety. Much of that literature is inspired by neo-conservative critiques of government intervention in the market place and calls for a retreat from direct regulation in favour of a vision of the state's role as one of facilitating market transactions or in the face of market failure, identifying the result the market would have arrived at by use of a variety of instruments such as injury taxes, inducing private parties to move towards the equilibrium market position. Liberal criticisms of the more extreme versions of neo-conservative attacks on occupational health and safety regulation emphasize that markets have failed, pointing to factors such as the severe information problems which constrain bargaining especially when dealing with occupational exposure to carcinogens and other toxic

substances whose long-term effects are unknown. Liberals also acknowledge that, at some point, questions of fairness must also be addressed. Nevertheless, market mechanisms are not wholly rejected and an economic perspective on the crucial question of how much protection should be mandated in the face of market failure is maintained. Problems with the distribution of wealth and power are not wholly ignored, but the major focus tends to be on efficiency.

*Assault on the Worker* has something of both approaches but its dominant focus is that of the literature of outrage: to inform and mobilize. As its title implies, the book starts from the premise that the failure of employers to provide safe work places constitutes criminal behaviour and should be treated as such by our legal institutions. Five chapters are devoted to describing the dimensions of the problem with special attention given to some aspects of occupational health and safety which have received scant attention until recently: toxic substance exposures, stress, women's health, and farming. These chapters, combined with the Appendices provide valuable information and resources in readily accessible form and in my view are the most useful part of the book.

Part III of the book turns its attention to some of the current approaches to the problem of occupational health and safety, focusing on the "blame the worker" syndrome, workers' compensation, and direct regulation. It is this part of the book that I find disappointing, not because the criticisms of the current approaches are undeserved, but rather because it lacks a unifying focus and fails to provide a more comprehensive perspective from which to analyze the problem. Take the case of workers' compensation, a major target of criticism in this part of the book. Workmen's Compensation Boards are characterized as bureaucracies that tend to view their role primarily as one of providing low-cost insurance for

employers. By controlling costs, they provide an environment conducive to rational investment planning. It is bemoaned that compensation is not employed as an integral part of a broader attempt to improve health and safety conditions. With respect to workers, the Boards are criticized for limiting their objectives to providing minimal levels of compensation to those who are inevitably injured in the production process. The coverage is not comprehensive, the levels of compensation are inadequate and the bureaucracy is often unsympathetic. All this has been said before, but does bear repeating.

The weakness of this analysis is the failure to go beyond these specific criticisms. With respect to the problem of compensation, is it rational to restrict our perspective to that of compensation for occupationally related injuries and accidents? By doing so we force decision-makers to draw difficult distinctions about the causes of disability because they must decide whether or not the claimant will get WCB benefits or be forced to resort to welfare or private resources if such are available. Should the right to a decent level of support depend on whether or not you can link your heart disease or lung cancer to occupational as opposed to environmental or life style factors? Admittedly, so long as workers' compensation does provide higher levels of benefits than alternative programs workers will fight to have the range of compensable injuries and diseases expanded. Nevertheless, a broader perspective on the compensation question might have contributed to a better analysis of the political and economic background of the problem as it appears in Canada, and to an alternative strategic response.

Furthermore, if we are going to explore workers' compensation as a mechanism for improving the conditions of employment, indignation at the failure of Boards to impose significant penalty assessments is only a first step in, but not a substitute for, analysis. Yes, the theory does state that the rational profit maximiz-

ing firm will seek to find the point at which the sum total of accident costs and accident prevention costs are minimized. If accident costs including penalty assessments rise to a level at which it becomes cheaper to invest in safety, then we would expect the firm to do so. But, what happens where the sum total of accident costs and accident prevention costs that the firm has to bear is increased absolutely? Will profits decline, or will the costs be passed on or will there be pressure to reduce wages or will the factory be shut down altogether? This is not intended to be an argument against mandating higher safety standards. Rather, I am simply suggesting that the limitations of regulatory solutions be recognized given the freedom of capital to move across provincial and national borders, as well as the non-competitive nature of much of industry.

The book then fails to go significantly beyond the literature of indignation. It does not, in fact, confront the neo-conservative and liberal styles of economic and regulatory analysis which have dominated the academic literature. Dissemination of information and mobilization are important objectives but, as well, it is necessary to place the problem of occupational health and safety within the broader context of capitalist accumulation. If capitalist production is taken as a given, then the trade-off between jobs and safety may indeed be real and the choices cruel. If not, then the strategy for improving worker health and safety may take a different course. This leads to the last point which may be of particular interest to labour historians. How in fact do workers respond to the allocation of risk in a capitalist setting? The authors have made some attempt to place the problem in an historical context. There is a brief discussion of the historical origins of Workmen's Compensation legislation (160-4) and a chapter exploring worker struggles for improved work conditions including shorter hours, improved facilities in work camps, and mine safety. There is no

attempt to do more than survey the field relying primarily on existing literature, which is scant to say the least. Much work remains to be done. To what extent was the market accepted as the ground on which struggles over working conditions would be fought? Did workers trade health and safety for higher wages? What was the role of organized labour in pressing for state regulation such as the Factory Acts in the late nineteenth century? While much has been written about the emergence of an industrial working class both with respect to its organizational and political manifestations and its culture, little attention has been paid to the specific response to workplace hazards.

In sum, the book attempts to synthesize a growing body of literature from a worker perspective. In so doing it provides valuable resource material and challenges many of the dominant assumptions about the causes, extent and appropriate responses to occupational ill-health and unsafety. Yet, the failure to locate the problem within the larger context of capitalist production and the current economic crisis weakens the critique of current approaches and limits the exploration of alternatives.

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Noah M. Meltz, Frank Reid, and Gerald S. Swartz, *Sharing the Work: An Analysis of the Issues in Worksharing and Jobsharing* (Toronto: University of Toronto Press 1981).

THE PURPOSE OF WORKSHARING is to provide an alternative to a temporary layoff. It is a measure designed to keep workers in existing jobs when demand for their services declines. Instead of reducing the number of workers employed during a period of lowered demand, the available work is shared by a reduction in



working days per week on the part of all employees. The shortfall in wages paid to each employee due to the shorter work week is usually partially compensated through payments from public funds such as unemployment insurance benefits.

Jobsharing, on the other hand, is a flexible, permanent part-time work arrangement that enables two persons to divide the responsibility for what might previously have been one full-time position. This shared job arrangement is designed to accommodate employees who desire to work less than the standard work week.

Authors Meltz, Reid and Swartz have performed yeoman service in explaining the difference between the two concepts in a relatively simple, straightforward manner. The stated objectives of their study are "to create a theoretical model that could be used to assess the feasibility of worksharing and jobsharing in the Canadian labour market, and to outline possible changes in government policy to facilitate such practices." They have succeeded in reaching both of their goals, which is especially commendable, given the timeliness of the project. The resulting readable product should prove to be a valuable addition to the libraries of any employer or workers interested in the subject of employment sharing.

The Canada Employment and Immigration Commission introduced worksharing on an experimental basis between 1977 and 1979 as an option for employers and workers faced with a temporary layoff. The mechanism has since been modified on the basis of evaluations of the experiments and has been implemented as a regular program. Under the agreements, both the management and employees of the work establishment must agree to worksharing and jointly request it. The firm pays its employees for the hours they actually work and Unemployment Insurance pays benefits for the days they are not employed. If a company that expects a 20 per cent reduction in production plans

to lay off one fifth of its work force, for example, it could maintain its full complement of workers by shifting to either a four-day week or by reducing its weekly schedule from 40 hours to 32 hours on a daily basis. The supplement from Unemployment Insurance ensures the individual worker that the weekly pay remains about 92 per cent of its usual level.

The Canadian worksharing scheme was modeled on the program which has been in place in the Federal Republic of Germany since the early 1950s. Under the German plan, short-term benefits are payable in the event of temporary emergencies if it can be anticipated that payments will secure workers' jobs and also guard the employer against the loss of his trained workers. While these are considered to be benefits emanating from the Canadian program, emphasis has been placed on the long-term economic viability of firms that are entitled to receive benefits, in order to prevent worksharing from becoming a subsidy for inefficient or non-competitive industries. Although it is too soon to assess the benefits and costs of the new program several reports have appeared in the media suggesting that it is providing assistance in preventing layoffs in a number of industries.

Experiments in jobsharing are relatively scarce in the business sector in Canada but they are becoming easier to find in the public sector. As Meltz, Reid and Swartz point out, the main barrier to jobsharing appears to be fear on the part of employers that costs will be increased. They put forward conditions under which employers might find jobsharing an attractive proposition: when there is a shortage of full-time workers available; when a wage differential between full-time and part-time workers would cover increased costs; and when contributions for fringe benefits are pro-rated and jobsharing employees agree to pay the extra costs. They state that since women appear to be among those most receptive to jobsharing, multiple-earner families might be

interested in some form of jobsharing. The novel idea is also advanced that older skilled workers who are approaching retirement age might agree to jobshare with younger unskilled workers and provide interesting part-time positions for new entrants to the labour force, perhaps reducing turnover and absenteeism among this group.

The economic models developed in the study show that worksharing and job-sharing can pay off for both employers and employees. Worksharing would appear to be more equitable and more efficient than laying off employees in an economic downturn. The authors favour Unemployment Insurance-assisted work-sharing because it is more equitable to share work than to allow a small portion of the labour force to bear the burden of unemployment caused by deficient demand while the majority reap the benefits of lower rates of inflation. Savings to the firms involved in lower hiring and training costs are expected to more than offset extra costs for increased fringe benefits caused by jobsharing. Administrative costs would probably be increased through jobsharing but it has the potential of increasing productivity, decreasing turnover, and providing greater flexibility in staffing arrangements.

A problem has been identified with the ceiling levels specified as the firm's contributions to Unemployment Insurance, Canada Pension Plan, and Workman's Compensation. Basing these contributions on a percentage of an employee's annual earnings creates an unintended bias against jobsharing in favour of full-time employment. The authors recommend that ceilings be specified as a percentage of hourly earnings to remove this bias. A caveat is also presented that worksharing should not be regarded as a panacea for Canada's present economic problems. While it can reduce unemployment, it applies only to workers on temporary layoffs and can do little for those entering or re-entering the labour force. One could

add a further caveat that jobsharing should be undertaken only on a voluntary basis so that workers are not forced into involuntary part-time positions which would amount to "worksharing" of a far less desirable type than that discussed.

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Peter Morris, *Embattled Shadows: A History of Canadian Cinema, 1895-1939* (Montreal: McGill-Queen's University Press 1978); and Forsyth Hardy, *John Grierson: A Documentary Biography* (London: Faber and Faber 1979).

FROM THEIR EARLIEST days, movies have been a major social force in Canada. Clergymen denounced them, politicians censored them, but by the 1920s a million Canadians nonetheless watched them each week. Arguably the King-Byng crisis had a weaker impact on many Canadian towns and cities than Buster Keaton or Mary Pickford, but Canadian historians have generally turned a blind eye to this major element in urban popular culture in the twentieth century. Historians of the working-class in particular need to correct this myopia, both because in their early years movies were a purely working-class pastime, and because the later documentary film-makers often took workers as their subject matter. A Canadian film archivist and a Scottish film critic have each produced a book which begins to open up this long-neglected field of study in Canadian social history.

Peter Morris' *Embattled Shadows* is a history of film production in Canada before the 1939 founding of the National Film Board. He opens with the birth of this new medium of entertainment in vaudeville houses, seedy store-fronts, and community halls across the country, and then turns to his major concern — the conditions under which the limited efforts at movie-making took place in Canada. He

documents the sporadic efforts of Canadian entrepreneurs to create a native movie industry, in the face of limited know-how in the field of commercial entertainment, shortage of capital, geographic isolation (Trenton, Ontario despite its sobriquet of "The Hollywood of Canada," never became a real focus of film production), and the growing monopolization of the industry by American giants, who controlled North American production and distribution by the 1920s.

For nearly a decade after 1914 a new nationalist consciousness in the country stimulated an outpouring of Canadian-made newsreels and features to replace or compete with the largely American product available in most theatres. Social historians will be intrigued to discover that Canadian novels like Ralph Connor's *Man from Glengarry* and *The Foreigner* and Alan Sullivan's *The Rapids* were translated into feature films in this period (though Morris notes with justified regret that few of these gems have survived). To the pantheon of Canadian cultural heroes, he would have us add the flamboyant Ernest Shipman, an impresario who produced several of these films on location in Canada after World War I. Ultimately, however, aside from a brief flurry of activity in the late 1920s in response to British quotas on American films, independent feature-film production in Canada was dead by 1922.

The rise and fall of the film industry has many parallels in other sectors of the economy, notably the new, technologically sophisticated industries like auto or electric parts, where American corporations also quickly eliminated their few Canadian competitors. But the other striking similarity to many Canadian industries was the crucial role of the Canadian state in sustaining what film production there was in the country. Following the lead of the CPR, the federal government sponsored Canadian newsreels for British audiences to help promote immigration

before World War I. During the war several departments funded newsreels and propaganda films, and in 1918 the government launched the Exhibits and Publicity Bureau (later the Canadian Government Motion Picture Bureau) with its own production staff — the first state-sponsored film-production facility in the world. A year earlier the Ontario government had opened the Ontario Motion Picture Bureau, which contracted with private companies to produce films until 1923, when it too opened its own studio at Trenton. Increasingly unable to break into American distribution networks, independent film production in Canada became limited to a documentary work for the state and private corporations.

While Morris documents, with loving detail, the vagaries of film-making in this restricted context, his book unfortunately stops well short of telling us why the Canadian state or the CPR would want to become movie moguls in their own right. That line of inquiry would have taken Morris more deeply into the place of movies within Canadian popular culture and their relationship to their audience, which was overwhelmingly working class before the 1920s. State officials and corporate executives must have recognized that they were intervening in a commercial medium which had grown up within working-class neighbourhoods and responded to real needs and aspirations. The silver screen provided much more than escapist fantasy. From the earliest days of cinema, movie-goers had been fascinated by films which expanded their horizons and their knowledge of places and events which they would never be able to see; the popular press, and even the new post-card craze, fed on the same fascination with access to the distant and unreachable. The state and the corporations soon recognized the value of feeding this curiosity with films which promoted their interests, whether immigration, tourism, wartime patriotism, or a good corporate image. Although Morris prom-

ises at the outset to "situate events within their social context," his emphasis on entrepreneurship and technique within the early Canadian film industry misses the vital element which made it all possible — the working-class audience. As much as government and industrial sponsors, workers' tastes shaped the kind of film production which took place in Canada.

For labour historians, probably the most remarkable manipulation of this form of popular culture which Morris unearthed was the CPR's sponsorship of *The Great Shadow*, a 1919 red-scare flick which starred Tyrone Power and depicted the attempt of a group of Bolsheviks to win control of a union in a shipbuilding plant, only to be defeated by a more conservative labour leader (in the mould of TLC President Tom Moore). Several Toronto employers supplied their workers with tickets to this movie, which enjoyed a modest success in the United States as well.

Forsyth Hardy's biography of John Grierson takes up the story where Morris leaves off, since Grierson took over administration of the new National Film Board in 1939 and launched it on its illustrious, often controversial career. He brought with him the new ideas and techniques of the documentary film movement which he had inspired and led in Britain, and set about to make films in Canada about "real people." Around him he gathered a large staff of creative young artists, technicians, and administrators, many of whom would continue in this work for years afterward. In 1945 the gruff, imperious Scott left the NFB with a brilliant record of film production behind him, only to face the indignities of fierce red-baiting in the investigations of Igor Gouzenko and Soviet espionage in Canada a year later. His influence on post-war Canadian film-making was nonetheless profound (movie critic Martin Knelham has dubbed him "The Father of Us All"); both the earnest, often pedantic quality of

the NFB shorts with which we all grew up and the social realism of so many feature films since the 1960s bear the mark of Grierson's thinking on documentary work.

Hardy's portrait of his old friend is heavy on anecdote and itinerary (often recounted with leaden pen) and quite light on analysis of the development of Grierson's theory of documentary film work and its impact on Canada. The fascinating detail about Grierson's intellectual apprenticeship on the Red Clydeside in the early 1920s is never properly linked to the man's later propagandizing for documentary films. Did Grierson see himself as part of the socialist cause (we learn in passing that he wrote for the *Clarion* until 1932)? Were he and his associates socialist realists of a Fabian stripe? Did they have affinities with the adult education movement which produced the Workers' Educational Association and, in part, inspired the Canadian Broadcasting Corporation? The book prompts these questions but provides at best incomplete answers. Thanks to Grierson's influence, the NFB has left us with a cinematic record of many aspects of Canadian workers' lives, and, as historians turn back to these for insights into twentieth-century working-class life, they will need to be forewarned about the intentions and the biases of the documentary tradition which produced them.

Peter Morris and Forsyth Hardy have opened up an area of historical inquiry which deserves much more attention. We are still a long way from a full, satisfying history of movies as a form of popular culture in Canada — something in the vein of Robert Sklar's *Movie-Made America*. We are still further from an integrated perspective on twentieth-century working-class history which can help us understand the world of men and women who, in the same period, sat spellbound in darkness before images of Shirley Temple, James Cagney, and Mae West and

then marched in picket lines to build industrial unionism.

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Tom Wayman, ed., *Going for Coffee: Poetry on the Job* (Madeira Park, B.C.: Harbour Publishing 1981).

TOM WAYMAN'S anthology of contemporary North American working poems had been advertised as achieving "in poetry what the Studs Terkel classic *Working* did in prose." This is to claim both more and less than comparison allows.

Like Studs Terkel, Wayman casts a wide net in gathering together over 250 poems by some 90 U.S. and Canadian poets in all, poems about every conceivable kind of occupation from bartending to doctoring, policing to oil-drilling, typing to any one of a dozen varieties of assembly-line work. The jobs range from the ordinary and lifelong to the odd, but the poems for the most part stay within the boundaries of the colloquial and immediately intelligible. The moods go from comic to tragic, from horse-play and fantasy designed to fill in the long hours of a shift to the drama and horror of violence and accidental maiming and death. The actors include familiar stereotypes (the cruel, domineering foremen; the fat, soft, selfish capitalist-owner-boss; the lackey-cop serving the establishment and private property; the unscrupulous manager ignoring safety regulations and crushing employee rights; the sex-exploiting hirer and firer; and a dozen other recognizable caricatures of the powerful faces in the workplace), and the less familiar and less identifiable characters, faces and personalities of the individual workers themselves, driving cabs, selling in stores, slaving in mills and slaughter-houses, restaurants and factories, trying to endure the hours which they often feel to be utterly

meaningless and unspeakably boring — though charged with possibilities for love and hate, cruelty and fun, which begin the moment two people or more are set down beside each other to make their living.

But Studs Terkel's method is that of the tape recorder. Wayman has collected poems, works of art which are fabricated, condensed and shaped, however simple and direct they try to seem, sweated over in their own kind of labour in some cases for many hours, away from the scenes and people they attempt to capture in words. And the more than 90 voices we hear are not really a cross-section of the working population, as Terkel's are supposed to be. Virtually all of Wayman's choices have something in common: they are writers by vocation, workers at other jobs by necessity. Most are publishing poets, some excellent; one is a Governor-General's Award winner, many are academics. How well do they reflect the actual working population? How far are they in fact alien voices? (One of Wayman's poets jibes at this possibility: "Canada Council gives 'em ten thousand bucks / to write about what it's like / to be a workingman.")

These questions are probably unanswerable. The indeterminacy principle must surely apply: any authentic unlitery workingman invited to read these poems would have his consciousness modified, if not created, in the process of testing their validity.

This is not, however, a "consciousness-raising" book in the usual sense of that phrase. Consider what a comparable anthology from, say, the 1930s would have to say. In Wayman's collection there is no unified cry of angry protest, no passionate class consciousness, no insurgent ideology — virtually no politics at all, to speak of. What we get is working as an "experience" in intimate and suggestive detail and with extraordinary eloquence and vividness: the lived experience of the workplace in hundreds of different examples. What we want to

learn from or make of the sum total of that revelation is entirely up to us.

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

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

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

On peut toujours reprocher à un ouvrage de synthèse d'avoir ignoré tel événement, d'avoir escamoté tel autre ou de laisser de côté des éléments d'interprétation. C'est inévitable dans une synthèse qui couvre près de deux cents ans d'histoire. Les auteurs ont du faire un choix d'événements et insister sur certains aspects plutôt que d'autres. À mon sens, ce choix a été fait avec intelligence et beaucoup de métier. Si on peut pardonner des lacunes au niveau de la connaissance et de l'interprétation, il faut être cependant plus sévère du côté de la connaissance de l'historiographie du mouvement syndical.

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

À un autre niveau, il est intéressant de relever la conception que les deux auteurs possèdent de l'action syndicale et de

situer leur perception de la présence du syndicalisme international au Canada. L'approche adoptée dépasse l'idée que le syndicalisme est un simple problème de relations industrielles; ils situent son évolution dans le cadre de l'émancipation de la classe ouvrière. Le syndicalisme et l'action politique ouvrière leur apparaissent comme les moyens par excellence de promotion des travailleurs. Ils ne minimisent pas les luttes ouvrières, mais ils évitent de les interpréter dans le sens d'un conflit irréductible entre classes sociales. Leur analyse laisse penser que notre société capitaliste et démocratique possède la flexibilité nécessaire pour assurer l'avancement des travailleurs.

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

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

il serait davantage la préoccupation des intellectuels et des politiciens.

Ces quelques critiques sur lesquelles nous avons insisté, ne doivent pas faire oublier que l'ouvrage constitue un apport précieux à l'histoire des travailleurs. En général, il révèle une bonne connaissance de l'historiographie du mouvement ouvrier et ne contient pas d'oubli factuel majeur. Les auteurs insèrent adroitement l'évolution du syndicalisme dans le contexte économique et politique de chacune des époques. Au niveau de l'interprétation, ils évitent les recours faciles et cherchent à présenter un point de vue balancé en serrant les événements du plus près possible. Leur volume représente, à n'en pas douter, la meilleure synthèse du syndicalisme canadien.

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Thomas Dublin, ed., *Farm to Factory. Women's Letters, 1830-1860* (New York: Columbia University Press 1981).

WHEN THOMAS DUBLIN'S prize-winning volume, *Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826-1860*, was published in 1979, it earned for its author a pre-eminent position within the new generation of North American social historians. *Women at Work* is a model of how quantitative techniques can be skillfully combined with traditional historical research to achieve a comprehensive and subtle analysis. Dublin's achievement is best exemplified in his painstaking reconstruction of the lives and experiences of the female operatives by means of company records, census returns, workers' petitions, newspapers, diaries, and private correspondence. It is his extensive use of the latter two sources which adds so much of the vitality and sensitivity to his study. Given the high expectations created by the first volume, perhaps it is not surprising

that this second endeavour should prove somewhat disappointing.

*Farm to Factory* is based on four collections of private letters written by, to, or about young women who worked in the New England textile mills. In addition, they provide some insight into the lives and work of women in other contexts — on the farm, in the schoolhouse, in the shop, and in one case, in a utopian community. Selections from all four collections had been included in *Women at Work*, and the human dimension they added to it no doubt convinced the author and others that a lengthier edition of this private correspondence would be desirable.

While the more recent volume permits us to contemplate the lives of women such as Sarah Hodgdon, Jemima Sandborn, Mary Paul, and Delia Page over a longer period of time, there is an annoying element of repetition in it. Many of the most informative comments contained in the letters are found not only in *Women at Work* but also in the general introduction of *Farm to Factory*, and in the introduction to the particular collection from which the correspondence was selected. Consequently, by the time we read some passages in their original context, it is for the fourth time. It is unfortunate that Dublin does not provide more variety in the extracts he incorporates in his expositions, for he fails to indicate the range of subjects and attitudes conveyed by the correspondents. For example, rather than quoting Mary Paul's statement to her father ("I hope sometime to be able to do something for you and sometimes feel ashamed that I have not before this") twice again in this book, he might well have chosen some of her illuminating comments about her work and wages (101, 103) or about housework and equal pay for equal work. (113)

The introductory essay to this volume is essentially a reworking of material contained in the original study. Although it provides an excellent account of the his-

torical context within which the individual experiences represented in the letters must be viewed, too much of it is simply a verbatim reiteration of sections of *Women at Work*. Regrettably, Dublin does not take this opportunity to offer us fresh perspectives into these particular sets of documents.

The actual selection and elucidation of the correspondence is generally well done. The correspondents are clearly identified, and the editor has been thorough in following up references to persons, places, and events, and explaining them in the footnotes. Each collection makes a unique contribution to our understanding of the hopes, fears, problems, and relationships of working women in antebellum New England. The letters evoke and enunciate a number of important themes: the degree of personal independence the women were able to exercise; the nature and role of kin and family networks; and to a more limited extent, the reaction of female employees to their working conditions. In the conclusion to the book, Dublin reflects on the lacunae in the correspondence: "... two things are strikingly absent. First, ... one has no sense of the broader economic and political conflicts that arose in the mills of this period. ... Also strikingly absent from the letters of mill women is any reference to immigrants." (188-9) While he suggests plausible explanations for the latter omission, a satisfactory account of the former is not provided.

The format of this volume is attractive and several interesting illustrations are included. Given his attention to detail, it is surprising however that Dublin has not included a single map. The letters reveal that the women were in many cases very mobile; therefore, it would have added significantly to our knowledge if a map or maps had been included at the beginning of each section indicating the geographical moves of the principal individuals which are mentioned in the letters.



*Farm to Factory* is a useful and interesting work because rarely are we permitted to enter into the intimate circle of family and friends of "ordinary folks" in the early nineteenth century. It is to be regretted that Thomas Dublin has not succeeded in his commentaries in conveying the originality and scope of the material.

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Paul G. Faler, *Mechanics and Manufacturers in the Early Industrial Revolution: Lynn, Massachusetts* (Albany: State University of New York Press 1981).

EDWARD THOMPSON's *The Making of the English Working Class* has been an inspiration for a whole generation of labour historians. Led by Herbert Gutman and David Montgomery, the past 15 years has seen American labour history crawl out from under the massive weight of the Wisconsin tradition. In the process labour history has become the history of the working class. Paul Faler's *Mechanics and Manufacturers in the Early Industrial Revolution* takes its place within this literature. Focusing on the impact of capitalism on the shoemaking industry in Lynn, Faler convincingly demonstrates that the transformation of the economic relations of the industry — the decline of the master cordwainer, the rise of the manufacturer, and the spread of outworkers — changed the social and cultural life of the community and led in time to the rise of an explicit class structure.

Faler's focus alternates between an analysis of the dynamics of early capitalism within the shoe industry and the development of Lynn as a community. Although shoes had been made in Lynn since the seventeenth century, it was only the protection provided by the Revolution and the nationalist economic policies of

the early republic that allowed the industry to take root in Lynn.

The early shoe industry, based on small masters and journeymen, occupied a "status halfway between the cottage economy of the subsistence farmer and mechanized factory system." (19-20) In its early phases, the development of production for the market had little impact on the internal organization of production. Merchants simply served as middlemen between the producers of Lynn and the markets that developed first in the East and later in the West and South.

The craft organization of the industry led to a fiercely independent, artisan culture. Based on the labour theory of value, the shoemakers' world-view rested on two tenets: the producer ideology and republicanism. The effect of this outlook was to focus on the "drones" of society — bankers, merchants, and lawyers — as the enemy and to obscure the differences between masters and journeymen.

Yet, in the meantime, the older organization of the industry was slowly declining, replaced by a capitalist system in which the shoemakers became wage-labourers. Although in theory each shoemaker remained independent ("buying" his materials from the manufacturer and "selling" finished shoes back to him), Faler asserts that, "by 1830, and perhaps much earlier, the shoemakers in the town were full-time wage earners with no important means of support other than what they derived from making shoes." (84) In addition, the manufacturers increased the use of outworkers located in rural New England to perform much of the work, thereby undercutting the power of the urban cordwainers. Thus, capitalism, understood as an economic system in which labour itself becomes a commodity, appeared in the shoemaking industry a generation before the factory came on the scene.

This economic division between manufacturer and worker did not make its impact on the society and culture of the

city felt for some time. However, the manufacturers, many of them Quakers, slowly developed an outlook and a set of institutions, particularly the Society for the Promotion of Industry, Frugality and Temperance, which set them off from the traditional mechanics' culture of the wage-workers. Then, in time, shoemakers became aware of their objective subordination and formulated a new ideology which identified the manufacturers as "drones" who also were a threat to the mechanics' economic independence and political liberty. This consciousness-raising culminated in the strike of 1860 which marked "an end to an appearance of harmony in the community," and the rise of class conflict. (222)

This book is based on Faler's dissertation which was completed in 1971. At that time it was truly pathbreaking. Gutman and Montgomery's work was relatively new, and Faler was one of the first generation of students to bring these insights to bear using the community study methodology. If this dissertation had been published in the early 1970s, it would have been hailed as a major breakthrough. But time does not stand still. The last decade has seen an outpouring of scholarship on the working class during the early industrial revolution including the work of Bruce Laurie and Alan Dawley in the United States and Bryan Palmer and Gregory Kealey in Canada. Whereas, although a pioneer work when it was written, now Faler's book is simply an addition to a well-developed literature.

Furthermore, Faler has not kept up with the theoretical development of the field. The beginning and end of the theoretical structure of this book is E.P. Thompson. Although Thompson's insight into the nature of social class liberated social historians from the straight-jacket of determinism, his work has not gone unchallenged. Much work still needs to be done on the relative autonomy of culture and its relationship to the structural determination of class position. Rather than

simply following Thompson, Faler could have used his study as a means of addressing this difficult issue.

Indeed, Faler at times seems to be content with simply mimicking Thompson. Here we have an American version of Thompson's discussion of religion, here we have a version of Thompson's discussion of time and work discipline, here we have a discussion of "free-born Americans." (188) Why, Faler almost finds an enclosure movement in the development of summer resorts of Nahant. (190) This is all fine in a dissertation, but Faler should have made better use of the last ten years.

The other shadow in which Faler finds himself is that of Alan Dawley. Dawley's research for *Class and Community* was undertaken at the same time as Faler's. Yet, his book appeared five years earlier and won the Bancroft Prize. Although both he and Faler have gone out of their ways to praise the other's work, this does not negate the fact that Dawley got it out first. Although Faler provides a greater degree of detail and precision about the early development of the shoe industry than Dawley, there are few themes or questions here that Dawley has not treated.

In summary then, *Mechanics and Manufacturers* is an important addition to our knowledge of the development of the working class in nineteenth-century America. Yet, its earlier appearance could have been more useful both to its author and to the field.

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Philip S. Foner, *British Labor and the American Civil War* (New York: Holmes and Meier 1981).

WHAT STANCE DID British working people take in relation to the American Civil War?

Dr. Foner finds that historians fall into three camps in terms of how they respond to this question.

First, there is what he identifies as "the traditional view." He points out that for almost a century historians "upheld Marx's contention that while the ruling class supported the Confederacy, the British workers unanimously condemned the Slave Power; that the Lancashire cotton-mill workers made real sacrifices to prevent British intervention from breaking the blockade and restoring the flow of cotton to the mills — a sacrifice which Lincoln himself called 'an instance of sublime Christian heroism which has not been surpassed in any age or country,' and that the British workers were a significant, if not the most important factor in preventing intervention on the side of the Confederacy." Foner goes on to cite Richard B. Morris in his *Encyclopaedia of American History*: "The war divided British opinion. The upper classes favoured the Confederacy. The commercial interests, irked at the new high tariffs imposed by the Union, looked to the opening of a vast free trade market in the Confederacy; British manufacturers and shippers expected to benefit from the defeat of their Northern competitors. The working class, however, and a large proportion of the middle class, favoured the Union."

The second camp is made up of "complexifiers" or mini-revisionists. Historians of this persuasion dispute the contention that the working class was unanimously for Lincoln and the North. They draw attention to differences that emerged over time and between regions and between generations. (Veterans of the 1840s; of the chartist years who did not go on to socialism as Ernest Jones did, tended to see John Bright and all his affections and all his works as the main enemy. They were drawn to the Confederacy out of detestation for the dollar-worshipping, Bright-like Yankee). A study of the labour press provided a great deal of evidence of this pro-Confederate presence.

On this and other grounds the adherents of this camp tended to question claims that British labour had thwarted the war party in Britain. But they never doubted that in mobilizing the working class in defence of the Federals the new generation of British labour leaders were helping to create the conditions for the carrying of the Second Reform Act in 1867.

Dr. Foner makes your reviewer "the most influential" champion of this, the second camp: one which he sees establishing itself in the 1950s. His impatience with my pedantry and empiricism is largely implicit. When it comes to my inconsistencies and muddle-headedness, he cannot be so discreet or tolerant. He finds a very substantial difference between the argument of my *British Labour and the Confederacy* (1957) and my *British Labour and American Slavery* (1961). I am singing a "different tune:" — a better and more traditional one! Alas! The damage has been done! "Strangely enough, Royden Harrison's original article, 'British Labour and the Confederacy', and not the amended version he published later" (in *Before the Socialists*) "became the basis of the revisionist historiography of the role of the British workers in the Civil War."

There is some justice in Foner's complaint. Taken aback by my own discovery about the bias in the British labour press: excited by my hypothesis concerning how to account for it: anxious to get a hearing against a complacent orthodoxy and writing in a special year (1956) I over-stated my case. Had I been an adherent of that monumental marxism of which Dr. Foner is such an energetic and accomplished exponent I would not have given the third camp, the camp of the full blooded revisionists, their opportunity: I would have recognized the "objective significance" of my own offering. I would have waited until my considered view had matured. And made my pen into a chisel! Foner finds the instincts of the third camp well expressed, as early as 1931 when

F.L. Owsley characterized the British workers as: "politically apathetic, sullen, ignorant, and docile, with the exception of a few intelligent and earnest leaders." Basing himself on a rather careless reading of my work, J.M. Hernon in the *Journal of Southern History* in 1967 thought to "flesh up" Owsley and allow the possibility that "a majority of British workingmen sympathized with Confederate independence." The full blooded revisionists then cast off all restraint. In *The History of a Myth: the British Workers and the American Civil War*, the epilogue which Peter d'A. Jones contributed to Mary Ellison's *Support for Secession: Lancashire and the American Civil War* (Chicago 1972), the traditionalist view is simply turned upside down. "The picture which emerges," wrote Francis Nicolson in the *Canadian Review of American Studies* (Fall 1973), "is as exaggerated and over-simplified as that which prevailed before."

Having made his preliminary characterization of the three camps, Foner proceeds to a re-examination of what he takes to be the main types of evidence: that supplied by the press, by the platform; and by the occasional well-placed observer or participant. With respect to the press, the reader is introduced to little that is new and there is some muddle with respect to what is familiar. For instance, "Scourge" writing in the *Bee-Hive* is identified as "a leading trade unionist," (58) then as "Schrooge [*sic*] who was Frederick [*sic*] Harrison" until he is, at last, correctly identified as Robert Hartwell, the old Chartist! (63) In discussing meetings, Foner has much useful material on the gatherings occasioned by the arrival of the relief ship, the *George Griswold*. But here and elsewhere he is as uncritical about evidence which supports his case as Mary Ellison is about the evidence which supports hers. If it must discredit Ellison to find that she nowhere refers to the *Index* nor to Henry Hotze, the Confederate agent working in England, it

certainly does not inspire confidence in Foner's respect for evidence or his sense of significance to find him attempting to recover for Karl Marx, the role of organizer of the great St. James Hall meeting in which the new trade union leaders joined hands with John Bright. Since Foner has not a jot of fresh evidence, what exactly is at stake here?

It is the want of fresh evidence which makes this monograph less useful and less exciting than it might otherwise have been. It is a merit of Ellison's book — I am still enough of the decadent liberal to find merit in work with which I profoundly disagree — that she repeatedly refers to *petitions*. What she fails to do is to reflect about their specific meaning and importance in the politics of mid-Victorian England. She seems only half aware of the official records of petitions and petitioning and then usually to prefer, for some unexplained reason, the references in the pro-Confederate press. Without explanation she appears to attend more to the number of *petitions* than to the number of *petitioners*. Perhaps, in the matter of petitions, as in the matter of meetings, a multiplicity of small efforts was characteristic of the pro-Southern forces and points to weakness rather than to strength. A first reconnaissance of this terrain suggests that it would have supplied Dr. Foner with a happy hunting ground.

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Eric Foner, *Politics and Ideology in the Age of the Civil War* (New York: Oxford University Press 1980).

IF CANADIAN, AMERICAN, and British social historians have anything in common, it is the growing awareness that the "new social history" of the past 15 years

has produced, not a new paradigm of historical understanding, but a fragmented and chaotic conception of the past without cause, process, and larger meaning. In American history, certainly, the older Progressive and consensual frameworks of analysis have been shattered, but, as Eric Foner argues, no new synthesis has taken their place. While recent social and political historians have contributed rigorous methods of analysis and have embraced subjects previously ignored, E.J. Hobsbawm's call for "a shift from social history to a 'history of society' [has] remained largely unfulfilled." (6) Of course other historians share Foner's misgivings, and whether one general interpretation or another will pick up the pieces is an open question. Bernard Bailyn and J.G.A. Pocock, for example, hope to provide coherence by integrating ideological, structural, and demographic developments in Western history, and by adopting the notion of "large-scale spheres and systems organized as peripheries and cores," much of this as a way of by-passing attention to modes of production, social relations, and class. Among those historians who believe that a coherent understanding of the American past is not only possible but is already in the making by a new generation of scholarship which does not ignore these concerns but which is equally capable of absorbing new findings, Eric Foner is one of the most talented and theoretically acute but subtle practitioners. Herein lies the importance of this collection of essays, which reflects Foner's "ongoing desire to reintegrate the political, social, and intellectual history" of the era of the Civil War (1830-80). In addition, the book is a useful supplement to Foner's two brilliant monographs, *Tom Paine and Revolutionary America* and *Free Soil, Free Labor, Free Men*. It explores some themes in greater detail and it includes one essay, "Reconstruction and the Crisis of Free Labor," previously unpublished. Considering that these essays span 15 years and that Foner is not

quite 40, his achievement is all the more remarkable.

Three sections of essays follow the introduction, itself a masterful analysis of the state of American historiography and of where and how Foner's work fits in. Part I, "The Origins of the Civil War," contains a judicious critique of the most influential recent interpretations of the era — the "new political history" and the "modernization" model — and argues for the relocation of the Civil War in political, ideological, and social developments. These have been "eclipsed" in recent Civil War historiography, which has substituted the study of ethno-cultural affiliation and a fascination with quantitative method for politics — "the way in which power in civil society is ordered and exercised" — the neglect of which "deprived social history of the larger context which alone could have imparted to it a broader meaning." (9)

Part II, "Ambiguities of Anti-Slavery," integrates politics with race, class, and artisan radicalism. It shows how the racism of the New York Free-Soilers allowed anti-slavery to enter politics respectively for the first time but in its least radical form, that is, shorn of a commitment to equal rights which would not be reintroduced into antebellum politics again. The important and outstanding essay on "Abolitionism and the Labor Movement in Ante-bellum America" for the first time puts the new labour history and mechanic ideology effectively into the anti-slavery picture. Mechanic anti-slavery, hitherto obscured by the preoccupation with evangelical religion, was based in the republican tradition of the American Revolution which "equated freedom with the ownership of productive property." This clashed with the "newer, liberal" definition embraced by the Abolitionists, for whom freedom meant "self-ownership — that is, simply, not being a slave." (64) One of the achievements of the Republican party in the 1850s was the successful incorporation of features of

each of these distinct ideologies into a political platform which spoke to mechanic ideology but which at the same time was able to channel this into a critique of the South rather than an indictment of the northern social order. At the same time, however, Foner argues for the persistence of artisan radicalism — revising the arguments of David Brion Davis, Alan Dawley, and others that it was shunted aside by the Civil War and by the hegemony of middle-class values. Foner sees it rising “like a phoenix from the ashes” of the war to inspire subsequent labour movements of the Gilded Age.

Part III, “Land and Labor After the Civil War,” pursues these post-war developments. “Reconstruction and the Crisis of Free Labor” shows that it was not race but the continuing “tension between capital and labor” which was at the centre of the Reconstruction period. The essay on Thaddeus Stevens and confiscation explores the same problem, as well as illuminating the predicament of a radical in politics, while the last essay on the Land League and Irish-America is a model of how to treat class in relation to ethnicity and radical politics.

Two points about these essays need emphasizing. First, there is a central argument that ties them together: that it is impossible to understand American history from the Revolution to the Civil War without studying the relationship between republican ideology and capitalist social relations, “the contradiction between republican thought and the expansion of capitalist production . . .” (10) Relatively harmonious for a time, in the era of the Civil War capitalism and republicanism parted company. Hence, in Foner’s conceptualization, post-war America was not simply a tragedy for race relations, as it is usually depicted, but was the era when the conflicts and failures implicit in American capitalism for a 100 years could no longer be avoided. (127)

Second, the essays as a whole reveal the strengths which have allowed Foner to

avoid a simplistic application of Marxist analysis yet still write social and intellectual history in class terms. The hallmarks of his scholarship are his sensitivity to ideological process and his appreciation of the theoretical and methodological issues involved in such study, qualities largely absent from the work of social scientists and political and quantitative historians. At the same time, it is Foner’s grasp of the larger contours of social and historical process that provides meaning to the themes that run through these essays: the ironic turns and unfulfilled intentions of the era, the complexity of anti-slavery ideology, the transformation of the meaning of labour, republicanism, freedom, and equality in American life, and the ambiguous legacy of the Civil War, which was both “a new birth of freedom” and the source “of much that was wrong with Gilded Age America.”

While no group of essays alone could achieve Foner’s purpose, herein is a conceptualization capable of reintegrating the now fragmented findings of recent American historiography. Foner’s Marxism is a challenge. It is neither simplistic nor reductionist, and no objection to it upon the high ground of historical scholarship has been forthcoming, unless this be that the effort to understand the course of history is itself futile. To the extent that historians themselves have contributed to this abandonment of “collective self-education” (12) and have lost sight of the relevance of the past, Foner’s work will stand as a continuing commitment to turn this around.

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Bruce Palmer, *“Man Over Money.” The Southern Populist Critique of American Capitalism* (Chapel Hill: University of North Carolina Press 1980).

THERE HAVE BEEN great advances this past decade in American scholarship which re-examines the Populist interlude in America. Palmer's book is another significant step in that advance. Not content with the polarized debate in earlier American scholarship on Populism — was it reactionary or progressive? — Palmer's study is a solid contribution to those who have begun to demonstrate that Populism was both progressive and reactionary, often simultaneously. More than that, Palmer shows, with sympathy and insight, that Populism, even Southern Populism, often portrayed as a movement of crackers and rednecks, was, on balance, largely progressive and articulated "a remarkably accurate analysis of the injustice and human toll the advance of American capitalism took" (89) and represented "the last major mainstream political attack on capitalism and its business culture in America." (xviii) Indeed, Palmer goes further and challenges some deeply held prejudices about Southern Populism when he asserts, and largely demonstrates, that the movement there was "thoroughgoing in their assault." (xviii) While one may want to quibble with such sweeping assertions, Palmer's data and analysis are convincing.

A theme which recurs throughout the analysis is that Southern Populism was a protest against a modernizing capitalism from the point-of-view of the petite bourgeoisie, in this case, the agrarian petite bourgeoisie. But, Palmer shows, it was more than that. It was also a systematic effort to advocate and struggle for an alternative developmental design to that which was in place, a design made up of "a two-tier economic structure which combined essentially a simple market society for the large number of independent producers with democratic government ownership and operation of those elements of industrial society which for one reason or another could not be controlled by individuals." (121)

In common with Populist movements elsewhere in the world, the Southern Populists failed in their efforts. Palmer's analysis of that failure is clear-sighted if overly friendly. Largely members of the agrarian petite bourgeoisie, the Southern Populists could not transcend their basic commitment to private property and commodity production. They were unable to understand fully the forces that confronted and were destroying them. They failed to transcend the race question, though their record was not as bad as generally believed. They failed to win the sympathy of labour, since they could not identify completely with labour's problems in industrial capitalism. Therefore their solutions — a simple market society of independent producers and the government ownership largely of the means of distribution — were class solutions and, though they were posed for the general good, such remedies failed to convince the allies the Populists needed, the urban petite bourgeoisie and the urban working class (and the blacks, of course). The Southern Populist view of effective social change and meaningful reform was confused and flawed: "any government would be all right with good people in it;" (48) "the natural and just law of supply and demand" should be upheld; (41) "the government should be the agent of the people;" (41) a belief in the "strict adherence to the law;" (34) workers should "vote right instead of striking;" (35) their fear of violent class struggle as a millenarist "chaos before order." (129) They had no theory of social and economic change but a moral one: "When public issues turned into questions of personal morality the solutions to them tended to become those of personal and private morality, not those of social, economic, and political change." (137) They equivocated on fundamental issue after fundamental issue.

Palmer's book is a gold mine of information for those curious about details of the debate between anti-monopoly greenbackers and free silver advocates. Further,

the work details on a Southern state-by-state basis the events and ideas which led to fusion with the Democratic party in 1896. The book is rich in detail as it documents the nature and evolution of Southern Populism's ideology in its efforts to wrestle with the unregulated modernization of American capitalism. Further, the book tells part of the story of how the new middle class in the South emerged to lead the South's final integration into American industrial capitalism. As such, it is a valuable source book for anyone who wants to learn about the era, the movement, and the region.

Yet the book is finally somewhat unsatisfying. Palmer walks up to and confronts the class reality of Southern Populism, but shies away from taking the next logical analytical steps. Part of this problem results from his method and the sources of his data: his analysis rests on the newspapers, speeches, and documents, of the Populists themselves. Clearly this work tells us much about what the Southern Populists believed and advocated; and Palmer uses such sources brilliantly in an imaginative textual analysis. Since Southern Populism, unlike Canadian Populism, failed to win decisive power in any state, we cannot know how they would have acted in power. However, relating his material to the actual social, economic, and political changes occurring in the American social system, and more clearly to the general political and economic crisis of that era, might have told us more about the failure of Populism. Why were the Populists unable (or unwilling) to develop a political alliance with labour? Did labour trust the Populists? How did the working class view them? What was the Populist record on farm wage labour? On tenancy? Most important of all, given developments in the American political economy, could the Populists have been any different, ultimately? Palmer holds up the Texas and the early Georgia movements as models, saying, significantly, "where their

analysis remained most radical... the Southern Populists succeeded best." (196) Would they have succeeded had they moved left? Why did they not move left? More basically, could they have moved left?

This is the most unsatisfying aspect of the book. Palmer seems to suggest that through better analysis, better insight, better leadership, and, indeed, a deeper commitment to basic change, the Southern Populists could have done more. There is, then, a stream of extreme voluntarism running through his analysis (something for which he criticizes the Populists). One is led to believe that had the Populists transcended the race issue, that had they come to grips with the essence of capitalism itself, that had they become more socialistic, American capitalism might have been transformed significantly. In fairness, Palmer never explicitly says as much, but the implication haunts the pages of his book like a lost promise. One striking example of Palmer's failure to take the next step in analysis is the way he handles the Populist conception of a simple market society, a society of independent producers of tangible wealth gaining earned and just rewards in the marketplace, a society where "all wealth will belong to him who creates it." (205) Of course, as Palmer notes, such a conception assumes there is no market for labour. Such a simple market society never existed in America. It may have existed in various pre-capitalist social formations, but it never existed in America. The conception, therefore, is not so much a result of flawed thinking as it is a reflection of class myth, a myth designed to defend and justify the concrete class interests of the agrarian petite bourgeoisie — private ownership of land and commodity production — while at the same time providing a moral and intellectual basis for an attack on big capitalism. For the Populists to have rejected that myth and to have embraced, for example, social ownership would have amounted to



them advocating the voluntary and more or less abrupt liquidation of their class rather than the slow, lingering liquidation they confronted in a modernizing industrial capitalism. Ultimately, then, Palmer is too kind to the Southern Populists.

The book, however, is exciting. It cuts through the old debates and empirically documents the basic correctness of the traditional Marxist approach to Populism. Populism was the political expression of the agrarian petite bourgeoisie in the class struggle which characterized modernizing, industrial capitalism. Populism expressed a critique of capitalism and a proposed developmental alternative from that class's point-of-view which sought, above all else, to defend that class's particular interests. Yet Populism was more than just a cynical, self-interested effort by a class to save itself, it was also an effort to build a better society. Hence Populism is typically both reactionary and progressive, both committed to a myth of the old ways of a simple market society based on private property and commodity production and to a new, more humane and egalitarian era of plenty and justice. As Palmer says, "For all their provincialism, narrowmindedness, and confusion, the Southern Populists, at their best and from their own particular standpoint, realized that a social order which set every person against every other, each group or class in society against every other, the Devil taking the hindmost, represented only exploitation to most of its members and would very likely destroy itself eventually." (221) That has always been the Populists' strong suit — a moving critique of the human cost of the unregulated modernization of industrial capitalism. But it was not enough. Palmer seems to believe more could have been accomplished by the Southern Populists at the same time as his data and analysis show us exactly why they could not have done much more than they did. Their accomplishments were remarkable, given their location in the American class system. The Southern

Populists' epitaph should record those remarkable accomplishments, not list the things they failed to do in a lamentation about what they never were and never could have been.

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Mari Jo Buhle, *Women and American Socialism 1870-1920* (Urbana: University of Illinois Press 1981); and Nancy Schrom Dye, *As Equals and As Sisters. Feminism, Unionism, and the Women's Trade Union League of New York* (Columbia: University of Missouri Press 1980).

MARI JO BUHLE's previous work has focused on women in the Socialist Party of America. If she had continued to study this single manifestation of socialism in the United States, we would have had a solid if partial study of the intersection of socialism with the "woman question" in the early twentieth century. Much to her credit, however, Buhle has pushed the parameters of her original study back to the earlier period and out from its institutional framework. Using an array of sources, which include foreign language newspapers and biographical materials, Buhle outlines a socialist tradition for United States women, a tradition that drew on both native-born feminism and radical immigrant socialism. As Buhle notes, the memory of this tradition was erased in the 1920s and 1930s as a new generation of women associated themselves with the CPUSA and rejected the "entire reformist legacy of the socialist movement." (318) Buhle's book is usefully complemented by Nancy Schrom-Dye's monograph on the Women's Trade Union League (WTUL), an important organization for working women and socialist women in the early part of the century. Buhle's book attempts a synthe-

sis, while Schrom-Dye's is more clearly a case study which addresses the relationship between feminism and unionism. Both books explore class and gender questions historically with particular concern for women's experiences.

Buhle's long-awaited revision and expansion of her doctoral thesis spans the period 1870 to 1920. Beginning with a brief discussion of post-1848 German immigrant support for women's rights and international peace, Buhle moves on to discuss the role of women among later German socialists, American reformers, and in the SPA. German socialists of the 1870s and 1880s rejected aspects of the American reform tradition (particularly temperance and currency reform) and turned to the task of building trade unions and a political party. Women among this generation of German immigrants formed auxiliaries and bypassed women's rights issues. With the emergence of the Socialist Labor Party in 1877 and the recruitment of the native-born veterans of earlier reform campaigns, the woman question reemerged. Bebel's *Woman under Socialism* helped to bring the woman's question to prominence, to create autonomous women's organizations, and to begin the questioning of romantic socialist views of women.

One of Buhle's most important contributions to women's history (and to our understanding of class relations in late nineteenth-century America) is her second chapter which attempts to sketch the relationship between the women's movement and socialism. The club movement, the WCTU, and the International Council of Women stressed the autonomy of women's organizations and identified the key issue of women's right to labour. WCTU leader, Frances Willard, best illustrates the attempts of the women's movement to link up the major reform questions of women's rights, temperance, and labour. Willard's support of Bellamy's collectivist philosophy of nationalism reflected a common pattern among urban

women in reform circles, while rural supporters of temperance tended towards populism. Willard's attempt to convert the NCW and the WCTU to this type of Christian socialism ultimately failed as did her bolder attempts to build alliances among reform organizations with divergent ethnic and political components. Nationalism and populism proved far more attractive to native-born women than the "foreign" socialism of the SLP. While Buhle has undoubtedly contributed much to our knowledge of the links between feminist groups and socialist ones, the reader is disappointed, however, to find so little on important radical labour groups such as the Knights of Labor who proclaimed belief in the equality of the sexes as well as the races. The limited realization of this goal suggests that radical labour and socialist organizations subscribed to a primarily domestic role for women, a role which only began to be challenged successfully in the twentieth century.

With the creation of the SPA in 1901, grass-roots organizations of women received new impetus and separate socialist women's groups emerged to parallel autonomous groups in the women's movement. While auxiliaries were more common, some independent women's groups emerged within the party. The role of women in the socialist movement grew through the influence of recent immigrants and a second generation of native-born women. Despite the SPA's attacks on independent women's groups, the strength of the socialist women's groups pushed the party to recognize women as part of the class struggle, a position supported by the Second International. In 1908 formal status was granted to the Woman's National Committee (WNC) of the SPA, which functioned until the war.

Even prior to the creation of the WNC, socialist women had found party attitudes toward working women unsatisfactory; the dual strategy of the SPA, which recog-

nized the need for unions as well as political action, failed to provide significant aid to working women who faced not only changing work processes (deskilling), but also recalcitrant male unionist attitudes to woman's role. Some socialist women turned to the strategy of cross-class alliances in the Women's Trade Union League (WTUL) whose members were middle class reformers and working women. The successes and difficulties of this relationship are traced in *As Equals and As Sisters*. SPA members were important in the WTUL leadership and the women's strikes in the garment and textile sectors between 1909 and 1913 tested the strength of the alliance. The SPA recruited members among strike organizers and cadre and in the process began to take the problems and strengths of women workers more seriously. On the other hand, the WTUL walked a tight rope with regard to the American Federation of Labor (AFL). Anxious not to alienate the AFL, the WTUL urged organized women to affiliate with the AFL and consequently earned the skepticism of the SPA. As Dye's account points out, the WTUL began to criticize openly the AFL for its indifference to women workers thus jeopardizing an uneasy alliance. By 1912-13 with the Lawrence and Paterson strikes, the WTUL and the SPA found their limits severely tested. The conflict between the Industrial Workers of the World (IWW) and the SPA resulted in disarray in the party, in the women's committee, and in the WTUL. Depression and war spelled changed circumstances for socialist and working women; the WNC dissolved and the WTUL turned to trade-union education and eventually to protective legislation for women workers. For some members the WTUL became a channel for mobility in the labour movement.

Suffrage and sexual emancipation served to divide the SPA further. A fundamental distrust of "bourgeois feminism" permeated most of the SPA, although New York socialists took an active role in the

suffrage campaigns of the 1910s. The party leadership backed off support for birth control despite arguments that the issue would not only attract otherwise unreachable people to the movement, but also help to create a new set of cultural and moral assumptions for socialism. In fact, Buhle argues that "the New Morality drove a wedge into party orthodoxy by placing culture on a parity with economics." (269) With the official end of war, anti-radical crusades ensured a temporary end to debate on these questions and the creation of new political groups in the aftermath changed the nature of the entire debate. 1919, argues Buhle, was the turning point for American socialism, and by extension for the socialist-feminist alternative.

*Women and American Socialism* is one of the most important books published in recent American women's history. Buhle's synthesis argues a strong case for the influence of a dual heritage on the socialist movement's attitudes and actions toward women. The American reform tradition, (temperance, populism, feminism, and nationalism) shaped American socialism as much as European traditions brought by immigrants. The book is strongest in its attempt to trace the former: except for the first chapter's detailed analysis of German-American socialism and a summary of various ethnic groups' response to the "woman question" in chapter 8, the analysis of ethnic groups is less satisfactory. The unique position of Finnish-American women within the socialist movement, for example, is contrasted with that of Slavic, Jewish, and Italian women. Buhle suggests that pre-immigration experiences and strong community roots among Finnish women help to explain their numerical strength in the movement and the existence of their own press. Slavic, Jewish, and Italian women were less prominent and this Buhle attributes to the strength of old world customs and the struggle against Americanization. Single

Jewish women, for example, achieved reputations for radicalism and militancy in garment and textile trades, while the married Jewish woman was hampered by cultural expectations. The reader is left with only glimpses of gender relations in these ethnic groups and many unanswered questions.

These two books suggest areas and questions for Canadian women's history as well. Although there was no Canadian WTUL, we know far too little about the history of women in unions and the relationship between middle class women's groups and labour and radical groups. Our knowledge of immigrant women's work and politics is still meagre. While current research promises to fill in some of these gaps, we have a good deal of digging still to come before we can attempt a synthesis like *Women and American Socialism*.

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Maureen Weiner Greenwald, *Women, War, and Work. The Impact of World War I on Women Workers in the United States* (Westport, Ct.: Greenwood Press 1980).

GREENWALD'S STUDY IS DIVIDED into two excellent overview chapters and three case studies. The former surveys changing patterns in women's paid jobs since 1870, and describes the wartime management of women workers by government agencies (including women's bureaus) and private industry. The latter examines women's paid work in the railroads, the street railways, and the telephone companies. She approaches her subject from the perspectives of women's, labour, and social history. Her sources are standard for these areas (no oral history, for example); within these boundaries, her research is solid.

Her major conclusions can be briefly summarized. War brought few fundamental and lasting changes, but rather speeded up trends already in motion. Wartime demand for women's labour did not precipitate a rush of new entrants into the labour force: the 1910-20 increase in participation was 6.3 per cent (in contrast to World War II's 50 per cent increase). Most employed women merely shifted jobs. Most got better pay. What was new was their entry into some male jobs in heavy industry, transportation, and munitions manufacture and the intensified feminization of clerical work. White women replaced white men; black women replaced white women and some white boys.

Male hostility was a major problem in most mixed workplaces. Many men viewed these women as pawns in employers' attempts to intensify management control, as competitors for decently paid jobs, and as interlopers in a male preserve. Greenwald's discussion makes the meaning of the brotherhood of the working man depressingly clear. Many women workers in turn may have become hostile to unions as a result of wartime experiences. Greenwald emphasizes the effects of mechanization and deskilling on both men's and women's jobs prior to the war, which made it possible for women to step into new jobs during the war. Further, the need for female labour made it easier for employers to intensify the deskilling process. Employers got to practice on women workers the techniques of work force control they would apply after the war to male workers.

Despite the breadth of Greenwald's analysis, her focus is narrow. This is the study of a minority experience, a point which is seldom made sufficiently clear in labour history. By 1920, only about 30 per cent of women in the U.S. were paid for their work. Since it is the unpaid working lives of women that shape our entries into and departures from paid work, I would suggest that we cannot make sense

of the latter except in terms of the former. This means looking at individual as well as corporate and social gender-based power relations and appropriation of women's unpaid labour. Moreover, it is hard to tell to what extent the paid work experiences of the women in the three case studies are representative of larger patterns. Nonetheless, Greenwald's study is a fine piece of work.

The railroads are presented as the best case-study of women's new wartime opportunities. Railroad jobs were eminently desirable (especially under federal wartime regulation). Most workers got an 8 hour day, 48 hour week, good pay, grievance procedures, and a seniority system for layoffs and promotions. Women could get equal pay for equal work, a chance at non-traditional jobs, and union protection. In the first year of war, there was a 224 per cent increase in female railroad workers. Most did clerical work (72 per cent) but women also did common labouring, cleaning, and some shop work.

The ideal was equality and comradeship. The reality fell far short. Jobs were normally gender segregated and in many other respects followed prevalent social patterns. In the offices, sexual harassment was rampant, and male bosses and coworkers expressed their resentment of women as challengers to male jobs and male authority. In the yards, protective legislation eventually limited labouring jobs open to women. In railway shops, the carmen, sexist and racist almost to a man, made it hard for women to get union protection. Craftsmen blaming women for skill dilution often made it as difficult as possible for female coworkers and did their utmost to keep out or kick out women.

After the war, women were bumped off by returning veterans and squeezed out by postwar depression by austerity programmes by vindictive and threatened male local officials. Women not laid off were often harassed until they gave up and quit. Some office workers and clean-

ers kept their jobs, but labourers and shop workers virtually disappeared. The union was more likely to fight against than for women. Many women workers learned from these experiences "to think of trade unionism and sex and race discrimination synonymously." (137)

Women street railway workers were in a somewhat different situation. Greenwald concentrates on Cleveland, Detroit, and Kansas City, but draws on national data. Women had previously worked as ticket and station agents, and during the war hundreds got jobs as conductors. The companies, typically in financial crisis, were enthusiastic about women as cheap docile workers. Many male coworkers were not. In Brooklyn, New York City, Kenosha, St. Louis, and Duluth, the women hired were usually relatives of male workers, and were not looking for a permanent job. They were thus less likely to be seen as a threat. In other cases such as Cleveland and Detroit, men saw the women as trojan horses for management. Where male unions were strong, so was male resistance: the AFL's Amalgamated Association of Street and Electric Railway Employees carried out a harsh antiwoman campaign. Women defended themselves, often with the support of middle-class reformers, and in the process some became feminists themselves.

Atypically, in Kansas City the men recruited the women into the union and fought for equal pay. Greenwald cites three reasons: working-class resistance to a concerted union-busting effort; the positive impact of an active cross-class women's labour organization created by a laundress organizing drive; and the high level of working-class solidarity after a one week general strike in support of the laundresses. In all cities, pay and working conditions varied. Greenwald estimates that women earned a minimum of one-third more on the street railways than in traditional women's jobs. This benefit was transitory: women were generally squeezed out at the end of the war and

only a handful remained by the end of the 1920s.

Conditions in the telephone industry turned women workers into militants. Operators were especially hard hit by scientific management. Their every move and every word were prescribed, timed, watched, and counted; increased quotas and split shifts intensified exhaustion and strain. "Once a Bell System employee, never again a Bell System employee," was a motto. Operators stayed away, soldiered, voted with their feet; many organized and struck. Not that it did them much good. Government wartime control and removal of operators' collective bargaining rights helped company bosses to gain new power over workers, as the industry boomed, rates increased, and wages fell behind.

Greenwald notes that government labour policy varied but that antilabour administrators were not slow to take advantage of union weakness in some sectors. Thus railway workers, with a strong union, got a better deal than telephone employees, whose union lacked the muscle to buck the companies. U.S. Postmaster General Burleson became wartime phone boss. He was a key figure in the antiradical, antialien, antilabour witch hunts of the period. Burleson gave telephone company officials a free hand in a virtual reign of terror against workers.

The book raises many questions for further research. For example, did the visible minority women who had moved into white women's jobs, get bumped by returnees, or squeezed out in the same way as white women were from men's jobs? What happened to them after the war? Certainly they could not afford married nesting on what most black and ethnic men could earn. I was left feeling curious about the lives of women workers off the job — their families, friends, communities. Greenwald is quick to point out that women worked in wartime as in peacetime because they needed the money to support themselves or dependants. We

need a number of studies of specific sectors and groups — occupational, ethnic, age, and otherwise — using the life cycle and family history approaches to women's work employed by Bettina Bradbury, Gail Cuthbert Brandt, Beth Light, Alison Prentice, and Sylvia van Kirk, for example.

Many of our colleagues in Canada and elsewhere are beavering away trying to answer the kind of questions that Greenwald deals with and raises by omission. The 1980s should prove an exciting decade for scholarship in the area. Greenwald's study is a useful beginning. Certainly it deserves a place in line ahead of most of what goes under the heading of labour history.

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Stephen Meyer III, *The Five Dollar Day: Labour Management and Social Control in the Ford Motor Company, 1908-1921* (Albany: State University of New York Press 1981).

FROM ITS OPENING in early 1910, Ford's Highland Park automobile plant in suburban Detroit was the centre of a revolution in the application of high-speed machine tools, material conveyors and work management. All the efforts of the firm were sharply focussed on achieving maximum output of one standardized product, the Model T car. The plant and its organization were intensively studied, visited, and ultimately, copied by competitors all over the world.

Meyer emphasizes that he is concerned with labour history from "the top down," examining the ways in which the company responded to multiple problems of a large, alienated (and alien) workforce in a period of very dynamic industrial growth. The study is based on research in Ford Archives and materials in the Archives of Labor History at Wayne State University.

Early chapters examine the evolution of the new industrial technology, its social impact in the factory, and the problems of an increasing pace of work requiring fewer skills. As Arnold and Faurote remarked in 1915: "The Ford Motor Company has no use for experience, in the working ranks, anyway. It desires and prefers machine tool operators who have nothing to unlearn, who have no theories of correct surface speeds for metal finishing, and will simply do what they are told to do, over and over again from bell-time to bell-time." In the very rapid transition from the old methods to the new, worker productivity increased but at a rate lower than anticipated by the engineers developing the mechanized production systems.

The second part of the book concentrates on the Ford management's response to the major inefficiencies of high labour turnover and the dominance of new immigrants in the factory workforce. In 1913, Ford's labour turnover amounted to 370 per cent — the company had to hire 52,000 workers to maintain its basic labour force of 13,600 employees. By the following year, a survey showed that 71 per cent of the workers were foreign-born and the cultural heterogeneity of these newcomers was an increasing source of inefficiency. The appointment of professional personnel managers began the rationalization of labour policies to parallel the technological transformation of the plant. Ford's unique and thoroughly paternalistic five dollar day introduced in January 1914 was not "the most dramatic event in the history of wages," as once apparently claimed by the *Economist*. But by its nature as a profit-sharing scheme, it allowed the company to modify the behavior of its workers on the factory floor. Women and office workers were not, however, brought into the scheme until later, and the labour force in the extensive system of branch assembly plants (nearly a quarter of the total company employees in 1914) continued on an older scheme of wage rates. The reduction of the working

day to eight hours allowed Ford to introduce a third shift on the production line. Formation of the Sociological Department and the introduction of language schools in the factory completed the range of schemes to settle the workers into the new production routines.

The range of policies began to break down once the United States entered the war. External circumstances brought new influences to bear on the relationship of workers and management. By 1921 Ford's labour organization was little different from other big industrial employers: the carrot had been replaced by the stick. From the company's viewpoint the new labour policies of 1913-15 had enabled the transition to mass production to be fully accomplished — output had risen from 200,000 units in 1913 to nearly 750,000 four years later. Lasting benefits to the workers were very limited indeed and many of their problems were not tackled again until unionization twenty years later.

This book, which had its beginnings as a doctoral dissertation in the Comparative Labor History Program at Rutgers, blends the history of technology, labour history, and business history in the study of the labour policies of Ford at a time of very rapid growth. It provides a useful link between Nelson's *Managers and Workers* (1975) and the first two volumes of Nevins and Hill on Ford published over 25 years ago.

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David Alan Corbin, *Life, Work, and Rebellion in the Coal Fields: The Southern West Virginia Miners, 1880-1922* (Urbana: University of Illinois Press 1981).

"WHAT'S THE MATTER with those God Damn Wobblies?" asked Brant Scott,

executive board member of District 17, United Mine Workers of America, in 1921: "They sure talk a lot, but they don't do much." Engaged in an armed mobilization of some West Virginia coal miners, a rebellion which began shortly after the signing of the Treaty of Versailles and was not finally put down until well into the administration of Warren Harding, Scott had a right to wonder just what all was the fuss up in Everett or Seattle. The history of the IWW and kindred organizations includes many a colourful and bloody chapter in the annals of American labour struggles, but few of them can match that of Districts 17 and 29 (West Virginia), UMWA. It is an ironic but significant truth that the most determined and violent labour challenge to American capitalism in the twentieth century had nothing to do with the IWW, with anarchism, syndicalism, socialism, or communism. It had everything to do, in the words of a District 17 manifesto, with the defence of "Constitutional Liberty as Announced by our Founding Fathers and Written into the Organic Laws of the State and the Nation." And it was led by a rank-and-file miners' leader named Frank Keeney who, after a brief association with the Socialist Party, declared in the year 1919 that "it is the duty of the [miners] to eliminate from their organization any group that is preaching their different 'isms'."

All of this and more is reported by David Alan Corbin in *Life, Work, and Rebellion in the Coalfields*, a history of the southern West Virginia coal miners and their union from the 1880s to 1922. Corbin, a native of Charleston, suggests that until "we understand and appreciate the . . . West Virginia miners, we as social and labor historians, cannot — and will not — fully comprehend the evolution of the American labor movement." (xv) The claim is not exaggerated. Ralph Miliband, for example, has situated the problem of the coal miners as central to the larger "paradox" of the American working

class. After a tour of the United States during the 1978 strike, Miliband confessed that he could not understand militants who were ready to die for the union, "but could not bring themselves to say, give us the bloody mines." By providing a comprehensive account of unionism and what passed for industrial relations in the West Virginia fields during the decades under consideration, and probing, albeit tentatively in some areas, the "totality" — material and cultural — of the context of these events, Corbin has added significantly to our understanding of both the miners, and the American working class.

Corbin's book is refreshing, in part, because of the author's great sensitivity, realism, and abiding faith in the reason and humanity of his subjects. He insists upon appreciating the experience of the miners on their own terms, rather than measuring it by the yardstick of an abstract "class consciousness." Class struggle, and, argues Corbin, "class culture," were very real in southern West Virginia. But the making of the mining community did not produce workers' movements which expressed themselves in the conventional (that is, British and Continental) forms. Most crucially the "miners did not find in socialism," with which they became acquainted during the first of the mass strikes in the field, in 1912-1913, confirmation of "the broad principles that they espoused." (101)

The grisly history of industrial feudalism in southern West Virginia is probably vaguely familiar to most social historians; the most important insights Corbin offers lie in the realm of the culture, traditions, and ideas of the common people of the coal camps and company towns in this vast industrial enclave. (Well over a 100,000 mine workers toiled in the coal counties of the State). Corbin effectively demolishes some cherished nostrums of American "fakelore" in the process. The Appalachian miners' peculiar patterns of thought and behaviour did not derive from traditional mountain "gun



tot'n," "moonshin'n," and "feud'n" mores. The old clan systems were thoroughly destroyed by late nineteenth-century industrialism. Moreover, the mining proletariat which came into being during the 1880s and 1890s was the product of massive immigration from widely scattered points and therefore, could not have been solely the product of the proletarianization of the mountain people.

A substantial number of mine workers in West Virginia were in fact "southern and eastern Europeans," a fact which begs a number of questions not adequately addressed by the writer. (One of the few irksome features of the book is its use of this hackneyed and over-generalized category, and consequent failure to say very much about the unique cultural experience of the Croat or Friulan who planted roots in a place like Mingo County). Many more were blacks; here the discussion is considerably more fruitful.

Driven northwards into the coalfields by Jim Crow, as well as southwards by its Northern corollaries, black workers initially welcomed their relatively less severe treatment at the hands of the mine owners. This was one of several factors which militated against labour protest in the late nineteenth and early twentieth centuries. However, the underlying cause — the fact that mine owners exploited their workers without regard to race, creed or colour — eventually gave rise to the phenomenon described in Corbin's chapter heading as "Class over Caste: Interracial Solidarity in the Company Town." Black militancy, based on the equation workers came to make between plantation and wage slavery, was fearsome to the mine owners, increasingly haunted as they were by the spectre of the "nigger with a high powered rifle." But that symbol was also a potent symbol of *worker* rebellion. The United Mine Workers in West Virginia (as in many other locales) was a fully integrated organization, an organization of the working class.

Corbin will no doubt be scourged in some quarters as a romantic for his perhaps overly sanguine interpretation of the co-operation between black and white unionists. His assertion of a "working-class culture" which transcended the colour line will not satisfy critics looking for "hard" data (say, on racial inter-marriage) to back up the case. Those critics will be, no doubt, people who never worked in a coal mine or faced an operator gun-thug. In the last analysis, I would suggest, it was the functional solidarity of the mine workers in the union battles which mattered to Capital. But in exploring one realm of miners' experience on the cultural level, Corbin does present persuasive evidence to support his thesis.

Corbin's treatment of the importance of the shared Christian values and beliefs of the diverse human material of the coal-mine proletariat contrasts highly favourably with the often indifferent and schematic interpretations of church and sect in the *working-class world found in North American studies* which draw heavily (as does Corbin) upon the insights of that brilliant secularist, Edward Thompson. Considering then rejecting, the Thompsonian metaphor of "psychic masturbation," Corbin states that the "ideal of Christian perfectionism informed and sanctioned the class struggle" in the American coalfields. (165) Indeed, his most provocative claim is that the class struggle itself produced an autonomous "miner's religion," based on a working-class reading of the Old and New Testaments. Only the power and control exercised by mine owners over the communities prevented, he suggests, a radical sect from emerging from this context.

Miners' rhetoric reflected a rich vein of inspiration workers mined from the Bible ("Go now ye rich men . . . your gold and silver is cankered and the rust of them shall eat your flesh as if it were fire" — from the Gospel according to St. James) and an evident thread of millenarian zeal:

"God spoke to me... and said if Socialism were adopted by all the world, everybody would go to heaven on horses of fire and chariots just like Enoch." (164) (Corbin insists that this injunction of socialism was atypical, but perhaps the line was not nearly so fine as he suggests). In both idiom and origin this was a religion of class, "For Christ has a union in heaven/How beautiful union must be." The peculiar institution (one of many in the coalfields) of the "company church" had first obliterated old sectarian (and racial) lines of worship, then set in motion the process of the development of a popular Christianity. "Company churches" were basically non-denominational: for sound financial reasons. Thus, the typical mobile collier "belonged" to eight or ten ostensibly sectarian congregations. After the turn of the century, however, with the coming of strikes and confrontation, the workers became effectively estranged from all of them. The churches were used as bull-pens or ammunition dumps by the notorious Baldwin-Felts; the companies also padlocked them during labour troubles. The pliant clerical functionaries of the coal operators acquiesced in these outrages, and thereby lost their flocks to the miner-preachers.

The most telling feature of the miner's religion in West Virginia was that it sanctioned not merely class conflict, but armed struggle on the part of the workers, beginning with the Paint Creek-Cabin Creek strike of 1912-13. Armed struggle in, for example, the British coalfields, was confined to small groups of conspirators in Wales during the Chartist period. It is very difficult for this observer at least to suggest that the armed worker was somehow less "class conscious" by the virtue of the fact that he was indifferent towards the religion of socialism.

During World War I, which Corbin calls, from the coal miners' viewpoint, "A War for Democracy," the working class did find a political platform upon

which to stand, and an ideology which the author (forced to find an "ism" despite Brant Scott and Frank Keeney) describes as "Americanism." "Americanism" was based on popular notions of traditional constitutional rights (most of which were denied the miners), but also "contained a powerful strain of radical, political ideology." (177) The mine owners themselves increasingly recognized this fact, and towards the end of the conflict lobbied Washington for a cessation of Wilsonian propaganda. (190) (Notwithstanding his focus on the workers, Corbin had read deeply into the minds and documents of the mine owners, which, he correctly says, contain "startling insights.") Miners sat on their demands during the war period and loyally supported the government, but after the Armistice, District 17 and 29 became a battlefield. Hundreds died in the tragically unsuccessful attempt to bring democracy home to the darkest underside of American capitalism.

In sum, Corbin has painted a bold portrait of life, labour, and rebellion in the coalfields, and presented a compelling analysis of indigenous American labour radicalism. It is true that the miners would not, in the end, fight their President, Warren G. Harding. The question of how many divisions the President had at his disposal, however (including fighter planes and poison gas), seems to have loomed as large in their thinking as any other factor. As in England or western Canada, the coal miners in West Virginia were always practical men.

We need to know more about the culture and traditions of the Appalachian mine workers. Corbin, regrettably, draws no broad conclusions from his study. As the book's dust-jacket indicates, however, Corbin is working on a sequel, and will perhaps there provide us with his conclusions. Readers of the journal would do well to pick up a copy of *Life, Work and Rebellion in the Coalfields, 1880-1922* and hope that Corbin continues to be as seri-

ous about his project as his subjects were about theirs.

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Joan D. Hedrick, *Solitary Comrade: Jack London and His Work* (University of North Carolina Press, 1982).

TO HIS CONTEMPORARIES Jack London appeared the typical American success story. A son of the working class, he decided early in life to rise, and, through talent and determination, he made it. He was one of the most financially successful writers in late-nineteenth and early twentieth-century America, and he was also one of the best, bequeathing a rich literary legacy comprising classics such as *The Sea-Wolf* and *The Call of the Wild*. But, in accordance with another grand American theme, success did not make Jack London happy. He ended life at age 40 at the bottom of a bottle, wondering whether life was anything more than a cruel joke.

To explain the seeming contradiction and its larger historical import is the mission of Joan D. Hedrick's *Solitary Comrade*. Hedrick sees London's life and art as a futile quest for a satisfying identity and an authentic sense of belonging. Born illegitimately, scratching for a living around the Oakland waterfront, shoveling coal in a power plant, rambling about the country as a hobo, shipping out in the fore-castle of a merchantman, and doing time for vagrancy in a county workhouse, London got a full taste of life on the underside of the American industrial miracle, providing him with impressions and insights that would later surface in his fiction. Yet he remained a pensive loner, more at home with books than fellow workers, and, beyond the chemical camaraderie of the saloon, never acquired a psychic home in the "man's world" of the working class.

Convinced that the realization of true manhood was impossible amidst the oppression and degradation that were inevitably the worker's lot and obsessed with a fear of sliding into the "Pit" of the underclass, London resolved to enter the cultured, ethereal realm of the bourgeoisie that he had read about in novels. This he would accomplish by becoming a literary artist and appealing to the developed intellects and aesthetic sensibilities he believed to be the *sine qua non* of polite society. In pursuit of this goal London consciously remade himself, reading voraciously, painfully learning "proper" speech and manners, cultivating "respectable" patrons, and burying his former identity. With the publication of London's Alaska stories literary lionhood was his. His arrival was confirmed by winning the hand of Bess Maddern, the ideal middle-class woman — to him the very personification of bourgeois culture.

It was not long, however, before London began to appreciate that the purchase price for upward mobility was soul-destroying repression of emotion and spontaneity and that the putative cultural superiority of the middle class was a sham masking base acquisitive drives. This awareness induced what London later described as his "long sickness," a period of profound, near-suicidal gloom about the prospects of modern man for self-realization and true comradeship. Both the vision of human happiness and its unattainability were expressed in *The Call of the Wild*, written during the "long sickness;" the novel's canine hero only retrieves internal peace and natural fraternity through reversion to a primeval state.

London did emerge from his depression and reaffirm his commitment to life, but he did so by deliberately embracing illusions, and when they failed to keep hellish reality at bay, by resort to "John Barleycorn" in the liquor cabinet. He was sustained for the remainder of his brief life by a professed faith in sentimental love and blissful domesticity, reflected in

his second marriage to a paragon of Victorian womanhood, Charmian Kittredge, and by an incongruous, abstract belief in "the people." This survival strategy worked for a time: he now did some of his best writing, including his enduring socialist fiction. But the corrosive effects of conscious self-deception and alcohol ultimately took their toll on both his art and his well-being. Jack London died unredeemed and unfulfilled.

The author's rendering of his story yields a rich, complex, subtle portrait. Hers is a distinguished study, thoroughly researched, convincingly argued, nicely written. It is informed, too, by a sophisticated socialist perspective from which London's inner turmoil is viewed as a function of the society in which he lived. To understand her subject, she writes, "is to understand a man who struggled to be human in a society that divided feeling from thought, muscle from brain, masculine from feminine, worker from artist." Such a vantage point enables the reader to comprehend the emotional consequences of capitalism at the level of the individual. Hedrick shows that a biography exploring the human interior need not be disparaging to the subject nor a sterile exercise in historical psychopathology.

If *Solitary Comrade* has a flaw, it is a certain interpretive incompleteness, owing perhaps to Hedrick's apparent conception of the study as something less than a full biography. She declines to examine "the host of bad books London wrote in his final years," because they are "only decadent extensions of patterns already established." But bald assertion does not satisfy; one expects demonstration. Virtually no attention is paid, either, to London's pronounced racism. Certainly, the man who intoned against the Yellow Peril in his dispatches from the Russo-Japanese War and who added his voice to the general lynch-mob howl for a "white hope" to dethrone the first black heavy weight champion, Jack Johnson,

should figure somewhere in Hedrick's scheme. And the matter of London's homosexual tendencies is treated far too briefly and only in passing, a curious lacuna in light of Hedrick's thematic emphasis on his confusions about women. Finally, her analysis of London's socialist fiction and the reasons for its appearance after his illusion-borne recovery from the "long sickness" is insufficiently developed. Hedrick's exegesis of *The Iron Heel*, the most ambitious of London's politically inspired works, is confined to the book's "sexual politics." And the paradox of socialist commitment from a standpoint of bourgeois conventionality is merely stated, not plumbed.

Nonetheless, *Solitary Comrade* is a major contribution and will be read with profit by anyone interested in Jack London and what his life reveals about the societal crucible that formed him.

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Philip Taft, *Organizing Dixie: Alabama Workers in the Industrial Era*. Revised and edited by Gary M. Fink (Westport Conn.: Greenwood Press 1981).

Edited by Merl E. Reed, Leslie S. Hough, and Gary M. Fink, eds. *Southern Workers and their Unions, 1880-1975: Selected Papers, The Second Southern Labor History Conference, 1978*. (Westport, Conn.: Greenwood Press 1981).

GARY M. FINK, the editor of both of these volumes, states that the death of Philip Taft in 1975 constituted a "significant watershed in American labor historiography," with the old labour history of the "pragmatic" Wisconsin school on one side, and the new labour history with its "Marxist underpinnings" on the other. (xiii) The posthumous publication of Taft's last work, therefore, simultaneously with that a volume of articles by

young historians, both dealing with the South, seems a good opportunity to consider Taft's role in labour history and to evaluate this idea of a watershed. Contrary to what has been argued recently, I would question whether Taft really was the "embodiment of the Wisconsin school,"<sup>1</sup> and, judging by the work of the young historians presented here, it seems to me that one can exaggerate the contrast between the old and the new labour history. Rather, I would argue that Taft in some ways stands outside the Wisconsin tradition, while these new historians share some of the attitudes of its founders.

Consider the record of Taft's predecessors at Wisconsin. Unlike the rather complacent pro-capitalist image that Taft later endowed them with, the early members of the Wisconsin school shared many of the assumptions of social democrats in other industrialized countries. (Not surprisingly, some of their foreign graduate students became prominent social democrats in their own countries).<sup>2</sup> The early career of John R. Commons, the founder of the school, would have made a ripe candidate for a social democratic future. Not only was he a strong advocate of trade unionism, but he was identified with practically every third party movement.<sup>3</sup> True, after the turn of the century Commons, along with his friend Samuel Gompers, decided to back the National Civic Federation, a body apparently dedicated to conservative reform. This may have been unfortunate for the development of social democracy in America but considering the revolutionary attitudes of the socialists before 1900, it is understandable. Moreover, before Commons is condemned for having sold out to the capitalists of the National Civic Federa-

tion, it must be remembered that he differed from them in one important respect: he believed in a democratic and independent labour movement as the most important element in the corporatism that the Civic Federation advocated. The Civic Federation capitalists originally accepted collective bargaining but gradually dropped it. It is ironic that United States Steel Corporation, which Commons trusted, remained one of the chief bulwarks against organized labour in the 1930s. Commons later admitted he had been deceived.<sup>4</sup>

Organized labour enjoyed a temporary success during the war, but soon sank into the doldrums of the 1920s. The Wisconsin school shared this gloom. Commons, along with the AFL and the now purely social democratic Socialist Party, supported the La Follette campaign in 1924, which might have resulted in a new social democratic movement.<sup>5</sup> For the rest of the decade Commons and his students fought for social legislation through purely voluntary bodies, like the American Association for Labor Legislation, but with no political base they achieved virtually nothing.

It was in the midst of this period that Selig Perlman, Commons' student and Philip Taft's mentor, produced his famous *Theory of the Labor Movement*. This work, more often referred to than read, is many things. For the most part it is a rather gloomy diagnosis of labour movements, centering on the idea that their chief purpose is nothing more than job control — the concern of a working class psyche obsessed with a sense of scarcity. The result, as Perlman shows, is that unionists will always have an uneasy relationship with political movements. Nevertheless, the historical sections of the book on Germany and Britain show that the chief tensions are caused by "intellec-

<sup>1</sup> David Brody, "Philip Taft: Labor Scholar," *Labor History*, 19 (1978), 9.

<sup>2</sup> For instance, Hilary Marquand, Mark Somerhausen, André Philip.

<sup>3</sup> J.R. Commons, *Myself* (New York 1934), 81.

<sup>4</sup> *Ibid.*, 88.

<sup>5</sup> L.G. Harter, *John R. Commons: His Assault on Laissez-faire* (Corvallis, Oregon 1962), 66.

tuals" who want to endow the unions with some other purpose like revolutionism, or producers' cooperativism, or even industrial efficiency. As far as trade-union-oriented social democracy is concerned, he writes sympathetically. Its absence in America is largely explained not by some peculiar American psyche of plenty, but by the legal and political constraints militating against it.<sup>6</sup> It is in his conclusion, however, that Perlman is most interesting. What he recommends is a system of bipartisan management of industry by unions and capital. The social democrats of post-war Europe could not have put it better.<sup>7</sup>

In 1928, the year Perlman's *Theory* was published, Philip Taft entered the Wisconsin school. He started off very close to the position of Perlman, with whom he collaborated on the fourth volume of Commons' *History of Labor*. This study of labour movements in the first three decades of the twentieth century takes a very wide approach to the subject. On the subject of politics it is scathing against revolutionaries, but is not at all unsympathetic towards the Socialist Party. But if Taft ever shared the pessimism of his mentor, he soon abandoned it. Instead, he witnessed the Roosevelt revolution in labour legislation and was quick to appreciate the significance of the mass organization that it permitted. Taft devoted the rest of his working life to the study and encouragement of this system.

Whether he was justified in the confidence he expressed in modern American organized labour must remain a moot point, but certainly Taft never seems to have permitted the slightest doubt to cloud his vision. His later works increasingly lauded the American labour movement and stressed its sufficiency in dealing with American problems. Not for nothing was Taft sometimes referred to as the "court historian of the American Federation of

Labor." If anything, his focus narrowed. He read the "bread and butter" philosophy of recent unionism back into the early nineteenth century and thus specifically repudiated some of the ideas of Perlman.<sup>8</sup> His emphasis on institutions he carried to extremes: as a publisher's reader he could see no point even in David Brody's seminal work on the steelworkers in the "non-union era."<sup>9</sup>

*Organizing Dixie: Alabama Workers in the Industrial Era*, is the epitome of Taft's later work. The early chapters take us from the first city organizations, through the Knights of Labor, to the foundation of a permanent movement in the Birmingham Trades Council, and also trace a parallel movement in the Alabama coalfields. Henceforth, the Alabama State Federation of Labor, founded in 1901, provides the structure on which Taft builds his story. Inevitably, there were advances during World War I, a retreat in the 1920s, stirrings during the depression, and new opportunities in the New Deal. As one would expect, there is the split in the labour movement and the rise of the Alabama Industrial Council, which remained in rivalry with the State Federation through World War II. The last chapter is on the Alabama Labor Council, AFL-CIO. In all, there are no surprises: it is a satisfactory record of organization and that is about all. Taft is aware that large parts of the labour force are unorganized and brutally exploited, but since they have no clout there is little point in discussing them. Eventual organization is the sole solution. He notes rank-and-file stirrings in the 1930s, but these are futile until brought under orthodox union processes.

Apart from organization, what the book stresses most — and this probably gives it its greatest interest — is the race question. Scarcely a page goes by without

<sup>6</sup> Selig Perlman, *A Theory of the Labor Movement* (New York 1928), esp. 155-76, 201.

<sup>7</sup> *Ibid.*, Chapter IX, and 233, 277.

<sup>8</sup> Taft, "On the Origins of Business Unionism," *Industrial and Labor Relations Review*, (1963), 20-38.

<sup>9</sup> Brody, "Philip Taft," 22.

a mention of race and Taft is constantly aware of the advantage given to opponents of unions by the race issue. He is also at pains to point out examples of racial cooperation. The Knights were exemplary here, and, surprisingly, two vice presidents of the original State Federation were black, as Taft previously revealed in a *Labor History* article in 1975. Nevertheless, it is an uphill struggle. And yet — at least in the final years — there are signs of improvement. In fact, this is altogether a most optimistic book. Though Dixie is probably its ultimate testing ground, Taft has faith in the labour movement. There may be no ultimate salvation, but the path is ever upward.

Some of the contributors to *Southern Workers and Their Unions, 1880-1975* undoubtedly share many of Taft's attitudes. For instance, Robert S. McElvaine's study of Claude Ramsay, a Mississippi labour organizer during the civil rights struggles in the 1960s, reveals a man with advanced views accepting the constraints that history and society had imposed upon him, and, through skillful manoeuvring rather than headlong confrontation, working for an integrated labour movement. In a similar vein, George W. Hopkins traces the contribution of southern miners to Arnold Miller's reform movement within the United Mine Workers. It must be admitted, however, that at least one commentator — himself a miner — did not seem entirely convinced by the progress made. Maurice B. Better, meanwhile, has a piece on organizing the unskilled in both private and public employment. It is a Taft-like "industrial relations" job that explores factors promoting and discouraging unions.

A somewhat different piece is Lorin Lee Cary's study of "middle echelon" labour leaders in the union-building process. The reader might expect a study of how bureaucratization of middle echelon leaders stifled incipient rank-and-file radicalism, as has been the theme of other writers, but this is not the thrust of Cary's

article. Like Taft, he argues that rank-and-file militancy was sporadic and unchanneled, and likely to be interspersed with long periods of apathy. Nothing could be achieved without the stable union. Similarly, Mark V. Wetherington's study of a general strike among black labourers that brought the economy of Savannah, Georgia, to a halt in 1891, is well within the Taft range. It failed, of course, as a result of racist tactics. Presumably only time and organization could cure the problem.

Two articles deal with communism. William H. Cobb's study of Commonwealth College shows a southern educational institution, dedicated to the working class, destroying itself through political dissension. The author concludes that the dissension was unnecessary, but one commentator disagreed: communists destroyed it. While it is true that the subject of a labour college of this kind would not have interested Taft, he would have endorsed the anti-communism of the commentator. Meanwhile, Robert P. Ingalls' description of the 1931 Tampa Cigar Workers' Strike, conducted by the communist Tobacco Workers Industrial Union would confirm Taft's views of the futility of communist based unionism. The author's point, however, is that the strike was killed by employer-led vigilante activity.

The two remaining essays seem to strike a different note, and contrast sharply with Taft's assumptions. One is Joseph N. Newman's poignant study of Mary C. Baker, a founding member of the Atlanta Teachers' union, and president of the American Federation of Teachers from 1925 to 1931. It seems that this woman's professional life was dedicated to the quest for a unionism with a degree of professionalism and with a sense of responsibility in social issues. Instead, the union achieved a minimum of bread-and-butter benefits. Taft would have considered this a victory, but the subject of the essay saw her life's work as a failure. The final

essay, like Taft's work, concerns labour in Alabama, but it provides a stark contrast. Gary Kulik's "Black Workers and Technological Change in the Birmingham Iron Industry 1881-1931," shows that, whereas the Northern steel industry had replaced heavy manual work with new machinery, Alabama mills found it more profitable to pursue the old methods using the plentiful supply of cheap black labour. Ironically, some of the new technology in the north had been invented in Alabama, but it was not used until the 1920s, when black labour voted with its feet and moved north. Needless to say, the fate of these workers is unknown. Kulik's study, incidentally, is the only one to reveal any "Marxist" underpinnings by discussing working-class culture. He attempts to show that blacks could impose their own work patterns, inherited from slave gangs, upon the work process. Though the latter point is a little forced, Kulik's study of how unorganized workers affected the course of industrial development is something that is entirely missing in Taft's institutional approach.

To sum up, it must be admitted that though their interests are much broader than Taft's, these newer historians share a good deal of his methodology. With the exception of the last mentioned article, there is little one could call Marxist in the writings of these new historians. But a contrast is present nevertheless. It is a contrast between the complacency that pervades Taft's work and the sense of dissatisfaction in the other studies — a dissatisfaction more reminiscent of the old Wisconsinites. I have attempted to argue that Commons and Perlman represented an American social democracy constantly frustrated through lack of political base. The writings of the new historians reflect this same frustration. In the study of individuals like Mary Baker, in the early history of the Commonwealth Labor College, in the occasional movements of cooperation among the races, one sees the substance of a social democratic move-

ment; in the constant denial of basic labour rights, in a kind of unionism among miners where the leader refused to take up an issue like black lung disease, as was the case with John L. Lewis' dictatorial successors, one sees the need for it. What an irony there is in the fact that the Southern labour movement was obliged to work with the party of racial bigots and social reactionaries.

Do the later writings of Philip Taft really constitute the culmination of the Wisconsin school? In many ways, of course, there is a continuity. But Taft's institutionalism was rigid and almost doctrinaire; in this respect he almost rivals the doctrinaire Marxists. The Founder of the school and his immediate followers were, above all, probes and questioners, dissatisfied with the system. It should be remembered that they did not only explore the socio-political history of America, but also devoted their considerable energies to trying to change many of America's institutions. They clearly relished America's promise, but they fully appreciated its pitfalls and deceptions for the labouring populations. Perhaps there is something of this spirit to be found in the work of these newer labour historians.

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John Bodnar, Roger Simon, and Michael P. Weber, *Lives of Their Own: Blacks, Italians, and Poles in Pittsburgh, 1900-1960* (Urbana: University of Illinois Press 1982).

SOME BOOKS ARE MORE SIGNIFICANT for their ambitions than for their accomplishments. Though its specific findings are substantial and important, *Lives of Their Own* is a good example. A comparative study of the shaping of black, Italian, and Polish immigrant life in twentieth-century Pittsburgh, it both proceeds from



and struggles to transcend the limits of a decade of ferment in social-historical urban studies. As such, even its disappointments seem a gauge of the formidable task the authors have set for themselves — to move beyond what they see as “the cultural determinism of immigrant historians, the class determinism of labor historians, and the structural determinism of urban historians.” (9) in order to arrive at what they call an “interactional framework.”

By this term they mean, informally, the way city, class, and culture combined for each group to produce distinctive patterns of life. The authors want to show that adjustment to the city was a varied process, but that the variations emerged in historically explicable ways, both inducible from concrete data and deducible from the careful study of broader themes in cultural history. The investigation also has a more implicit but more particular agenda: to explain usefully the twentieth-century divergence of black and immigrant experience without falling into the twin traps of invidious cultural comparison on the one hand, or the denial of cultural particularity on the other, an approach that would see all variation as attributable to external forces such as racism. Instead, the authors want to understand the more complex ways in which both black and immigrant communities, responding to both shared and unique challenges in a variety of dimensions, shaped the contours of their own experience.

The book's chapters fall into two major sections. The first and most substantial is based on an intensive analysis of individual samples drawn from the 1900 census, and traced longitudinally in time through city directories and other sources. These lives are then aggregated to reveal family, sub-group, and group patterns of experience. After setting the Pittsburgh context and describing in rich detail the cultural and behavioural influence of each group's particular circum-

stances before and during migration, the authors carefully examine variation in the process of finding work and establishing neighbourhoods, in family composition and strategies regarding work and education, in career and inter-generational occupational mobility, and in patterns of home-ownership — this last based on some very imaginative and laborious research in deeds and mortgage histories. The last few chapters follow all of these themes through to 1960, based on the close study of a number of carefully constructed neighbourhood profiles and individual tracings, exercises that compensate in spirit if not quite in precision for the impossibility, given available data, of repeating the earlier procedures for the 1930-60 years.

All this adds up to an investigation unique in several respects among the urban community studies of recent years. The focus on twentieth-century experience is a first and most obvious virtue. Second, the multiple dimensions studied — and the way they are shown to have influenced and shaped each other — provides a far more informed and credible base for thinking about culture, structure, change, and response than have the more familiar topically-focused community studies. Finally, the catholic approach to methodology — both innovative sources and refreshing combinations of more familiar ones, such as the use of oral history to ground culturally the analysis of mobility data — helps the authors avoid the fate of so many recent studies, where the questions to be asked tend to be restricted to those that a narrow data base is prepared to answer.

Generally, the results reward the authors for their considerable labours, and readers for the happily unformidable journey through some very clearly presented data and narrative. The book demonstrates that each group's experience requires, for minimal explication, a grasp of its pre-migration heritage, of the particular conditions and opportunities encountered and

how they were particularly engaged, of the structure of the city and its neighborhoods, physically and socially, and of the larger institutional and social context of "adjustment." More concretely, they show that there was, in the early twentieth century, enormous diversity between and within black and immigrant groups, but that this variation was neither obvious in cultural meaning nor predictable in social effect; observed phenomena could be, to various degrees, cultural expressions, adjustment strategies, or indicators of inequality or constraint. More generally, they are shown to be elusive if fascinating precisely for their combination of all three.

By the mid twentieth century, however, black and immigrant groups had each become relatively more homogeneous, and their experiences more distant from each other. Blacks, increasingly, far from being simply another ethnic group — the "last of the immigrants" — found themselves encased within a unique, hardening structure of oppression and inequality. Even here, the story has an intriguing complexity, as the authors demonstrate how patterns originating as adjustment strategies settled into cultural styles that then became liabilities when distorted by the weight of contemporary oppression. For instance, it is shown that from early on, Pittsburgh's blacks evolved perhaps the most individualistic cultural style of all the city's migrants, as far as property and job-seeking were concerned. This emerged not out of weaknesses in family structure — these are shown to be largely mythical for the early twentieth century — but rather out of the very demonstrable difficulties blacks had translating kin networks into employment and housing, as other groups were more able to do. In recent years, the authors argue, the patterns generated by the individualistic "strategy" have themselves rendered blacks even more vulnerable than they might have been to alterations in neighbourhood and economic structure,

making even more inaccessible those resources needed for the protection of threatened families and neighbourhoods.

These are all useful and important findings. But it must be said that for all their stress on dynamics rather than static attributes or structures, for all their curiosity about interaction rather than simplistic webs of dependent and independent variables, the authors seem to shrink from generalizing, much less theorizing, about what they discover, as if it were enough to demonstrate the existence of a phenomenon rather than to understand clearly its structure and workings. As a result, the book as a whole seems something less than the sum of its parts. This is a paradox that suggests some deeper problems in both the methodological strategy and in the common-sense conceptualization propelling the study.

As for the first, sections of the book demonstrate the proposition that problems in one method cannot be eliminated simply by combining it with another: this may even complicate and magnify the limits of each. No matter how it is qualified or contextualized, for example, the analysis of occupational mobility, a central pillar of the study, remains weakened by widely recognized conceptual flaws. The authors are constrained by data and method to construct tables based on movements between gross categories of unskilled, skilled, and white collar work; from these, summary indices of group movements, and assessments of relative success and failure, are derived. But aside from the degree to which all this is an artifact of the categories — one discovery of substantial black upward mobility evaporates when the data categories are adjusted in a subsequent passage, for instance — the whole exercise assumes the dubious proposition that occupational change, *per se*, is an especially meaningful dimension, much less a primary measure used by people to understand progress, or its lack, in their lives.

Generally aware of such problems, the authors bring other methods — especially oral history — to the rescue, hoping to examine mobility not abstractly, but rather in reference to the particular goals and values of each group. But these are derived so casually that the laudable efforts seem only to muddy the waters further. A rich collection of oral histories is used well as a source of otherwise unreachable biographical information, but the authors seem to have no real sense of how to read the transcripts carefully as cultural documents: snippets of interviews are presented without real context, and frequently are taken too literally, even where they are manifestly nostalgic ("neighbours were really neighbours, then"). Contemporary remembrances are uncritically presented as evidence of past attitudes, perceptions, and values, becoming the basis for some sweeping generalizations about the comparative "levels of aspiration" among various groups before and after their migration to early twentieth century Pittsburgh.\*

Considering the book's ambitious agenda, such problems tend to make it most interesting and most frustrating at the same places. The best sustained example of this is the treatment of the black experience, a subtle and provocative interpretation on which much of the larger

argument comes to rest. In addition to exploring the origins and consequences of the abovementioned weak family-employment-property linkages so functional for other immigrants, the authors offer some similarly intriguing reflections on the role a primarily Protestant religious base played in shaping the distinctive structure and culture of the black community. It produced, they argue, a pattern of congregational fragmentation balanced by larger city-wide and even national special-purpose organizations, rather than the integrated, community-focused institutional grid characteristic of Catholic parish culture. In almost Weberian fashion, the authors suggest that Protestantism and individualism reinforced each other, resulting in a weakly focused community.

Rather than explore such speculations carefully, however, the authors tend to load onto them an interpretative burden they can not quite sustain. It is not at all necessarily clear, for example, that multiple congregations either generate or indicate a weak and fragmented community; it is even less self-evident that more individualistic job-seeking and less kin- or place-related job patterns indicate that blacks had a career-focused, white collar-targeted mobility impulse, that added up to "the highest aspirations of the three groups." (130-1) This claim is not particularly well supported by formulaic expressions, drawn from the interviews, about the goal of "getting ahead."

A gap between interpretive reach and grasp may derive, in part, from the authors' commendable effort to avoid stereotypes of either passive victimization, heroic cultural resistance, or unthinking acculturation. But the rescue is not really effected by combining these views, or even blurring the distinctions between them. When one page argues the cultural dimension, grounded in pre-migration circumstances, for the apparently lesser value placed on education by Poles than by blacks, it is not helpful to find documented on the next page a Pole's

\* That all this represents an insensitivity to the complexity of oral historical evidence seems confirmed by the unfortunate (and unexplained) decision to identify subjects only as Umberto B., Leroy W., Lidwina P., and so on. Surely there are better ways to handle pseudonymity than by denying working-class people the dignity, in a public appearance, of a full name, especially when they are being presented to an audience not used to taking their experience seriously, in a society that for so long was content to address adults in its servant underclass as if they were children. Can one imagine such a manner of "protecting" sources seeming as comfortable in an oral history of scientists, corporate executives, or diplomats? If not, it should not have been employed here.

complaints about the indifference of the Pittsburgh schools to the language problems of immigrant children, and the outright hostility encountered by non-English speaking children in school. On another point, consider the following explanations drawn from different places in the book:

The priority placed on homeownership among black migrants, in contrast, was less apparent than it was among the immigrants. Since blacks were systematically denied the opportunity to acquire property during and after slavery, ownership as a particular form of status and prestige may have had less significance for blacks than for eastern and southern European groups fighting to hold on to land that became so important for them in the latter part of the nineteenth century. The oral history interviews indicate that blacks came to Pittsburgh with a general desire to improve their status and pursue some career. Rarely was homeownership even mentioned as a specific goal. (154)

The lower rate of black home ownership resulted from a combination of job discrimination, greater independence of the children who might have contributed to savings, and perhaps more interest in career than in property acquisition. (158)

Large numbers of blacks continued to migrate to the city after European immigration had passed its peak. That fact, together with discrimination in jobs and housing, accounts for the lower aggregate rate of black ownership. Furthermore, when blacks finally did begin to purchase homes in substantial numbers, redevelopment and housing projects sometimes destroyed their neighbourhoods. (179)

All three groups, as demonstrated earlier, adopted the prevailing middle-class attitudes towards home ownership. Homes were viewed as stable elements in an uncertain world... But here, though, Pittsburgh's blacks enjoyed less success than either second generation Poles or Italians. (225)

Homeownership was a lower priority among blacks partially because they appeared to have considerably less familiarity with proprietorship in the South, but primarily because of their inability to secure stable employment and of the high degree of entrenched absentee ownership in their neighbourhoods. (265)

Note that each statement is reasonable and probably defensible, and there is a

very general sense in which they can be seen as almost consistent. But the price of that consistency is the abandonment of any serious claim to explanation, settling instead for a highly informal sense of "interaction." This amounts, in the final analysis, to an identification of factors involved, rather than a clearly worked-out understanding of the process of their interaction in place and over time.

The book's triumph and limitation then, is that it sensitizes us to an enormously complex and dynamic process, but tells us very little about how to think about it clearly as a whole. This is virtually confessed in a murky conclusion recalling Orwell's observation that the collapse of metaphor and syntax can indicate a surrender of clear thinking:

By the middle years of the twentieth century, blacks, Italians, and Poles could look back over their experience in Pittsburgh with mixed results... The expansion of industrial capitalism that drew them from disparate backgrounds molded them not into a unified working class but into a segmented mass with deep fissures running along occupational, neighbourhood, racial, and cultural lines. ["Networks of contact"] acted as prisms in which newcomers selectively filtered cross-currents emanating from their past and present to fashion what can best be described as alternative strategies of life... They acclimatized themselves to an industrial city amongst the interplay of all these forces at specific times. Unless this intermeshing of forces is appreciated, a full understanding of dissimilar paths of adjustment is not possible. (263-4)

*Lives of Their Own* succeeds at the appreciation part of the formula, but the full understanding remains elusive. The book's problems suggest two requirements for achieving it. In the first place, if a complex historical interaction is to be examined, a more precise understanding of each of its components would be useful first. It seems clear that the authors have done themselves a disservice by their cavalier, not to say presumptuous, dismissal of the "three determinisms." We read about urban structural forces and

constraints, for instance, in the absence of any clear description or analysis of urban economics and geography. Discussions of cultural change lack the sophistication and precision *certainly permitted by a recently rich literature*. And any discussion of the meaning and relevance of the concept of a working class would have been useful, but it is simply missing in the book.

There is also a sense in which the problems of the "interactional framework" lie in the way it has been defined — by what has been left out, in particular — as much as by the care with which its parts have been assembled. Here the authors pay a final price for casual assumptions and imprecise concepts. Their analysis is built on a largely implicit model in which migrant individuals, families, and groups "adjust" to the "city," or the forces of "urbanization" or even "industrial capitalism." Institutions, group life, and politics have little place in such a model, except as they enhance, limit, or qualify the nature of a more privately experienced and measured adjustment whose results are taken to be both visible and most meaningful in the course of individual careers and family life. To put the matter this way is to begin to see why it is so hard for issues of class and power to surface coherently in the analysis; there is little sense of immigrant experience as being in any serious sense social, and there is little appreciation of the city as a complex institution, itself "interactional" with many things, including the social, political, and institutional expressions of working-class communities. As a result, all the key issues and processes are defined in fundamentally private terms, with the connections to public life remaining not so much ignored as more simply inaccessible. The social history of Pittsburgh — in contrast to the not-quite-social history of some of its constituents — remains invisible.

There is a powerful irony here, in that tools exist for studying precisely such interactions between public and private,

between culture and structure. The dialectical methods of Marxist scholarship, for instance, have their greatest utility here, and indeed the very concept of dialectics offers a more profound tool, with a rich and complex philosophical literature, for even thinking about the processes of interaction so central a concern of the authors. The absence of such questions and methods — or, indeed, of any alternative yet comparably serious approaches — stands as something of a testimony to the limited stock of ideas the authors have drawn on in framing the work. For all its imagination and methodological sophistication, it proceeds in something of a theoretical vacuum. The consequence of this is that constricting and culture-bound assumptions — which an intellectually "interactional" approach would have forced the authors to confront — escape critical scrutiny, and the power of some substantial and important research is accordingly and perhaps unnecessarily diminished.

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Cletus E. Daniel, *The ACLU and the Wagner Act: An Inquiry into the Depression-Era Crisis of American Liberalism* (Ithaca: New York School of Industrial and Labor Relations, Cornell University 1980).

ONE OF THE MINOR PUZZLES of New Deal historiography is the opposition of the American Civil Liberties Union to the early New Deal labour-relations program. Cletus Daniel has read carefully the records of the ACLU, and he has provided a useful accounting of its activities during the two-year struggle that ended with the passage of the landmark National Labour Relations (Wagner) Act in 1935. This brief monograph is, among other things, a case study of the vulnerability of Ameri-

can voluntary associations to domination by their professional staffs. Until the membership finally rebelled and forced the ACLU to take a neutral stand on the Wagner bill, its executive director Roger Baldwin managed to make its policy a reflection of his own disapproval of the New Deal. He was able to manage this, in part, by allying himself with the Communist elements within the ACLU, who had their own reasons for opposing the New Deal labour program. Daniel sees a larger significance in this story, however: it is a tale, writ small, of the collapse of American liberalism in the face of the economic crisis.

That the facts will bear the burden of this thesis is problematic. One of Daniel's difficulties derives from the contradiction between his analysis of the internal politics of the ACLU — which shows Baldwin shamelessly exploiting his strategic position and the confidence reposed in him as a champion of civil liberties — and Daniel's treatment of the resulting anti-New Deal stance as reflective of the ACLU as an organized movement. It is true that Baldwin's own civil libertarian commitment was badly shaken by the economic crisis and by the temporary appeal of the Soviet Union.

If American workers, with no real liberties save to change masters or, rarely, to escape from the working class, could understand their class interests, Soviet "workers' democracy" would be their goal. And if American champions of civil liberties could all think in terms of economic freedom as the goal of their labors, they too would accept "workers' democracy" as far superior to what the capitalist world offers to any but a small minority. Yes, and they would accept — regretfully, of course — the necessity of dictatorship while the job of reorganizing society on a socialist basis is being done. (83)

But, as Baldwin's own words imply, it was quite another thing to assume his twisted vision was shared by civil libertarians in general. Then there is the question of treating the ACLU as "a virtual

synonym for liberalism." (136) The ACLU had been founded after World War I to help protect the civil liberties of the victims of the Red Scare. Perhaps a case can be made that the ACLU evolved from a single-issue organization into a body that reflected the broad spectrum of American liberal concerns. But Daniel does not make that case. He is, in fact, entirely silent about the history of the ACLU, as well as of its roots in civil libertarianism, prior to the coming of the New Deal. What is more, in spelling out the liberal response to the economic crisis, Daniel relies on the *New Republic* and on similar organs of progressive opinion. The link to the ACLU is merely asserted, never demonstrated.

It is entirely possible to account for the labour stand of the ACLU — or, more accurately perhaps, of the willingness of its membership to follow Baldwin's lead — in strictly libertarian terms. The issue that initially prompted ACLU criticism was the realization that the Progressive Miners of America, a small rival union in Illinois, would lose its representational rights to John L. Lewis' UMWA under Section 7a and NRA labour policy. That was a legitimate libertarian concern. So was the fear of curbs on the right to strike throughout the NRA period. The intrusion of the state into the labour-relations arena was, in fact, something that American civil libertarians (including those in the labour movement) had historically opposed. What the economic crisis forced on civil libertarians was the need to balance these traditional concerns against the benefits that workers would derive from public protection of the rights to organize and engage in collective bargaining. It can be argued that the balance shifted during the labour struggles of 1933-35 and then went decisively in the New Deal direction with the passage of the Wagner Act, which adequately met libertarian concerns about political constraints on the right to strike and bargain freely. Unaccountably, Daniel ignores the one book that offers an

alternative, libertarian interpretation for the ACLU shift during the mid-1930s to avid support for the Wagner Act. In *Labor and Liberty* (1966), Jerold Auerbach argues that a redefinition of civil liberties occurred in these years, partly by expanding the sources of danger to include private citizens — anti-union employers — and partly by expanding the scope of civil liberties to include economic rights. The vehicle for this transformation was Senator Robert LaFollette's Civil Liberties Investigating Committee, which was formed in the wake of the Wagner Act.

Daniel's book tells us a good deal about contemporary liberal perceptions of the early New Deal and its limitations, about the internal history of the ACLU, and especially about Roger Baldwin's personal odyssey during the 1930s. But the book cannot bear the burden that Daniel places on it. The ACLU opposition to New Deal labour policy can be better understood within the civil libertarian terms that the organization espoused than as a reflection of the crisis of American liberalism that came with the Great Depression.

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Cletus E. Daniel, *Bitter Harvest — a History of Californian Farmworkers, 1870-1941* (Ithaca, N.Y.: Cornell University Press 1981).

CALIFORNIA'S FARM WORKERS have been a subject of intensive study for several decades, by scholars (academic and other), special investigating commissions, journalists, novelists, movie producers, and others. One would expect that there would be little more to be known or written about them by now. This new study by Cletus E. Daniel, however, covering the period 1870-1941, is far more thorough and exhaustive in the

many sources it draws upon, and provides a number of new and important insights on the subject.

California, as Daniel and numerous previous writers have stressed, is unique in the degree to which its agriculture has been dominated by large-scale "factory farms" or "agro-businesses." They are a carryover to modern times of the large semi-feudal estates that were established under Spanish, and later, Mexican administration. They represent an almost complete antithesis to the diversified family farm, which has been the traditional model for agriculture on this continent. Farm workers have been the most difficult element of the labour force to organize into unions, and they have generally been denied the benefits of protective labour legislation on the assumption, based on the model of the family farm, that as "hired hands" coming from the same background as their employers, they enjoy closer personal relations with the latter and thus have greater security than workers in other industries. In California's agricultural economy, however, large and small farmers alike in each main area specialize in one or two main crops, the harvesting of which requires large numbers of migratory seasonal workers for only a few weeks each year. Their relationships with their employers are thus highly transitory and impersonal. There is vastly unequal bargaining power of farm workers with their employers, and their exemption from protective labour legislation until recently left them particularly vulnerable to exploitation. They have been generally the lowest-paid and most insecure element in the entire labour force.

These shortcomings have been enhanced in California, as Daniels and other writers have brought out, by the dominant role that disadvantaged racial or ethnic minorities have played in the farm labour scene: first the Chinese, during the latter nineteenth century, then briefly Japanese for a few years, followed by Mexicans and a minority of Filipinos and,

for a few years during the Great Depression of the 1930s, the displaced and impoverished "Okies" and "Arkies" made famous in the novels of John Steinbeck. The low pay and depressed working conditions, together with their distinct racial or cultural characteristics and relative lack of acceptance in other fields of employment, created a politically and economically powerless lower *caste* that has been, in effect, isolated from the mainstream of American society.

Such conditions have also generated, over several decades, periodic attempts to organize unions and bargain collectively with their highly organized farm employers. There were at least three such attempts on a state-wide scale prior to World War II, as described by Daniels. These were undertaken by the Industrial Workers of the World (IWW) prior to and during World War I, by the Agricultural Workers Industrial Union (AWIU) of the Communist-affiliated Trade Union Unity League during the early depression years of the 1930s, and the United Cannery, Agricultural, Packinghouse, and Allied Workers of America (UCAPAWA) of the CIO during the latter 1930s. The campaign of the Communist-led AWIU in particular led to several large and spectacular strikes, most of which were suppressed by "vigilante" organizations of farm employers, in league with local authorities, displaying ferocity and violence, with widespread injuries and loss of life, to a degree rarely encountered in other industries. The author describes these incidents in graphic detail.

Such efforts, while temporarily attracting widespread public attention and generating demands for reform, generally failed to improve the farm workers' lot in California, or to establish effective and long-lasting unions acting on their behalf. Effective organization of such a poverty-stricken, migratory, and generally deprived group as California's farm workers required expensive and long-continued subsidization and other support, such as

sympathetic strikes and continent-wide boycotts, by the established labour movement in the United States. Such support was not forthcoming until well after World War II, when a new and more successful organizational drive was undertaken under the charismatic leadership of Cesar Chavez, supported by the AFL-CIO.

The only criticism the reviewer has of this study is that at times it seems to lack a proper balance. Some aspects of the subject seem to be analyzed at great length, and roles and actions of some individuals described in great detail, out of proportion to their real importance. Particularly is this the case where the author sets out to prove some of his more surprising findings — for instance, that President Roosevelt's New Deal administration in the beginning, prior to 1935, was almost consistently hostile to unions, particularly in agriculture. Or again, Daniels brings out the fascinating power struggle between Earl Browder and Sam Darcy within the Communist Party of the United States, and the key role played by the latter in the AWIU's campaign — an aspect of the subject that has been almost entirely ignored in the literature about farm labour organizations and strikes in California during the 1930s. In so doing, however, the author appears to downplay the roles of Pat Chambers and Carolyn Decker, the two main strike leaders who were very much in the public eye at the time.

But this is a very minor shortcoming in an excellent work of research that is likely to remain the definitive study of the subject for a long time to come.

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Barbara J. Harris, *Beyond Her Sphere. Women and the Professions in American History* (Westport: Greenwood 1978); Winifrid Wandersee, *Women's Work and Family Values 1920-1940* (Cambridge: Harvard University Press 1981); and



Susan Ware, *Beyond Suffrage. Women in the New Deal* (Cambridge: Harvard University Press 1981).

NEW BOOKS IN American women's history appear almost daily, so at least it seems to the envious observer north of the border. Some do much to develop and refine hypotheses about the female experience; others of course have much less to offer. The three recent volumes under review illustrate both the richness and the limitations of much new scholarship.

Except perhaps for the newcomer to the history of American women, Harris' *Beyond Her Sphere* is the least interesting. Chapters originating as lectures to Pace University's Career Management Program for Women range from the medieval heritage of western civilization to the 1970s United States. Drawing on a wide range of secondary sources in American and European history — the volume itself has significant bibliographic value — Harris focuses above all on victimization. *Beyond Her Sphere* pulls no punches in its indictment of the misogyny of the Christian tradition, the oppressiveness of the Victorian cult of domesticity, the selfishness of male professionals, and the sexism of psychoanalytic theory. Women's experience of the male professions in the United States is employed to illustrate the larger picture of discrimination. This is not a volume, however, for those seeking a detailed treatment of professional women.

The first three chapters establish the context in which female professionals emerged. In brief, an oppressive tradition combined with new educational opportunities and the abolitionist movement in the mid-nineteenth century to create a women's rights movement. Organized feminism provided crucial support for women's entry into new and old professions. Even so, determined male opposition slowed progress. The vote brought no special benefits and the years between the Great Wars spelled disaster for women's

numbers in the so-called male professions. In contrast, World War II broadened opportunities. The postwar years, although they brought the further entry of women into the labour force, saw no change in the dominant sexist ideology. Harris explains that "the lag between cultural norms and everyday behavior was possible because the new feminine role was conceived in economic terms rather than as a commitment to new social values." Women's paid labour was justified by "inflation and rising expectation" not by any right to worthwhile employment. (156) Not surprisingly professions or careers for women suffered. In the 1960s and 1970s the "New Feminism" like its predecessor bolstered women's claim to the best jobs, that is the professions, society had to offer. The result was a massive increase in the numbers of female doctors, lawyers, dentists, and so on. For Harris the lesson is clear: "Contemporary women owe a large debt to the 'strong-minded' females of the nineteenth and early twentieth centuries. The debt should be repaid by continuing the crusade to build a society founded on sexual equality." (191) Unfortunately, this appeal to self-interest on the part of female professionals largely ignores the fact of class. Women to be sure have routinely been oppressed by men throughout history, but when class interests conflicted with feminist values women have often turned away from the powerlessness of their own sex. If the past is any guide, Harris' business-minded audience hardly offered the surest guarantee of an egalitarian future. This failure is endemic to the volume as a whole. The absence of any serious consideration of class makes this attempt at synthesis of limited use for understanding either the character of female professionals or the nature of their relationship to feminism.

The other two volumes, both monographs, are less ambitious and more satisfying. Wandersee takes up the question which now absorbs many students of

women's work: what is the connection between women's waged employment and their role, anticipated, idealized, or actual in the family? She identifies a significant shift in family values, among the middle class in particular, in interwar America. Women's work was directly affected when a "scarcity psychology" gave way to a psychology of abundance which stressed not the satisfactions of production but the pleasures of consumption. New definitions of an acceptable standard of living drew women, especially married women, increasingly into paid labour without a concomitant acknowledgement of their right to equality within the workplace. Ironically the same consumption dynamic which demanded an enlarged family income also affirmed women's primary responsibility for home and child care. A number of developments including fewer children, emphasis on the mother-child bond, and the rise of the ideal of companionate marriage helped married women accept limited job horizons and reject feminist critiques of the family.

Wandersee develops this argument in an Introduction, six chapters and an Epilogue. In Chapter One, "The Economics of Family Life," she documents rising expectations and uneven income distribution. The desirable standard of living, affordable in fact only to prosperous professionals, tantalized with the promise of mass production and frustrated with the elusiveness of goals set ever higher by the advertising industry. New possibilities for consumption focused interest on family life just at a time when overall job satisfaction was ebbing in the 1920s. Chapter Two considers "Deficit Living" as a response to the aspirations which soon came to be defined as necessities. In particular, the automobile was clung to tenaciously despite debt and inadequate food, housing and health care. The third chapter examines "Mothers and Children" as the focus of the consuming family. At its heart lay a paradox. To be

good and fulfilled mothers, women should remain at home. To remain housewives, however, was to forgo additional income. Without this a middle-class standard of living which guaranteed children the best start in life was at best uncertain. Mothers who entered the workforce not so much from dire necessity as in pursuit of this latter goal were still a minority between 1920 and 1940 but they represented, Wandersee argues, the wave of the future. Even the Depression with all its injury to economic equality did not affect in any fundamental way the growing commitment to a lifestyle based on relatively high levels of consumption. Chapter Four, the "Married Woman Worker," examines the at best ambivalent response to her presence. The feminist case against her opponents concentrated on economic need but nowhere was this precisely defined. Women's right to work as such was downplayed in face of a powerful anti-feminist backlash. The fifth chapter, "Working Women in the Great Depression," illustrates the retardation of women's economic progress, the loss of confidence among professional women and the indifferent support of the New Deal. Chapter Six returns to "Women's Place in the Home." Discrimination in waged work combined with a sense of guilt, fostered by advertisers, psychologists and anti-feminists when wives sought scarce jobs and alternative childcare, to make women prize the family. Until the 1960s and so long as home life was bolstered by the promise of continued consumption no real assault was mounted on women's restricted economic opportunities.

The Epilogue criticizes feminism's failure to take account of women's special orientation to the family. This failure is undeniable and remains the major tactical and theoretical problem confronting the modern movement but it is not a satisfying note on which to close the discussion. For some reason neither the final chapter nor this epilogue returns to the critical issue of

the standard of living. Yet Wandersee's earlier assessment of the power of a life-style goal in reaffirming women's domestic orientation while simultaneously driving them into wage labour constitutes her major contribution to the literature. Feminism like radicalism in general could not easily compete with America's promise of two cars in every garage and a home in the suburbs. Material comforts could make family life and thus women's roles a good deal more attractive. It certainly offered some compensation for inequality in the market place. One would have liked to see the Epilogue speculate not so much about feminism as on the implications of a no or low growth economy for women's attitude to the family. The growing withdrawal, even to the middle class, of America's promise of high levels of consumption will act again, one suspects, to challenge family values. Whether this will radicalize the women of Reaganite America is of course another matter.

The last volume, Susan Ware's *Beyond Suffrage: Women in the New Deal*, chooses a more conventional subject, focusing as it does on patterns of elite interaction. In contrast to both Harris and Wandersee, Ware's assessment of the depression decade for women is somewhat sanguine. Although women are absent in traditional accounts of the New Deal, some found, at least in its early years, an unprecedented opportunity for public power. This experience was not equalled until the 1960s. As Chapter One, "The Women's Network," observes, New Deal reformism "encouraged and facilitated progress for women, particularly those interested in pursuing careers in public life." (6) Serving in a variety of official agencies and departments and the Democratic Party, women like Eleanor Roosevelt, Molly Dewson, and Frances Perkins spoke for the social feminism which had won women the vote. Ware identifies 28 such outstanding individuals as part of a powerful female network centered in Washington. The second chapter

entitled "A Generational Approach" sets out a portrait of women born in the last quarter of the nineteenth century, especially the 1870s and 1880s. "The experiences of their generation — access to higher education for women, participation in the woman suffrage campaign and World War I, widening activities in the 1920s, young friendships from social work and politics — provided the foundation for their cooperation in the New Deal." (42) Chapter Three, "The Critical Year 1933-1934," outlines how such women, especially Molly Dewson in her role as Head of the Women's Division of the Democratic National Committee, supported each other for political and public service appointments. The most notable success was Frances Perkins' selection as Secretary of Labour in 1933. Although her feminism grew more muted, Ware demonstrates convincingly that the doughty secretary was a social feminist. Chapter Four on "Women and Democratic Politics" continues discussion of Dewson's efforts within the majority party. Most network women were equally partisan and contributed significantly to Democratic success in the 1936 election. The fifth chapter, "Women and Social Welfare Policy," suggests that the same group "played a crucial role in developing and implementing the New Deal's social welfare policies." The "National Recovery Administration, the 1935 Social Security Act and the 1938 Fair Labor Standard Act," together with "the relief programs for women in the Works Progress Administration and its predecessors" (87) are cited as examples of influence. For all its pull, however, the female network could not eliminate lower wage provisions for women in the NRA codes, find enough support for a permanent consumer agency nor maintain specialized relief programmes for women. In the face of widespread anti-feminism it was finally too small and too partisan a group to win advances for women as a whole. Chapter Six, "A Generation on the Wane," indicates

just how short women's moment was. Peaking by 1936, the network was largely in retreat or retirement by 1940. "Because women in the network saw themselves as exceptional, they failed to question women's second-class status in American Society. Never did they delve deeply into the causes of discrimination against women." (130) They had no answer for the mass of women trapped in deadend jobs and uncertain employments. The experience of public power ended in the 1930s. There followed no comparable influx of women into senior positions during World War II. The next generation of educated women had smaller vision, less ambition, and most importantly limited support. The true heirs of the mantle of Roosevelt, Dewson, and Perkins did not emerge until the 1970s.

Ware provides a fascinating and important study of a small group of female leaders. Talents long honed in female professions and female associations and causes found an auspicious moment in the early years of the New Deal. That moment, however, was all too brief. Indeed the limitations on even these outstanding women is as striking as their strength and successes. To be sure it is useful to set the record of the 1930s straight but the experience of the exceptional is not a sure guide to that of the majority. There is as much tragedy here as the triumph Ware is concerned to emphasize.

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Larry Ceplair and Steven Englund, *The Inquisition in Hollywood: Politics in the Film Community, 1930-1960* (New York: Anchor Press/Doubleday 1980). William Alexander, *Film on the Left: American Documentary Film from 1931 to 1942* (Princeton: Princeton University Press 1981).

IN 1952 THE LINCOLN THEATER in Port Angeles, Washington, advertised its upcoming showing of Charlie Chaplin's *Limelight*. It was not a bad time for movies in that small town. We had *A Streetcar Named Desire*, *High Noon* (whose political overtones were lost on me), Renoir's *The River*, and Olivier's *Hamlet* within a year. Some of us, still in high school and without guidance about films, were beginning to look at them in a spirit different from that in which we had trooped dutifully down each Saturday afternoon for the Three Stooges, Bugs Bunny, and Gene Autry (with the Sons of the Pioneers). We were beginning to learn and to discriminate. Thus we were looking forward to Chaplin, who had not been on screen there in our generation. When the time came, the theater, without notice or explanation, substituted another film. I telephoned the management and learned that the Lincoln (*Lincoln!*) "did not show films by Communists." I quizzed my elders about this and got three replies, first that it was right and proper, second that you could not do anything about it, and third "ssshhh." Happy days indeed.

It is the political background leading up to that kind of insanity that Ceplair and Englund explore. They begin with 1930, the advent of the talkies, because although their title suggests that they take all Hollywood as their province, they are really most interested in the politics of screenwriters, who came into their own as a separate group of employees, once talking pictures got established. The famous Ten (Alvah Bessie, Herbert Biberman, Lester Cole, Edward Dmytryk, Ring Lardner, Jr., John Howard Lawson, Albert Maltz, Sam Ornitz, Adrian Scott and Dalton Trumbo) whose politics and troubles are centerstage in this book, were all writers except Dmytryk, a director. Among the most militant and ideological of the Hollywood unions in the 1930s and 1940s was the Screen Writers' Guild, also prominent in Ceplair's and Englund's account. The book opens with a chapter on the

peculiar position of writers in the Studio system, anomalous from the point of view of romantic theories of art, which hold that the artist ought to be independent with complete control over the "work." Studio writers turned out scripts that others, as the producer saw fit, might revise without the original writer's knowledge or consent; the final movie might be far afield of the original idea. A writer might be asked to revise somebody else's script. The studio, not the artist, owned the product.

The Screen Writers' Guild, founded in 1933, tried vainly to break this system, or at least to get a bigger cut from it. Much of the leadership in the early days was Red, and therefore vocal, class-conscious, and controversial. They made sure the Guild was and remained a union. Readers of this journal will find the chapter on the Guild's internal and external politics during its heyday (133-47) the most interesting in the book. It is also very sad, for although the Guild won a few battles in its struggle with the studios, it lost the war, partly because the studios were strong and the writers weak, partly because the Guild itself was split in the ways familiar to every student of the American left.

Following the chapter on the Screen Writers' Guild is a potted history of left-wing politics in the United States, especially as it concerned politically conscious people in the film business. They cover the Popular Front, the War in Spain, the purge trials, the Pact, anti-fascism, the beginnings of the Cold War, and the witch hunt. The last half of the book covers the depressing story of the arrival of the House Committee on Un-American Activities in 1947 and the left's futile defenses against it and the political tendencies it represented. The Ten stood firm, kept silent, and went to jail. Others at first raised money, held rallies, wrote and spoke out against the thickening repression. Then came the Blacklists; many informed. Careers went down the drain,

friendships and loves fell apart. Very few stayed Red; the rout was complete.

How are we to understand all of this? There are two theories current, one from Marx, one from social psychology. Ceplair and Englund appear to subscribe to the former and argue:

The general structure of the American Anti-Communist crusade is familiar to the student of Political reaction: There is always a general staff of interlocking ecclesiastical, economic, political, and social elites which never meets but which is highly aware of its membership and which consciously confers social prestige and significance on the witch-hunt... [A]nti-communism functioned in this period... as nationalism had in other periods; naked class, economic, and political objectives were dressed in more appealing, demagogic garb. In the guise of a loyal opposition and protectors of a public interest, the elites launched their attack on liberals and labor — the un-Americans. (202-3)

In other words, a compact bourgeoisie, conscious of its interest, put the lid on a dangerous "progressive" social movement — with the Hollywood branch of the CPUSA as its vanguard — whose logic, if not its immediate intent, was to overthrow that bourgeoisie. In this view, the bourgeoisie (or "interlocking elites") knew what they were doing.

Adopting this position lands the authors in a muddle. In order for it to be true, the Ten and their colleagues have to be made into much more impressive organizers and agitators than they were. Throughout the book Ceplair and Englund try to impress us with how successful these "radicals" were, without showing what they were successful at, except at pulling some film people into the Popular Front. (Of course, measured another way, some of the Ten were quite successful — Dalton Trumbo was pulling down \$200,000 a year before 1947). True, they organized the Screen Writers' Guild into a militant trade union. Why? Ceplair and Englund keep insisting that the issue was always "control" over scripts, and that the sticking point with the studios was always

the right to strike. But it is not clear what "control" meant, because at various times it meant simply a clear screen credit for a script individually written and respectfully filmed. Furthermore, one takes it as axiomatic that no boss wants a union strong enough to stop production. So what is all the fuss about? The Screen Writers' Guild was a union; did this make its members sufficiently threatening to compel us to see the events of 1947 on as rational?

Instead I would argue that the whole godawful history — the witchhunts, CHUA, the purges — was irrational, that its sources were mainly religious, social, and nationalistic rather than economic, and that the big bourgeoisie neither initiated it nor thought much of it while it went on. Who represented the American bourgeoisie, Josephy McCarthy or Joseph L. Welch? David Caute's term "the Great Fear" is more apposite than the "interlocking elites" invoked by Ceplair and Englund. It was social pathology rather than calculation, because the left was in fact weak, and the CPUSA influential only as long as it worked for traditional liberal, or lib-lab, goals; the houndings smacked much more of scapegoating than of destroying an effective and dangerous political force. It was furthermore a *mass* movement. Perhaps "grass roots" is a better term; at least, such was my experience of it. The Gallup polls testified to its popular backing, and even pointed to a wider support, when approving of McCarthy's methods was in question, among "manual workers" than among business and professional men. Any attempt to explain the "anti-communist *crusade*" as an elite's attack on a more sensible (or "progressive") proletariat flounders in the mud of American idealism. This "crusade" divided families and groups of drinking buddies. Cimino caught the note of it in *The Deer Hunter*, especially in the scene at the dance hall. Ceplair and Englund, with their heavy-handed, glib, sporadic Marxism, have missed it altogether.

There is much else to complain about: padded and imprecise language, uncertainty about tone and audience, shifting definitions, ambivalence towards the *dramatis personae*. It is an infuriating book. To wit: language. Here is my favourite sentence:

At his best in the short story genre, [Albert] Maltz crafted compassionate, naturalistic depictions of the denizens of Bowery flophouses and slum tenements, and powerful evocations of the personal and social costs of racism, exploitation, strikebreaking, etc. (170)

Maltz was one of the Ten. I am willing to believe that he wrote good short stories. Let us leave that aside. In Ceplair and Englund's book, no one ever *writes* anything, he "crafts," or "pens," or "scripts," or "authors." Is it a virtue in an author to be "compassionate?" What is "naturalistic?" Is there a difference between a "depiction" and a story, between a "denizen" and somebody who lives there, between a "slum tenement" and non-slum tenement? I will not go on, except to say that there is a whole world of fashionable non-thought in that "etc."

A single example will do for the shaky tone and the difficulty in deciding whom this book is written for. Ceplair and Englund introduce us to the Hollywood Anti-Nazi League as follows:

In terms of securing national attention and raising money, the League was the most important of Popular Front organizations in Hollywood [1936-1939]. The brainchild of a small handful [as distinct, I suppose, from a large handful] of leading film talents — writers Dorothy Parker and Donald Ogden Stewart, director Fritz Lang, actor Fredric March, and composer Oscar Hammerstein — The League always appeared to be something of a "starstudded" affair, even while its executive staff planned activities and sent around petitions which persuaded hundreds of people, in and out of show business, to join the League and thousands of others to contribute to its mission. (104)

What readers have to be told that Parker and Stewart were writers and that the rest were who they were? How are we supposed to take the designation of the

League as a "brainchild," suggesting a sport, or a scheme, or a con game? Ceplair and Englund cannot make up their minds about whether these people were serious or not. If stars took part, then the executive staff must have worked "even while." That is, Hollywood always really worshipped fame and fortune, but, on the other hand, elements therein had some political ideas. Sometimes in this book the ideas are the real thing, the desire for stardom incidental, sometimes the other way around.

Laziness about terms and about "stars" extends to the central figures of the story — the Ten. Ceplair and Englund try hard to depict them both as heroes of the left, and amongst the world's best writers for the screen — the "finest among the best." (306-7) One is tempted to ask: who is being compared to whom? Dalton Trumbo and company to Cesare Zavattini, Sergei Eisenstein, Yasujiro Ozu, Jean Cocteau, Graham Greene, Orson Welles, Jean Renoir, Jacques Prévert, Carl Theodor Dreyer...? Ceplair and Englund are defensive about their heroes' screenwriting, and probably rightly, since the record is hardly memorable.

Similarly they are torn about the politics of the Ten and other militants of the period. On the one hand Communists in Hollywood saw the world and their own position in it more clearly than anyone else; they "raised the consciousness" there. On the other hand they were "untrustworthy," devious and manipulative; they were finally blind to the true political configurations they were up against. The ambiguities of this book are summed up in its subtitle: after showing how unalterably the screenwriters' and the studios' interests were opposed and how readily the differences can be understood in Marxist terms (the first sub-section of Chapter 1 is entitled "the rise of the screenwriter class"), the subtitle reads: "Politics in the Film Community." (My emphases.) Either it was a community

with conflicts perhaps but in the end a deep harmony of interest, or else it was not — or else it was the regime of injustice, oppression, and war to the death. I am not sure there is or was a middle ground. Ceplair and Englund certainly do not venture to map it out, nor do they suggest that their screenwriters squarely faced the dilemma.

William Alexander's book is better. It explores left-wing film outside Hollywood, the documentary cinema of Paul Strand, Leo Hurwitz, Joris Ivens, Pare Lorentz, Ben Maddow, and Ralph Steiner, among others, who singly and cooperatively produced a body of work that still stands up to viewing. Some of these filmmakers were Communists, some were not, but either way, working alone or in groups, they made their own films, wrote them, shot them, edited them, and tried to distribute them. No fat salaries here. What money there was went into keeping as many filmmakers alive as possible, and into production.

Tracing the history of the Workers' Film and Photo League, Nykino and Frontier Films from 1931 to 1942, Alexander shows how the main conflict amongst this group of moviemakers was not God vs. Mammon, or between the "moguls" (as Ceplair and Englund invariably call the studio bosses) and the yeomanry, but rather the struggle in the various groups between those who wanted to make good films and those who wanted to make correct films, that is, Art vs. Propaganda. In 1935 this dilemma drove Leo Hurwitz, Ralph Steiner, Irving Lerner, and Paul Strand out of the League to form Nykino. (Steiner: "Film makers must keep in mind that the statement 'there is not art without propaganda' is also true in the reverse: There can be no effective propaganda without good art," [53]) Nykino's success with Paul Strand's *The Wave* gave birth in 1937 to Frontier Films, a cooperative production and distribution company which solidified "all progressive film forces in the East." This organization

too suffered from the tension between the personal and the political, and eventually in 1942 succumbed to these tensions, to a chronic shortage of money, and to the War. All of this is well told, but the book as a whole suffers from inconclusiveness. Perhaps it is built into the subject matter. The efforts and organizations he describes did rather peter out, and when a later generation of filmmakers in the States began to make documentary films (Wiseman, the Maysles, Barbara Kopple) they seemed to owe less to Alexander's artists than to the tradition, carried over into television documentary, of John Grierson.

Alexander is a man of the 1960s. Not only his purposes in writing this book (official and ostensible: "to write about social and political documentary film and to bring to light a significant and neglected group of filmmakers;" real: "to experience a certain aspect of the thirties in order to work out something that had been incomplete for me in the sixties . . . to further develop my own true roots in the sixties." [ix-x]), but his categories of explanation cut deeper than Ceplair and England's chic Marxism. The recurrent words are "feeling," "experience," "sensitivity," "commitment," "hopefulness," "drives," "tensions;" virtually every change is explained in terms of emotion. Why did the Workers' Film and Photo League come into existence? "It was the human suffering of the Depression that united these men." (17) Why the split and Nykino? "Political decisions served to limit, frustrate, and deaden their [the founders of Nykino] artistic power . . . and [to deny] the necessity for growth and change that is vital to the human spirit." (65) Why the drift away from Frontier Films? "Irving Lerner . . . disliked conflict [Stalinist-Trotskyist fights] and desired more freedom in his work . . . Ben Maddow . . . was not entirely comfortable in the cooperative . . . He felt his poverty keenly." (221-2) Consider finally this Freudian or Frommian transformation of

what must at the time have been an intensely Marxist argument:

In terms of the *Native Land* experience, the difference in strain experienced by, say, Maddow and Meyers, on the one hand, and Hurwitz and Strand, on the other, is that the latter pair were more fully in touch with their own particular artistic visions as a result of being in control of the film. Consequently they felt less acutely the conflict between personal creativity and the formulation of collective political art. (225)

Everywhere Alexander denies the objective situation, and argues from people's perceptions or "feelings" about it.

This principle, however, makes him more, rather than less, reliable in his judgements about the films themselves. Film is, with music, the most emotional of the arts. Both unfold on their own terms over time, so that if you attend, you surrender the will's control over emotional rhythm. Therefore, when Ceplair and England say that *On the Waterfront* (written by Budd Schulberg and directed by Elia Kazan, both of them finks and namers of names) "glorified their hero's decision to testify against his former friends and union comrades," they are in a sense right (although one can quarrel with the description of Johnny Friendly, the union boss played by Lee J. Cobb — another "friendly witness," before HUAC, by the way — as a "comrade.") But they are also wrong-headed; they cannot dismiss the film so facilely because its emotional power transcends this reductionist description. Judging it by the political background of its makers is arguing *ad hominem*. As a critical position Alexander's remarks seem to me to be more satisfactory:

What I do find missing from Frontier's work . . . is something more difficult to define, perhaps, something close to Maddow's 'human heart of things,' a certain profound quality, a tone, an attitude, a depth of love for humanity. It is a quality we find in the films of Marcel Ophulus, a tragic sense not dissociated from the will to political commitment. Although



there is tremendous concern and commitment in the films by the *Frontier Filmmakers*, this larger feeling for the human condition is absent. (241)

The *Inquisition in Hollywood's* smart knowingness never rises to the generosity of spirit that Alexander manifests throughout his book.

Both books rely heavily on interviews and quote freely from them. I think this is a mistake. Spoken replies to an interviewer's questions rarely make good reading. They are too diffuse, redundant, and replete with words too rapidly or easily chosen. And of course both books draw on film for parts of their arguments. May I here make a plea for standards in *filmography* akin to scholarly standards in bibliography? Please tell the reader in one place the format of the film, its collaborators, its length, and, especially, where it is to be found — whether only one copy exists, or whether it is in commercial distribution, where you saw it, and what kind of condition it is in. We are after all going to have more books on the social and political meanings of cinema. I hope the authors will think a little clearer and write a little plainer than Cepclair and Englund. I hope also that they will be as sympathetic as Alexander. Perhaps they will start to answer the question that lurks behind the books under review: why is the culture of the left in the U.S. so discontinuous, so fissiparous and so fragile?

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Raphael Samuel ed., *People's History and Socialist Theory* (London: Routledge & Kegan Paul 1981); Raphael Samuel, ed., *East End Underworld: Chapters in the Life of Arthur Harding* (London: Routledge & Kegan Paul 1981); and Jerry White, *Rothschild Buildings: Life in an East End Tenement Block, 1887-1920* (London: Routledge & Kegan Paul 1981).

HISTORY WORKSHOP IS a phenomenon that has, after more than fifteen years, developed into a movement. As a coalition of worker-historians and socialist researchers, it has been concerned with the "real life experience" of the popular classes, the modes of domination employed to secure their subordination, and the theoretical problems that develop out of attempts to comprehend these processes. Equally important as this Workshop content has been the form of presentation and argument: History Workshop is an attempt to "democratize" historical practice, to break down the barriers that a recently-imposed professionalism has erected around our knowledge of the past. All of this unfolds within a series of contradictory twists and turns, incongruencies and idiosyncracies that make History Workshop, at one and the same time, an unprecedented and healthy expression of the popular revitalization of historical consciousness, and an exasperating, often frustrating, confrontation with a series of problematics in both the substance and the delivery of the Workshop's many messages.

One has to experience the Workshop first-hand — meet the people involved, perch on the edge of a pub stairway watching and listening to the chaos and coherence of exchanges of opinion and evidence, or crowd into one of the large annual meetings, where hundreds, if not thousands, compete for a piece of the floor that will allow them access to the Workshop speakers — to know what this is really all about. But in North America History Workshop is known through its publications, its journals and its books, edited by Raphael Samuel. Three recently-released volumes introduce us to the Workshop, to its attachment to the sharp detail of limited and particular studies, as well as to its range and scope.

Samuel's *People's History and Socialist Theory*, a collection of 52 short essays first presented as public talks at the 1979 History Workshop, is perhaps the

best single volume to introduce the non-initiated to the Workshop movement. The volume speaks to the occasional lapses of discrimination that appear in many Workshop publications, for some of the papers are mere bibliographic commentaries or short written versions of someone's particular existential effort to "think aloud." But, more importantly, it is also a monumental testimony to the capacity to explore divergent historical contexts in an effort to probe comparable theoretical terrain, where peasantries of Africa and Russia may meet, where old debates on the transition from feudalism to capitalism may find new meaning, or where oral traditions and feminist perspectives may recast our conceptions of what is known and knowable. Buried within this mountain of words are some truly outstanding and original formulations of considerable importance. Stuart Hall's thirteen-page "Notes on Deconstructing 'the Popular,'" for instance, is an exemplary charting of the complexities at the foundation of an understanding of popular culture. And in his forthright concluding sentences Hall speaks eloquently for all the contributors, rooting at the importance of this interpretive problem in the contemporary struggle for a more humane, socialist order:

Popular culture is one of the sites where this struggle for and against a culture of the powerful is engaged: it is also the stake to be won or lost in that struggle. It is the arena of consent and resistance. It is partly where hegemony arises, and where it is secured. It is not a sphere where socialism, a socialist culture — already fully formed — might be simply "expressed." But it is one of the places where socialism might be constituted. That is why "popular culture" matters. Otherwise, to tell you the truth, I don't give a damn about it.

To say that all those in and around the Workshop movement would agree with Hall's emphasis is not, however, to preclude debate. *People's History and Socialist Theory*, like *History Workshop*, is about debate, from the attempts to reconstruct the exchange of views that fol-

lowed many of the short papers to explicit confrontations over patriarchy's usefulness as an explanatory construct for feminists, pitting Sheila Rowbotham against Sally Alexander and Barbara Taylor. The most concerted, and obviously most disturbing, debate, however, emerged out of controversies generated from various readings of E.P. Thompson's *The Poverty of Theory*. As Samuel's note to papers presented on a panel devoted to this work (by Hall, Richard Johnson, and Thompson himself) makes clear, the Workshop, ironically (given all that it has done to encourage spirited debate), has a view of "debate" not unlike the professorial milieu of gentility and depoliticized detachment that it has done so much to distance itself from. There, too, colleagues tread only very lightly on the toes of their right-thinking peers; they reserve their venom and their spleen for the left. In the Workshop movement, one suspects, it is acceptable to assault non-socialists, but when criticism is levelled at those on the left it is to be done quietly, and without undue dramatization. This is not something I would wish to quarrel with, except that debates do not develop out of a political and intellectual vacuum; especially on the left, such debates take place within a charged atmosphere of relevance to the creation of socialism, and this was especially the case with the voices raised against *The Poverty of Theory*.

Consequently, when Edward Thompson, whose own history is cast within another, highly polemical, and passionately political, tradition of debate, declined to box himself in to the Workshop's form, the content of his refusals to accept much of the critical assault upon his dismantling of the Althusserian orthodoxy seemed to slip through the distressed fingers of the Workshop advocates, who clutched at some ideal notion of fraternity while political issues of weight were hurled at their heads. "Against the intentions of the organizers," notes Samuel, the

Hall-Johnson-Thompson panel "resolved itself into something resembling a gladiatorial combat." From another quarter came indications of the Workshop's anguish at the "personal pain and discomfort" caused by Thompson's "demolition job," delivered, apparently, "with maximum theatrical force." Debate had turned into disaster: "The result was that subsequent discussion was almost impossible. The aftermath of the Saturday night's fusillade hung like a pall of smoke over the rest of the Conference." But did the organizers of the workshop actually think that it could have been otherwise?

The catastrophe of the Thompson performance brings to the forefront the two thrusts of the Workshop movement: what it means to "democratize" history and what it means to politicize it. These far from easily resolved aspirations are also at the very heart of two other Workshop publications, Samuel's edited history of the life of an East End London criminal, Arthur Harding, and the efforts of one of the Workshop's "self-made" historians, Jerry White, to explore life in an East End Jewish tenement block in the years from 1887 to 1920.

Both books have similar strengths and weaknesses. They open up previously obscured histories of particular plebeian strata and, in copious and often remarkably exciting detail, reconstruct experiences that were little appreciated, if not blatantly misunderstood, by academic historians. This is a part of what the democratization of history is about, along with an attempt to write history outside of the confinements of careerist advance; it speaks to a concern to bring "the people" back into history, accentuated in these volumes through reliance upon oral testimony. Harding's recollections are the entire project in one volume, while in White's more conventional study a series of interviews are the core evidence around which more traditional data are marshalled. For History Workshop the people are both creators and recreators of history.

Beyond this, both volumes also take us toward an interpretive corner seldom stumbled into by other historians: an explicit attempt to conceive of experience as a complex totality, where the social, cultural, political, and economic interpenetrate. In *East End Underworld*, Harding's rise to the stature of a "king" of the criminal sub-culture of the Jago is a unique presentation of the place of family, penny capitalism, rule of law, ritual of the street, ethnicity, gender, politics, and age in the political economy of Bethnal Green. White's *Rothschild Buildings*, similarly, is unrivalled in its depiction of the totality of experience, from the acute understanding of the significance of the physical (and awesome) barracks-like severity of the structures people lived in, to the home-life, sense of community, aspects of childhood, labour processes, and political sympathies that all emerged in the shadows of the block's tenements. All of this, as well, can be read as an attempt to democratize history.

To an extent it is also to politicize it, and both books end on the notes of "politics and class." This is as it should be. But there are also problems here, for politics and class are not processes captured neatly in the experiences of a street or of a criminal slum: they necessarily extend well beyond them. This poses less of a problem in *East End Underworld* which, after all, aims only to present the life of Harding through his own words (a companion volume, situating and interpreting the political economy of crime, is promised, and on it rests the larger analytic burdens), but it intrudes directly on White's study. For here is a book premised on the belief that it is possible, if not desirable, to write the history of a street. *Rothschild Buildings* goes as far as any previous work to establish the validity of this orientation, and the History Workshop movement itself, with its commitment to local studies and micro-history, has done much to substantiate it. As an empirical exercise, there is much to be

said for this narrowing of focus. But to understand the larger unravelling of politics and class we will require a wider frame of reference and a much more theoretically-poised discussion.

To say that the three books here introduce us to that problem, sharpening our awareness of it, is to compliment the History Workshop at the same time that it is to push them and others towards a more rigorous attempt to resolve it in future studies. But more than any other contemporary development within social history, the Workshop movement, in all of its many ways of presenting itself, has brought us closer to an understanding of just what a politically-committed and democratically-developed understanding of the totality of lived experience might look like. There remain many complex processes — both in terms of form and content — that still seem stalled and stunted, caught midway between established conventions and the promise and possibility of a transformed practice. But this only means that there is that much more to look forward to from the people who are trying to work themselves out of these dilemmas, and to create a fuller and more useable past.

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John Stevenson, *Popular Disturbances in England 1700-1870* (London: Longman, 1979).

START WITH THE "Rage of Party" and the election riots of 1695, 1698, and 1705, add to these the Sacheverell riots in 1710 and the Jacobite-Hanoverian disturbances of 1715, then mention religious riots against Catholic chapels in 1745, against Quaker corn dealers, and against Methodists in the 1740s and 1750s; consider the manifold disorders against the Militia Act and its implementation, skip over enclosures and turnpike riots, and

mention quickly disturbances by or against smugglers, wreckers, and poachers; mention a few skimmingtons, a bit of witchcraft, Stamford bull-running, and the attempts to preserve rural sports; describe the mobs against Walpole's Excise Act and his Gin Act; mention how the silk-weavers and the coalheavers used to march and demonstrate in the 1760s, and how the Gordon Riots gave the ruling class a fright from which they never recovered; summarize how the tanners, colliers, boatmen, cloth-workers rioted to keep food prices down, and how labour disputes before the Combination Laws involved cloth-workers, framework-knitters, keelmen, sailors, colliers, shipwrights, and carpenters, and you've chronicled the disturbances of the eighteenth century.

Take up the chronicle again with the "threat" of revolutionary ideology and the 'Church and King' riots; describe the anti-crimp house riots (especially vivid), tell how the Queen's coach was mobbed and how Kyd Wake threw a stone at the King; talk about the LCS; mention the naval mutinies, the conspiracies of Despard and the United Irish; show that Westminster elections continued to be tumultuous and that keelmen, shipwrights and croppers continued to organize; consider Luddism; review what have become the set-pieces of postwar Radicalism — Cat Street, Pentrich Uprising, Peterloo and the Queen Caroline Affair; go on to the Reform Crisis describing the riots at Derby, Bristol, and Clerkenwell; return to industrial and agricultural protest — the rise of the unions and the "Swing" riots; then the anti-poor law disturbances, the anti-police disturbances, the Newport Rising, the Plug-Plot riots, 1848; and finally the anti-Catholic, anti-Irish, trade union, election, Reform Act, and "Hyde Park railings" disturbances at the time of the mid-Victorian equipose when, among hopeful sentences about the "orderliness" of British society, this huge and prodigious volume of riot and disorder comes to a halt.

Stevenson has concentrated his attention on the collection of innumerable "disturbances." His work relies heavily upon a great many historians: the Webbs, the Hammonds, George, Rudé, Hobsbawm, Thompson, Chambers, Beloff, Tilly, Williams, Prothero, Shelton, Hay, Peacock, Rogers, Donell, Rogers — whom the author acknowledges generously. It is an academic book. The history writers are mentioned more frequently than the history makers. The names of very few rioters or "disturbers" find their way into these pages whereas the historian who seemed to transform historiography by naming the faces in the crowd (Rudé) is mentioned nineteen times and the historian who rescued the poor stockinger from the condescension of posterity (Thompson) is mentioned seventeen times. It is surprising, therefore, to find that such a survey, aimed at the academic market, chooses to avoid the disputes and arguments that have long characterized the monographic work on the period.

To be sure, within the vast concatenation of events that the author has linked under rough geographic and chronological headings, Stevenson hazards a few closely qualified and conditional generalizations; such as, that over the period as a whole "disturbances" came to take less the form of riot and more that of strike, and that "industrial" disputes came to be less about prices and more about wages. On the whole, however, the overwhelming impression is that of unremitting gradgrindery as fact, after fact, after fact is compiled without pause for interpretation, interruptions for reflection, or discussion about their meaning. The result is not impartiality; what emerges is an evasive and one-sided empiricism.

When explanation is required Stevenson resorts to vague and question-begging phrases, like the following. The destitution of agricultural workers in the 1820s was caused by the "exogenous pressures of population growth and agricultural change." (243) But why did agricul-

ture change? It would be better to speak of low wages. At the time of the 'Wilkes & Liberty' agitation the London mob was motivated by "a complex medley of atavistic passions." (74) If the passions were reversionary, to what earlier experiences should we return in order to understand them? It would be better to write of the "free-born Englishman" and the Levellers. His account of Luddism accepts it as a "backwards-looking" response to "rapidly changing circumstances." (162) But who changed the circumstances? It would be better to write of those hard-boiled capitalist entrepreneurs who introduced the machines. Struggles in the eighteenth century mode of textile production, a theme which since Marx has caused some of the deepest and most fiery thought in historiography, is here accounted for by "the quickening pace of economic activity." (115) Who quickened it? It would be better to write of the "real domination" of the capitalist mode of production. C.L.R. James has written that "the truth of the labour movement consists only in relation to capital."<sup>1</sup> Here capital is present not as a human relationship, but in phrases of insubstantial reification which like shadows in a dream enter the scene only at moments of inexplicable tension.

Stevenson tries to avoid both the classic disputes of the period over land, machinery, and exploitation, and the more recent discussions about working class organization by adopting important and balanced appearing sentences which when examined turn out to be confused or illogical. Consider these.

1. Although often using clandestine or ad hoc organizations, many of these groups [of workers] were beginning to use strikes rather than riots to obtain improvements in living standards or halt their erosion. (229)
2. Although historians have disputed about the role played by enclosure and other changes in agriculture, growing dependence

<sup>1</sup> C.L.R. James, *Notes on Dialectics: Hegel, Marx, Lenin* (1980), 86.

on wage labour left the rural worker competing in an overcrowded labour market and vulnerable to agricultural depression, technical improvement, and high prices. (237)

3. . . . although in the short term these protests were functional in that machines were destroyed and some concessions wrung from local farmers and authorities, the 'Swing' bands were operating in a much more localised and ad hoc way than many contemporary urban workmen. (243)

Such locutions do not provide a clear glass to the author's meaning. The first makes sense only if it is assumed that clandestine or ad hoc organizations are *not* conducive to implementing a strike, but why should we assume this when experience suggests that the opposite is very often just as true. Supporting families, raising strike funds, arranging benefits, establishing communications, deploying pickets, controlling publicity generally *do* require ad hoc committees and sometimes the names of those on some of these should not be easily discovered. Organization generally follows social action; indeed, it has been argued that permanent organization may actually be a sign of the weakening of mass action.<sup>2</sup> Does Stevenson think something like this — in the eighteenth century when workers had no unions they rioted while in the 19th when they did have unions they went on strike? If so, he should state this and argue it through, though I think that the evidence will controvert it.

The second sentence cannot be what the author means, for he cannot believe that, did historians not dispute about enclosure, then the wage-labourers would be less likely to compete in the labour market or be less vulnerable to agricultural depression, etc. Historians' disputes have nothing to do with these relationships, of course. In fact, it is to be wished that Stevenson was more familiar with the disputes. Why is it assumed, for instance, that technical improvements would make

the farm labourer vulnerable, and not, on the contrary improve his or her lot? A great many believed it might. Here is a passage from *The Pioneer* (1833):<sup>3</sup>

Q. What is the effect of Machinery?

A. To do that labour which must otherwise be done by hand, and to do it more perfectly and expeditiously.

Q. To whom then ought the machinery belong?

A. To the men whose work it does — the labourers.

It is not asked that he adopt as his own this or that position in the debate about machinery, only that he at least describe the arguments that could have such dire consequences as befell the Swing rioters of whom 644 were sent to prison, 505 to Botany Bay and nineteen "to dangle in the Sheriff's picture frame." It's true — to return to another part of the sentence — that historians have disagreed about the effects of enclosure upon productivity, the labour market, the profits of the squires, and the lives of the cottagers and labourers, so it is annoying to find Stevenson recognizing the existence of these disputes here but elsewhere while considering the subject of enclosures directly he proceeds as if historians now shared a consensus of opinion, which they don't.<sup>4</sup> His own conception of enclosure is so brittle that the existence of evidence which does not conform to it, such as Goldsmith's long poem, "The Deserted Village," is considered "unfortunate."

The third sentence is similar to the first in that the logical connection between the two clauses depends on accepting an unstated general rule. Proposition One (the "Swing" protests were functional in that they wrung concessions and destroyed machines) and Proposition Two

<sup>2</sup> Maxine Berg, (ed.), *Technology and Toil in Nineteenth Century Britain* (1979), 90.

<sup>4</sup> J.M. Neeson, "Common Right and Enclosure in Eighteenth Century Northamptonshire," unpublished Ph.D. thesis, Centre for the Study of Social History, (University of Warwick 1977).

<sup>3</sup> Frances Fox Piven and Richard Cloward, *Poor People's Movements* (1978), *passim*.

("Swing" rioters operated in a localized and ad hoc way, unlike contemporary urban workmen) considered separately may have some truth to them, although the second can be accepted only with misgiving in light of what we know of the creativity and variety of organization shown by the working people of the town (think of John Forster's work). Let that be as it may. If the causal relation that Stevenson indicates between the two propositions is to be established then a third proposition is required, one saying that local or ad hoc organizations do *not* lead to functional protests. Clearly, however, that is not the case even on the evidence produced in this book. The tactical versatility and protean flexibility of the English working class brought to its relationship with the rulers of England a range of popular mobilizations (Parliamentary and extra-Parliamentary, mass or cadre, street or field), of communication (correspondence, music, "illumination," print), and of action (strike, lobby, mutiny, insurrection) that were by no means always entirely successful or "functional" but whose "ad hoc" appearance and local setting were often exactly the conditions of victory. Moreover, this experience was continually examined, questioned, and evaluated, and not only by the Home Office.

Not only is there something evasive in the manner in which Stevenson engages with historical disputes, there is a one-sidedness to the fundamental concept which selects his historical facts. The author seeks to find the authoritative voice by the use of presumed authoritative categories of analysis. Such categories, it has been well said, as often as not tend to be the categories of authority. Class conflict, insurrection, strikes and turn-outs, riot and tumult, the eighteenth-century mob and crowd, war and revolution are all assimilated into the single category of "disturbance" and in so doing the principal point which so many of these con-

flicts raised gets lost. Who shall be the authority and by what reason?

It's a question he never raises. Instead, he considers four definitions of "disturbance." First, there is that used by social scientists, "temporary but violent mass disorder." Second, there are those definitions developed by historians who, beginning with Rudé, have made something of a typology of riot according to their purpose and social composition. (Stevenson finds the substitution of the word "crowd" for "mob" — Rudé's well-known starting point — "too simplistic.") Third, there are the contemporary definitions which spread across a range of behaviour from the extravagant or noisy to the "insurrectionary." Fourth, he considers the definitions arising from eighteenth century law, within whose "authoritative" terms he is most at ease. He makes some useful distinctions as he considers the law of public order, of unlawful assembly, and of breach of the peace. Oddly, he does not consider the law of disturbance.

The tendentious quality of this discussion ought not to require extensive comment, yet on the basis of the evidence produced in this book, a reader would not know that the entire social order was being repeatedly challenged from the forces, the properties as well as the unproperties, making it up. Under such circumstances it should not be presumed, at least not without argument, that "tranquility" belonged to the ruling class and "disturbance" to the popular ones. It needs to be emphasized that not only was it widely believed that the interruption of "tranquility" — or at least a kind of negotiated stasis — came from the intrusions and initiatives of the ruling class, a class itself in the throes of transformation, not only was this believed widely, it was so. "If our oppressors shall break the peace — if our tyrants shall violate the law — if our despots shall trample upon order — then we will fall back on the Constitution, and defend the few remain-

ing of the blood-bought rights left us by our fathers," said Harney in his Derby speech of 1839. A great many English men and women thought that what was "disturbing" were the Molochs called machines, the Bastilles called workhouses, and the newly constituted police whose introduction had been successfully resisted by generations of English "commonwealthsmen" opposing anything that smacked of a standing army, but which Stevenson dubs an "administrative innovation."

Not to draw explicitly the lessons of history is one thing; not to notice that the participants drew lessons from their own history is quite another. In an outpouring of print for which one would have to return to the English Revolution to find a parallel, in newspaper, journal, pamphlet, broadside, placard, and handbill a debate about the fundamental order of society was raised throughout the country. The keywords of that time — "industry," "class," "democracy," "capitalism," "socialism," "mutualism" — were hammered out in the hot forge of debate becoming powerful tools of intellectual work then and now. Even the most cursory glance at the unstamped press shows the affinity of the written word to the spoken cadences of declamation, argument, dispute, and talks, and it is this which both dates the language in its grandiloquence and authenticates it against lived experience. Dorothy Thompson has seen in this discussion "a degree of rationality, even of optimism" not found in earlier movements. She also detects a class bitterness unparalleled in English history.<sup>3</sup> But of debate, argument, theory, or reasoning, not to mention "class," a reader will find precious little in these pages.

This is partly the result of using that term "disturbance," a word of the authorities. Partly, but not wholly,

because (as is not noticed especially in this book) "authority" itself underwent basic changes as its class composition, forms of association, and means of rule responded to the developing character of the working class. Consequently, what it meant by "disturbance" changed. There is an eighteenth-century, legal definition which Stevenson has overlooked: "disturbance is a wrong done to some incorporeal hereditament," Blackstone wrote, "by hindering or disquieting the owners in their regular, and lawful enjoyment of it." What were these "incorporeal hereditament?" Did they include outdoor relief? A just wage? No standing army? This meaning of disturbance belongs to quite another matrix of class relations than that which emerged victorious in 1870. However, to discuss how a "keyword" like this changes its meaning is to raise a whole number of issues about the relations between the classes, and hence the meaning of their forms of interaction.<sup>4</sup>

Stevenson does not do this. He has nothing to say about "living standards," nothing of wages or prices or other forms of income. Thus the material relations of circulation are ignored. With nothing to say about "concrete labour," of the labour process and its technique, the material relations of production are likewise ignored. The huge effort of the State through the War Office and the Admiralty to repress disturbance not to its liking (or create official ones which were) are totally omitted from study. The silence about Scottish, Welsh, and Irish developments has multiple effects: the relation between suppression of insurgencies in these lands and England is ignored, the relations of military affairs to civil ones is missed, and the circulation of experience among the peoples of the four places is lost.

In the middle of the book there is a map of London showing the main West

<sup>3</sup> Dorothy Thompson, *The Early Chartists* (1971), 14.

<sup>4</sup> Raymond Williams, *Keywords* (New York 1976).



End squares and growing grid of development of the West End during the Regency. East, north, and south London are left relatively grey and nameless. It's good to have the squares named, but in a book on popular disturbances one wants especially to know where the great gathering places of the population were — St. George's Fields, Spa Fields, Copenhagen Fields; these, however, are not shown. The emphasis in the prose is the same: it is on the Squares, not the Fields.

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E.H. Hunt, *British Labour History 1815-1914* (Atlantic Highlands, N.J.: Humanities Press 1981).

E.H. HUNT has written a book that will be much admired for its apparent "balanced" views, its commendable thoroughness, and (in general) its clarity of exposition. It is divided into two parts: Part I deals with "The Labour Market, Incomes and Consumption," and includes discussions of population, living standards, poverty, and migration; Part II, on "Working-Class Movements," comprises two chapters on the period between 1815 and 1850 (one is on Chartism), one on 1850 to 1888, and a final chapter on 1889 to 1914. The overall organization is sensible, neat, and orderly. And in other important respects this is an admirable and authoritative study. It is, in particular, a very substantial, well-informed piece of work. It is full of pertinent evidence, clearly and concisely presented; it is impressive in its informative coverage of major themes. Quantitative evidence is effectively and judiciously deployed. On many topics Mr. Hunt offers a reliable, up-to-date, and clear-headed analysis of the issues involved. The chapter on "Migrants, Emigrants, and Immigrants," for example, and the account of the fluctuating fortunes of trade unionism during the generation

before World War I, are exemplary synthetic treatments of their subjects. This, then, is no humdrum textbook churned out for the student market. It is probing and intelligent and has an unmistakable ring of solidity.

If these are some of Mr. Hunt's undeniable strengths — and they are no mean virtues — there are other features of his work that will not please everyone. He has, for a start, some definite sympathies in his interpretations — a marked leaning toward an optimistic assessment of the social implications of economic expansion during the years between 1790 and 1850, and pronounced sympathies in favour of movements that embraced moderate reform as against radical proposals and actions (some of whose agents are presented as "political agitators"). His handling of the standard-of-living question is, in my view, less than satisfactory, partly because he formulates the problem too narrowly — he tends to emphasize "industrialization" rather than the larger process of capitalist growth (there is no doubt that wages in expanding industrial districts were relatively buoyant); partly because he gives insufficient attention to the evidence — evidence, in particular, from local and regional studies — of stagnant or declining real wages, especially for the latter half of George III's reign; and partly because his overall representation of plebeian experiences during these years of social upheaval, and of labouring people's relationships with men of power and property, is excessively complacent and anodyne. As for his approval of moderate, pragmatic reform, this disposition is frequently evident, particularly, perhaps, when the discussion turns to questions of class consciousness and class hostility, the importance of which he is usually concerned to depreciate. His treatment of these issues is not always steady or consistent: in his discussion of "the making of class, 1815-50" (245-50), for example, there are several noticeable and rather grating shifts in emphasis, as he

veers from one position to another. But it can be said, I think, that on the whole he is much more sceptically critical of the language of class conflict than he is of the language of deference and paternalism. Moreover, he too often becomes hung up on the ill-formulated question of whether or not "a single working class" had been "made" at some particular point in time: this is not, I think, a subtle, or helpful, or culturally sophisticated approach to the complex problem of class relationships. And when he writes of the 1830s that "most workers . . . would have resented historians' attempts to steamroller them into an undifferentiated working class" (248), one can only say that these attempts are largely a construct of his imagination.

It can be said as well that, while Mr. Hunt's version of labour history is in certain respects impressively comprehensive, in the tradition of some of the best English economic historians, one is also struck by what it leaves out. His book has very little to say about leisure, or religion, or childhood experiences, or beliefs, customs, and values. Cultural themes are seldom discussed, and for the most part literary sources — memoirs, petitions, oral recollections, descriptive accounts, and the like: those sources that shed the most light on cultural issues — are drawn upon infrequently. We get little sense of the *feelings* of labouring people, and of their experiences outside work and politics. Indeed, their actual voices are rarely heard. This silence — and it is not that the sources themselves are silent — can easily be misleading. For the almost complete omission of such evidence means that we are seldom offered a perspective from *within* popular experience. Few occasions are afforded for any sort of elaborated appreciation of how ordinary labouring people, in various trades and localities, understood themselves, their circumstances, and their social positions. And we certainly get little sense of popular bitterness, or resentment, or disaffection, or

feelings of injustice. Such sentiments — sentiments like those of a body of Nottingham framework-knitters, who in 1833 complained of the principles of political economy that "they mean nothing more, than that we should live contentedly in poverty, and disgracefully in a workhouse, in order that capitalists may inhabit palaces, and live in luxury" — are strangely missing from its rendering of labour's past. It is the "hand," rather than the full person, whose presence commands the greatest attention. Others, I think, will have rather different, and more experiential, stories to tell. But even contrary thinking is likely to be guided in part, and sometimes fruitfully provoked, by Mr. Hunt's considerable achievement.

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Brian Harrison and Patricia Hollis, eds.  
*Robert Lowery: Radical and Chartist*  
(London: Europa Publications 1979).

IN LARGE PART this book is a full reprint of the autobiography of Robert Lowery which was serialized in a British temperance periodical in 1856 and 1857. Born at North Shields on Tyneside in 1809, the son of a sailor, he spent most of his youthful years at Peterhead in Scotland. He died in Woodstock, Canada in 1863 but his autobiography covers only the period up to 1841.

At age 13 Lowery became an apprentice seaman and made several voyages on Tyneside timber ships to the Gulf of St. Lawrence. On one trip he became ill and was placed in the Hotel Dieu nunnery in Quebec to recuperate. There he underwent a fundamental educational experience. Daily visits by Catholic priests and Protestant clergymen apparently aroused the intellectual curiosity of this "Geordie" lad. He acquired analytical reading skills and a knowledge of the Scriptures

which stayed with him throughout his later career.

In the years leading up to the first British reform bill, Lowery became politically active. He joined a debating club at Newcastle and made his first public speech in 1833. About the same time Lowery became a tailor and was secretary to an early trade society with its secret initiations, passwords, and locked strongboxes. For his activism he was given what was then called the "silent sack" by his employers. He also spoke out against the sentences passed on the famous Dorchester labourers, or Tolpuddle martyrs, in 1834. In spite of this, as the editors point out, he was not a "natural trade unionist." (14) There is too much in him (especially in later life) of the independent artisan and individualist for that.

In the 1830s Lowery was developing his skills as a public speaker on various popular causes. He supported the Polish insurrection against the Russian czar and the rebellions in the two Canadas. He denounced the New Poor Law, the stamp tax on newspapers, and the niggardly extension of the franchise allowed by the Reform Bill of 1832. In 1838 he was elected a Chartist delegate from Newcastle and embarked on a series of speaking tours to Cornwall, Scotland, and the north of England. His autobiography is an extremely valuable source in this regard as it provides an inside view of the Chartist convention and the uprisings of 1839 to 1840.

There is another sense in which this autobiography is useful to the student of nineteenth-century popular history. Here we have a testament of one of those articulate "uncommon common men." It contains details of his political and literary readings; his admiration for the poetry of Robert Burns, as well as the life of Benjamin Franklin, and his negative reaction to the co-operative theories of Robert Owen. There are accounts of a religious revival in St. Ives, Cornwall, a violent public meeting in Dublin, and a visit to Paris

where Lowery met the likes of the poet Lamartine, the writer de Tocqueville, and the utopian Etienne Cabet.

The editors have provided an introduction in which they deal with some problems associated with this autobiography. It was written in the 1850s and by then the author had moved on to another phase of his career to become a temperance lecturer. Many of his references to the evil effects of drink, it is noted, are really retrospective moralizations pasted onto his earlier political career. In addition there is a tendency for Lowery, the autobiographer, to play down the radicalism of Lowery, the Chartist orator. To make some compensation for this distortion, the editors have included an appendix of primary documents from Lowery's early career. These consist mainly of accounts of his speeches, his reports of his tours on behalf of the Chartists, and a hard-hitting pamphlet on the radical tactic of "exclusive dealing." These are most useful, as they allow the reader to grapple with the problems of distortion common to all autobiographies.

Throughout this work the editors have taken great pains to provide explanatory footnotes to personalities, technical terms, archaic usages, or particular historical events mentioned by Lowery. In general they have done an excellent job. They are at their best in providing cross-references to Lowery's connections with the radical, Chartist, and temperance movements. They have done well to chase down nineteenth-century nautical terms, Scottish dialect words, and proper citations for briefly-mentioned pamphlets.

There are, however, some minor problems. The editors appear to be somewhat disoriented in their Canadian geography. Lowery's reference to sailing to Picton in Canada must be a printing error. It can hardly be the case, as the editors suggest (50, n 17), that a 300-ton Tyneside timber ship could be plying Lake Ontario before (or even after) the completion of the Lachine canal. This is probably a refer-

ence to Pictou, Nova Scotia, especially as the next paragraph has the vessel turning for home by way of the "Gut of Canso."

Elsewhere Lowery explains that "Sidney, Hampden and Russell were my heroes." (63) The editors are at a loss to explain just who these persons were. Like many radicals of his era, Lowery looked to a more volatile seventeenth-century England for his models of patriotism. The editors are unable to identify William Russell (1639-83) who was wrongly executed for treason in the reign of Charles II. They confuse John Hampden (1594-1643) who refused to pay the ship money tax in 1635 (and after whom the early nineteenth-century Hampden clubs were named) with another person of that name. They also mis-identify Algernon Sidney (1622-83) who was highly revered as a martyr in the cause of liberty as Sir Philip Sidney (1554-86), the Elizabethan poet-courtier. (64 n 32) The latter error is compounded by the fact that in his speeches reprinted in this book, Lowery twice refers to the martyr Sidney. (208, 248) A quick check in a biographical dictionary would have revealed that this could not refer to Sir Philip Sidney who died of battle wounds received in the Low Countries in 1586.

In an otherwise copiously footnoted work there are also some curious omissions. A reference to British radical concerns about the rebellion of 1837 appears to be oddly confined to Lower Canada. (123, n 131) One acquaintance who greatly influenced Lowery is referred to as a follower of the "Hume radical school" (103) without explanation. We are left to guess that this means a follower of Joseph Hume, the radical M.P., mentioned several times elsewhere in the text. Lowery also repeats the common Whig-radical assertion that a certain Alexander Richmond was a government spy and *agent provocateur* in early nineteenth-century Scotland. (126, 174) These claims need to be tempered by directing the reader to William Roach's study of

this matter which appeared in the *Scottish Historical Review* of April, 1972.

The editors are to be congratulated for bringing this autobiography to the attention of a wider audience. It serves a most useful purpose in that it will not allow the student of early nineteenth-century popular movements to pigeon-hole them into separate compartments. In the life of Robert Lowery we see the roots of Chartism in the earlier radical campaigns of the 1830s, a concern for both domestic and international causes, the use of the Bible as an authority for secular reform, and the link between trade union activism and Parliamentary reform.

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Charles More, *Skill and the English Working Class, 1870-1914* (London: Croom Helm 1980).

CHARLES MORE's book is about apprenticeship and other forms of on-the-job training such as "migration" (the practice of learning a skill by moving from shop to shop and/or machine to machine), "following-up," and "picking-up." Generally speaking the treatment, whether by design or not is uncertain, is synchronic rather than diachronic. Shifts and developments in job training between 1870 and 1914 are not chronicled or analyzed. Data are taken from a variety of sources — census materials on occupational distributions, Charles Booth's report on London in the 1890s, the 440 interviews making up the Family Life and Work survey of Edwardians — to probe the importance of apprenticeship for the English working class. Some of this material is discussed in relationship to two problems which More singles out.

How important was apprenticeship to the formation of the English labour aristocracy, given the possession of skills as a

distinguishing feature of that group? And to what extent were the skills learned under the various programmes "genuine skills" or "socially constructed skills?" By the former is understood knowledge and manual dexterity necessary to carry out specific work processes; by the latter attributes of skill or semi-skill unrelated, or weakly related to specific job types which nonetheless are part of job training programmes and which may serve to limit or screen entry to jobs.

By far the best section of the book is the second chapter focusing on "Skill in the Engineering Industry" during World War I. As mass production developed in response to the exigencies of war, women, many unskilled, entered the industry to take up a mass of unskilled work. Every sort of stop, jig, and appliance was introduced to make this substitution possible. Contrary to often held views, this dilution of the skilled component of the total work force did not reduce the demand for skilled labourers who had an understanding of the overall work process and could perform its different parts. With the end of the war and the reduction in demand, traditional methods of production were reintroduced. Small-batch production required a higher proportion of skilled labourers having a knowledge of a wide range of work processes. More does not suggest to what extent the apparent harmonization of the interests of skilled workers with those of new unskilled workers reflected the particular economic conjuncture of the war years. Nevertheless, his description of this episode is promising, for it does describe and analyze the shifting mix of skilled, semi-skilled, and unskilled labour in the context of changing levels of demand and output and changing technologies.

The chapters which follow this one single out various aspects of on-the-job training for consideration. Different types of apprenticeship are distinguished and the industries with which they are associated. Some data are presented on how

apprentices were recruited and how they were taught by skilled workers. More finds little in this evidence to support the view that the skills learned were "socially constructed" and unrelated to "genuine skills." On the one hand, employers themselves held that learning such skills was necessary. And on the other, unions appeared to have had insufficient power to restrict entry to a job where it was contrary to owners' interests.

When all has been said and done, More is unable to resolve the problem whether the skills learned were "genuine" or "socially constructed," for he takes as his measure of skill the length of the training programme. As length of training could reflect the length of time necessary to teach skills or the degree to which entry to jobs needed to be restricted (or any combination of the two aims), its measurement does not allow us to disentangle "genuine" from "socially constructed" skill training. Nor are we advanced in understanding the other problem More raises: the importance of work skill and apprenticeship to the labour aristocracy of Edwardian England. There is much in More's account suggesting that the persistence of apprenticeship programmes was related to the interest of both skilled workers or old hands in an occupation and employers in recruiting a committed and reliable labour force. On-the-job training programmes were as important for selecting the committed and reliable as in teaching them particular skills.

*Skill and the English Working Class* draws together a miscellany of information on apprenticeship and job training programmes in the nineteenth and early twentieth centuries. It attempts to relate this information to two important themes in English labour history, the position of the labour aristocracy and labour skills. But in this it fails. Apart from the episode described in chapter two, there is no sketch, let alone detailed discussion, of technological changes in the work processes. Training programmes in ship-

building, shoemaking, engineering, textiles, and construction are considered, again sketchily, but invariably without reference to what was being taught. Case studies of industries in this period of dramatic shifts in the technological and organizational context of work are not cited. At the end of the book we have no concrete sense of what workers were actually doing when they worked, what kind of skills they required, and how these changed through time. The evidence presented does not relate to the problems raised by the author. Also it does not relate well to the period between 1870 and 1914. Almost nothing is said about the years between 1870 and 1890, and time and again data from the first, second and third quarter of the nineteenth century and the 1920s are drawn upon to resolve questions specifically related to developments between 1870 and 1914.

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William H. Sewell, Jr., *Work & Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge: Cambridge University Press 1980).

EUROPEAN LABOUR history in America is reaching a consensus on some major issues. Indeed, a school of thought has formed — what can be called the *artisan league* — which interprets the origins and early evolution of the working class movement as the history of artisanal proletarianization. Peter N. Stearns caught the trend in *1848: The Revolutionary Tide in Europe*. Edward Shorter and Charles Tilly summarized the phase in *Strikes in France: 1830-1968*, Joan W. Scott detailed the process in *The Glassworkers of Carmaux*, and Michael P. Hanagan recently followed the argument in *The Logic of Solidarity*. Now, William H. Sewell, Jr., joins the ranks with his *Work & Revolution in France*, unabashedly pro-

claiming that, "The nineteenth-century labor movement was born in the craft workshop, not in the dark satanic mill." (1) He offers a dialectical analysis of workers' consciousness and socialist ideology that helps to give the *artisan's league's* interpretation a sophisticated edge hitherto lacking.

What is Sewell's thesis? Preoccupied by the paradoxical persistence of seemingly archaic corporate terminology in the radical revolution of 1848, and frustrated by the limitations of local quantitative history at grasping the form and content of a popular ideology that assumed national scope by the mid-nineteenth century, Sewell turns with determined logic to the old regime of France prior to 1789. There he discerns a consistent *artisan ethos* of moral collectivism, trade exclusivity, self-reliance, public pride, and devotion to the common good. The ethos pervades both legal master-organizations and clandestine journeymen-associations, despite apparent outbreaks of class-like struggles. Having set forth this *corporate idiom* as the enduring paradigm of the old regime, the author presents a chronological treatment of the ensuing period that incorporates an asymmetrical conflictual scheme. The views of Enlightenment thinkers actualized in the Revolution of 1789, which instituted individualism, private property, and formal equality in the marketplace, sweeping away the socio-political framework of corporate life. In the wake of the *ancien regime's* demise, tradesmen (imbued with moral collectivist sentiments) participate in the *sans-culottes'* movement and come to hold supreme the principles of popular sovereignty and unmediated allegiance to the national state — principles which expose and oppose the Enlightenment's contradictory identification of property with active citizenship. Sewell emphasizes the point that the Republican radicals were consistent in their political position because it was based on the notion that labour occupied a primary place in society. Class conscious-

ness among workers first emerges in the 1830s, when trade soliditary transcends local vertical loyalties. The change occurred as France underwent industrialization at a pace fast enough to seriously diminish artisanal status and well-being, but slow enough to enable, "illicit collective organizations to promote and maintain what they saw as the good order and legitimate interests of their trades." (161) Agitation and organization thus passed from master tradesmen to journeymen; and in 1848, with the right to associate won, workers clearly exhibited class consciousness in demanding an egalitarian community of corporately organized labourers.

For Sewell, then, class consciousness is a collectively achieved awareness, pragmatically arrived at by artisan workers themselves, through an evolving conflict in dialogue between corporate and Enlightenment idioms. It develops in a spiral of stages, dialectically: foreshadowed in the 1790s, it first emerges in the 1830s and becomes *distinctively* manifest in 1848. The workers' ideology of socialism also evolves from this idiomatic conflict, to result not in the espousal of class conflict as an end in itself, but rather in the proposal of fraternal rapprochement in a future society wherein property would be ontologically secondary to the primacy of labour. Although *Work & Revolution in France* presents a dialectical interpretation of the development of working-class consciousness and socialist ideology, it obviously counters Marxist considerations which posit the omni-presence of class conflict throughout history and which, in particular, categorize artisans as "petty bourgeois." For example, Sewell rejects a class-adversarial explanation of such laws as *Le Chapelier*: instead of viewing the 1791 measure (forbidding "coalition" or strike activity) as an anti-labour plot of the bourgeoisie, he prefers to see it as an integral part of the Enlightenment's global assault against the hierarchical privilege-ridden arrangement

of the *ancien regime*. Furthermore, he criticizes Georges Soboul's estimation of the *sans culottes* as a heterogeneous group, ambivalent in political leanings and ultimately aligned with the upper class. Sewell stresses the consistent nature of the radicals' perspective anchored in the notion of labour's worth. The focus on artisans' linguistic and perceptual experiences is in keeping with the author's cultural anthropological approach; processes such as the changing mode of production and distribution are but tangential to the argument, relevant only to the degree to which the historical actor supposedly gives them signification. In this regard, *Work & Revolution in France* makes an impressive contribution to labour history, for it illustrates that far from being peripheral, the persistence of archaic terminology in the language of nineteenth-century workers can reveal vital popular commitments to a moral organic model of the community.

Is Sewell's thesis convincing? Were the artisans the creators of the nineteenth-century, working-class movement, imbuing it with a collectivist ethos derived from the corporate idiom as challenged by the "application of Enlightenment ideas to the details of social and political life?" On three accounts, Sewell's thesis encounters difficulties.

Firstly, the *corporate idiom* tends to be rather vague. It consists of several sub-idioms of varying weight, theoretically speaking. For example, it contains an "idiom of paternal authority," which complements that of "filial obedience," both of which prove important for non-manufacturing artisans, but in the case of other trades they become reduced to a mere "juridical doctrine of the old regime." Moreover, the *corporate idiom* comprehends the "idiom of solidarity," which creates "sharp boundaries between different groups" in the *ancien regime*, yet it achieves wider trade awareness by 1848. As for those embodying the *corporate idiom*, they are a minority within the

population prior to 1789 who subsequently come to represent the normal worker prior to 1848. For example, Sewell admits that during the old regime, legal and illegal corporations were restricted to "autonomous bounded, urban social communities." Within these communities, the unskilled and independent producers remained excluded if not aloof; and outside the urban centres, non-incorporated labour predominated in rural and industrial production. Nonetheless, he would have us believe that, "the typical French worker of the nineteenth century lived in an old city with long-standing artisan traditions. . . ." (154)

Secondly, the actualization of Enlightenment ideas in the socio-political realm of life is portrayed as essentially completed by the 1789 Revolution: little attention is devoted to the Napoleonic era and the Restoration period, when the State and the upper class sought to impose certain elements of the very idiom that Sewell presents as proper to the artisan tradesmen. For example, in 1806, Napoleon Bonaparte introduced the system of arbitration boards (*conseils de prud'hommes*) in Lyon; and the measure was quickly extended to other industrial centres. By 1848, the authoritarian *conseils* would be vaunted by Republicans as the solution to conflict between worker and employer — just as it would be vaunted by Fascists under the Vichy regime. Similarly, Sewell overlooks the extent to which free commercial competition was quickly limited after the 1789 Revolution. The *taxe du pain*, which set the price of bread for the labouring population, was re-established during the Restoration and it would not be abolished till 1863. In short, the *laissez-faire* model espoused by Enlightenment thinkers was neither the dominant system nor the immutable anti-corporate paradigm of the first half of the nineteenth century.

In the final account, *Work & Revolution in France* unjustifiably confines the early working-class movement to the

peculiar experiences of the urban corporate artisans, and notably those of the capital — Paris. This exclusive selectivity inevitably forces qualifications that reveal just how linear is the author's cultural anthropological approach. While Paris is supposed to be the crucial centre that defines the workers' socialist ideology on the national scale, it is the Lyonese silk-weavers who agitate most conspicuously in 1831-1832, usurping the role of ideological innovators. (287, n 13) Whenever Sewell encounters rural workers manifesting discontent collectively, he discounts their behaviour as irrelevant to the formation of working-class consciousness. Hence, with regard to country spinners and weavers he remarks that, "Even their strikes in the first half of the nineteenth century often bore more resemblance to rural grain riots or festival processions than to the disciplined work stoppages of the urban artisan trades." (157) Besides applying criteria (such as structured organization and discipline) somewhat anachronistically to the early working-class movement, and without measuring the relative distribution of repressive forces in operation against the different populations, the author disregards popular protest activities (Luddism, strikes, food riots, etc.) that illustrate an ethos just as significant as that of the *corporate idiom*. For if the invisible web of capitalist dependencies undermined the artisans' status, so it weighed heavily against the dignity and well-being of the non-corporate working population of factory town and country. To concentrate on only one sector — the urban artisanal one — where tradesmen still retained some ownership of the means of production, is to restrict the field of investigation and possibly misinterpret what the French call the development of "ouvrierisme" for the fuller development of working-class consciousness and socialism that opposed both paternalist corporatism and liberal individualism.

Like other works of the *artisan*



*league, Work & Revolution in France* is impressive yet unconvincing.

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Ted W. Margadant, *French Peasants in Revolt. The Insurrection of 1851* (Princeton: Princeton University Press 1979).

THE PROVINCIAL UPRISING provoked by Louis Napoleon's coup against the French National Assembly on 2 December 1851, once neglected, has become over the last two decades the focus of serious historical inquiry. Indeed, it has been the subject not only of first rate French Doctoral Dissertations by historians such as Philippe Vigier and Maurice Agulhon but also of a recent historical novel. Yet far from being redundant, Ted W. Margadant's recent work, *French Peasants in Revolt: The Insurrection of 1851*, proves to be a welcome addition to the historiography of the Second Republic and of 1851.

Unlike its predecessors, *French Peasants in Revolt* attempts to provide a synthetic overview of reaction to the coup throughout France. Margadant differs also in his approach from Agulhon and Vigier who, in the tradition of the *Annales* school, endeavour to elucidate social structures through patient positivistic long-term regional studies. Conforming more to a North American model of sociological history, Margadant's work is centred on the examination of numerous theoretical constructs and is characterized by the continuous testing of hypotheses derived from the works of historians and other social scientists involved in the study of the 1851 uprising, in particular, and of the evolution of peasant societies, in general. Moreover, because of the more compressed time-frame marking Margadant's study, the insurrection of 1851 occupies a correspondingly more central position in his work than in that of his French predecessors. Nonetheless, by the effective use of their findings as well as

through the extensive study of the trial records of the commissions established after the uprising, Margadant succeeds in providing a convincing analysis of the background and origins leading to the insurrection, of its meaning and importance.

Dismissed by a previous generation of historians as a non-event, the uprising of 1851 is envisaged by Margadant as a watershed in French social and political history. The last quasi-military popular attempt to obtain power by force, the uprising constitutes a transitional moment between the traditional expression of mass violence and the mass politics of the Third Republic. Traditional in form, the 1851 movement was modern in content and, as such, indicative of the integration of vast sectors of the French peasantry into the national fibre of France. Indeed, 1851, according to Margadant, "marked the dramatic entry of tens of thousands of peasants into an urban-based movement for social democracy." (xxii) This interpretation is indicative of the thrust of Margadant's study which focuses upon the urban-rural linkage existing in areas involved in the resistance to Napoleon's coup. As a social category, it was the rural artisans who played an essential role as political intermediaries between town and countryside. The secret Montagnard societies, which in the mildly repressive context of the Napoleonic phase of the Second Republic provided the framework for peasant radicalization, also provided, as Margadant shows, a framework in which the former bourgeois mentors of Republicanism were displaced by artisan leaders.

Margadant does not neglect the economic factors leading to the uprising of 1851. Grievances concerning economic conditions provided the grist for the ideological mill of the Montagnard societies, and it was the economic integration of the countryside into wide-ranging market networks — the "proto-urbanisation" of the countryside — that permitted its

political integration. However, economic factors remain, for Margadant, "preconditions" but not "determinants." The explanation of 1851 resides in socio-political and institutional factors. It was not the reaction of a depressed or backwards peasantry. It represented a crisis of modernization, a conflict between, on the one hand, the bureaucratic state pursuing its logic of unhampered authoritarian direction of the nation, and, on the other, what Margadant calls the "polity," the democratic popular will searching not only for power but also for institutional means of expression. This view of the role of the state may seem to reify an institution which is often envisaged as the expression of social relations among the classes. However, there is a tendency amongst French historians to restore to politics and political institutions a certain degree of autonomy. This point of view, shared by Margadant, helps explain the severity of the repressive measures taken by the Napoleonic state against bourgeois accomplices of Montagnard conspirators, or simply against bourgeois Republicans. For in espousing democratic views not only had they renounced their paternalistic responsibility towards the lower classes incumbent upon the privileged classes, but also they had betrayed the State dependent upon its alliance with the bourgeoisie to maintain its hold on power.

Margadant's study of the 1851 uprising is an important, provocative work which should interest not only historians working in French rural history but also those interested in the development of modern representative political institutions. It does not displace previous works on the subject, but it can join them.

Pierre Simoni  
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James F. McMillan, *Housewife or Harlot: The Place of Women in French Society* (Brighton: Harvester 1981).

THIS IS A BOOK we have been waiting for. Neither a narrow monograph nor a new "theory" of women's history, *Housewife or Harlot* synthesizes old and new work on French women during the Third Republic and thus constitutes the best approximation of a textbook for the field. This type of "national" history of women has begun to be written for the United States, but for Europe general works of this quality are hard to come by. So Professor McMillan has performed a great service in surveying the political, working, and domestic experience of French women and in outlining the cultural beliefs about women that provided the context for their lives.

McMillan's thesis is reasonable. For all the changes in politics and the economy and despite the upheaval of World War I, French women clung to domesticity. Indeed, French society as a whole maintained its belief in the Proudhonian alternative, "managère ou courtisane," and women usually took what they saw as the higher road. Feminists might agitate — McMillan describes them all in full detail — and seamstresses might show their stripes by striking in wartime. When the turmoil subsided, statistics showed most women preferring their kitchen or *salon*.

In the home what did women do? What thoughts did they hold? If most French women eschewed politics, industrial work, and syndicalism, why, the reader will ask on finishing this book, does McMillan concentrate on workers and feminists (who are sometimes called "hare-brained" or the "lunatic fringe")? The political field fully portrayed, one small chapter entitled "The Domestic Sphere" is devoted to carping attacks on Patricia Branca's, Michael Phayer's, and Edward Shorter's equation of modernization with personal or sexual liberation. This is a waste of space, and especially in the case of Shorter the attacks are redundant. From a student of Richard Cobb one has reason to expect an immersion in the

quodidian of domesticity without a lot of vulgar theorizing. Now a certain amount of *nostalgie de boue* is there all right: McMillan revels in the furtive embraces and adulterous women (they had their "needs" too) of wartime Paris. And so is the banal sentimentality: "... the working-class household was held together by strong emotional as well as economic ties and the devotion of its members to one another was no whit inferior to that to be found in the 'modernized middle-class family.'" Such assertions notwithstanding, if most French women led domestic lives during the Third Republic, *Housewife or Harlot* does not tell their story.

McMillan has encountered the problem, so overwhelming in the history of women, of combining the public and private past in one narrative. He has revolved it by recording the historical debate over the family and sexuality which he confounds with women's domestic lives. Substituting historical debates for women's subjectivity, McMillan for the most part ignores the diaries, letters, accountbooks, school manuals, and other forms of primary evidence so nicely used by such historians as Mary Beth Norton, Nancy Cott, Leonore Davidoff and others. Nor does he suggest ways of probing the more concealed domestic lives of *working-class women*. Finally, McMillan resolutely disregards the challenge posed by the history of women: that is, traditional historical narrative must be stretched to include multiple senses of time, space, meaning, and the like. Only in this way can history, which is essentially a public record (using, for example, a public chronology), be adapted to describe the private world. Of course, few historians have met this challenge, and only biography has offered any model for combining public and private experience. For the moment McMillan's book will stand as the best survey of women in the Third Republic with its accent on the pub-

lic and with domestic history still left to our imaginations.

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Frank Farrell, *International Socialism and Australian Labour: The Left in Australia 1919-1930* (Sydney: Hale and Iremonger 1981).

FRANK FARRELL's book is based on a doctoral dissertation, one of many which in Australia as in Canada have added depth to labour history in recent years. The transformation from thesis to book has been properly executed: the scholarly apparatus is not obtrusive, broader connections are indicated, the book is always clear and readable with a feel for the people and drama lightening the analysis.

His theme is the impact of international socialism and communism on Australia between the wars, a subject which previous historians have examined only in part. He shows that the turmoil of World War I and the Russian Revolution brought international issues to the centre of the stage immediately and that these did not disappear despite isolation and difficulties. The Pan-Pacific trade union scheme and the Red International of Labour Unions had more influence than the Comintern and the Communist Party until the depression brought a sudden accretion of strength to the latter.

Much of this is new ground, for the 1920s have been passed over in the saga of Australian labour. The great formative times are in the late nineteenth century when trade unions were strong enough to win large gains, even the eight hour day; then in the depression of the 1890s they were halted and humbled, so they forged a political arm in the Labor Party which formed provincial and federal governments before 1914, attracting much attention as the first ruling social democratic

party. That success was shattered in the bitter divisions of World War I, where the heroic story usually stops.

In the 1920s the Labor Party seemed embittered and faction ridden, the old socialist groups withered, and the Communist Party which supplanted them remained a stunted infant. Farrell has to reconstruct this scene, showing that although the left wave receded from 1921, when the Labor Party adopted a socialist objective, neither the party nor the trade unions renounced working-class policies. For his purpose too he can demonstrate that despite isolation and all difficulties Australian labour did not cut itself off from the international movement.

Hence Farrell has to retread the tortuous path of Comintern history and face the question of whether the Communist Parties, in this case in Australia, were mere puppets. The conventional answer sees no problem at all: with Stalin as devil incarnate a *deus ex machina*, so to speak, intervenes and the misfortunes of the left then and for long after are pre-ordained. It always seemed odd to me that sugar cane cutters in Queensland, miners in the outback, Sydney and Melbourne waterfront workers, men not noted for their subservience, should suddenly become willing dupes of distant Moscow.

Farrell does much better than this because without being an apologist for communism he is concerned with the facts of the situation. In the picture he constructs the Communist Party was confronted by the same dilemma as its socialist predecessors: of working within the reformist Labor Party and trade unions, hoping to change them; or standing outside and becoming isolated. Nor could the Communist Party, a minnow amongst whales, set any terms. Disgust with particularly reactionary provincial Labor governments had already in 1927, before the Comintern Sixth World Congress and "third period" denunciation of social fascists, led communist activists to declare that they must oppose and expose the

Labor Party. Then came the depression in which Labor governments collapsed or joined with the conservatives in enforcing sound economics. Militant workers and unemployed did not need Moscow to tell them that Labor politicians were time-serving hypocrites and that batons wielded by Labor-controlled police were no softer than any other.

Farrell shows this fairly enough, and equally fairly that in the depths of the depression the Communist Party, swollen by the unemployed, violent, and almost millenarian, turned its back on the newly radicalized labour left, rejecting any coalition for socialism. All this has needed scrutiny and his concern with internationalism illuminates it.

He is concise on the familiar territory of the later 1930s when the growth of anti-fascist feeling and the united front policies of the Communist Party enabled it to become a force in the labour movement and the wider society. He assesses this triumphant progress coolly, as he does the equivocal policies of the Labor Party in the era of appeasement.

For Australian readers the book presents a theme not previously assembled, for Canadians it contains a great deal of information about Australian labour between the wars.

From it I learned that Jack Kavanagh, leader of the Australian Communist Party from 1926 to 1930, came from Canada, where he had been active in the socialist movement and a founder of the Canadian Communist Party. Thus he continued the long line of militants and socialists of the English-speaking world who for 30 or 40 years shipped across the Pacific in a West Coast-New Zealand-Australia triangle. Much more remains to be discovered about them.

Connections, similarities, and differences can be stimulating to Canadian and Australian labour historians. In 1981 when the first conference on labour history of the British Commonwealth was held at Warwick University in England,

Australians and Canadians were reminded of their lack of knowledge of each other's history. *Labour/Le Travailleur* and its Australian counterpart, *Labour History*, can help remedy this, especially when from both countries there is so much to report.

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Michelle Barrett, *Women's Oppression Today: Problems in Marxist Feminist Analysis* (New York: Verso 1981).

WHAT IS THE relationship between Marxism and feminism, class and gender? Is a synthesis of the two perspectives possible and if so what are the problems in developing a Marxist feminist analysis? In the last few years these questions have received increasing attention and have been dealt with in edited works such as Eisenstein's *Capitalist Patriarchy and the Case for Socialist Feminism*, Kuhn and Wolpe's *Feminism and Materialism*, and Sargent's *The Unhappy Marriage of Marxism and Feminism*. Michelle Barrett's *Women's Oppression Today* is an important contribution to this debate.

In general terms Marxist analysis focusses on the relations of production without distinguishing between women and men's experience under capitalism. Feminist analysis focusses on the relation of gender without considering the specific historical and economic context of these relations. Marxist feminists are trying to develop an analysis which focusses on the way in which gender relations have been historically affected by capitalist relations of production: by class domination and class struggle. Attempts to reconcile the two approaches, however, necessarily involve confronting serious theoretical and political issues. It is Barrett's aim in this book to uncover and deal with these issues.

She begins in the first chapter by delineating two central questions which underly most of the debate and three major concepts which are applied in most of the analysis. The concepts of Patriarchy and Reproduction are used to address the question: Can women's oppression in capitalism be seen as independent of the general operation of the capitalist mode of production? Ideology is used to address the question: Does women's oppression take place exclusively at the level of ideas?

An examination shows that these concepts are used with significantly different meanings and Barrett argues that it is at least in part the lack of systematic definitional work which gives rise to various problems and confusions. For example, she shows how early feminists such as Millet and Firestone use patriarchy to mean a universal trans-historical category of male dominance which is grounded in the logic of biological reproduction, while recent feminists such as Christine Delphy use patriarchy to focus on social, that is gender, rather than biological relations. Delphy argues that, since men appropriate the unpaid labour of women through the institution of marriage, the material oppression of women lies in patriarchal relations of production within the family. In both of these cases patriarchy is given analytic independence and analytic primacy over the capitalist mode of production.

Barrett shows that among those using the concept of reproduction to relate women's oppression to the organization of production there is a tendency towards functionalism and/or reductionism. A functionalist approach assumes that the explanation of a phenomenon lies in a search for its original purpose within the system and generally minimizes the possibility of an historical account which would illuminate the conflicts and contradictions that characterize the development of social relations. The tendency towards reductionism is even more prob-

lematic since it reduces all gender relations to an effect of the operation of capital. In addition the concept of reproduction as it has been used to date, conflates two very different processes. They are women's role in the biological reproduction of the species and their role in the historically specific issue of ensuring the social reproduction of the male labour force and in maintaining the relations of dominance and subordinancy of capitalist production. Barrett's point is that these processes are analytically distinct and "attempts to combine an analysis of social reproduction with an analysis of patriarchal reproduction represents the fundamental problem Marxist feminists face." (29)

Within Marxism, Louis Althusser's work has been crucial to a fundamental shift in the definition of ideology which has been important for a Marxist feminist analysis. Althusser rejected both the notion of ideology as a manipulation of reality by the ruling class and ideology as a mechanical reflection of an economic base. From a feminist point of view neither of these conceptions left room for understanding the construction of gender. According to Althusser ideology enjoys relative autonomy from the economic level which is still in the last instance determining. Ideology in this conception is "lived experience" and represents the "imaginary relationship of individuals to their real conditions of existence." This view allows for the understanding and the importance of gender in the ideological construction and reproduction of individuals.

In discourse theory people have moved beyond Althusser's reconceptualization to the view that ideology is absolutely autonomous. They do this by simply abandoning any notion of "objective reality" and locating all aspects of women's oppression in terms of a theory of discourse. They essentially dissolve the knowable real world into discourse about it. Barrett concludes that, if knowledge of real social relations is denied, it must follow that dis-

course itself must be the site of struggle. (95) Thus locating gender in an absolutely autonomous realm results in idealism.

In the following chapters Barrett considers the substantive work of Marxist feminists in terms of these concepts and questions. Chapters two and three focus on "sexual politics" as an arena of struggle and the role of culture in producing gender. Chapter four deals with the role of educational systems in reproducing the fundamental divisions of class and gender, while chapter five considers the connection between gender and the division of labour in the work force and in the home. The family and the role of the state are the topics of chapters six and seven. The final chapter considers what if anything can be done to liberate women under capitalism and draws some conclusions about the relationship between women's liberation and the left.

Throughout the book Barrett stresses the fact that Marxist feminists are in the early stages of developing a coherent perspective from various fragmentary bodies of work. There is a pressing need to formulate new concepts and to situate and frame the question of women's oppression correctly. She argues strongly for an historical and empirical and not solely theoretical approach to the problem. She places the question of women's oppression not in relation to the mode of production but in terms of the development of the social formation, in this case British capitalism. Gender divisions preceded capitalism and will not automatically nor necessarily disappear with a socialist revolution. The question we must deal with is how did capitalism as it was developing adapt and use the existing gendered division of labour? The historical answer, Barrett argues, is through a particular form of family household which has both an ideological and material base. It is through relations within the family household that women became financially dependent on men and through the ideol-

ogy of familialism and gender that domesticity and maternity became central aspects of femininity. These aspects are linked together by the concept of women's dependence, that is financial and ideological dependence. These aspects are mutually reinforcing and have a profound affect on women's position in the wider division of wage labour as well as a profound affect on the relationship between their wage labour and domestic labour.

"A sexual division of labour, and accompanying ideologies of the appropriate meaning of labour for men and women, have been embedded in the capitalist division of labour from its beginnings." (98) Once embedded in the fabric of the capitalist system the ideology of familialism as well as the household relations that it helps to define become necessary for its reproduction. The key point that she is making, however, is that women's oppression within the family household as it exists today was not a necessary prerequisite to the development of capitalism. Other possibilities were available and could have been used. The present situation is the outcome of processes within capitalism, especially the development of wage labour and the tendency to split home and workplace. It is also the outcome of the struggle between different interests of working-class women and men and the coincidence of interest between working-class men and the bourgeoisie.

*Women's Oppression Today* is clearly an important and ambitious work and it is difficult in a short review to do justice to the overall theoretical and political argument or to the wide range of questions, issues, and source materials covered. In fact, it is this very comprehensiveness that provides both the weakness and strength of the book. It is a weakness because it is difficult when dealing with such a large amount of diverse material to provide an in-depth exposition and therefore most of the discussion assumes a high degree of

familiarity with the literature. In addition, while she restates and does herself deal with the question of women's oppression in historical terms, a significant portion of the book examines problems at the theoretical and conceptual level. For these reasons those new to the debate will find the book difficult if not overwhelming. This is compounded by the fact that, while she interjects her own views and position throughout, nowhere does she lay out clearly and concisely her entire argument.

The strength of the book is that it uncovers assumptions, examines conceptual and theoretical issues, and assesses debates within the larger controversy between Marxists and feminists. It confronts the theoretical problems and political implications of developing a Marxist feminist perspective and it brings us further along the road to a Marxist feminist analysis. For those involved in the area it is necessary reading. For those interested in moving into the area it is a useful, although difficult place to start. I recommend the book because I think the author is on the right track.

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Sarah Fenstermaker Berk, ed., *Women and Household Labor* (Beverly Hills: Sage 1980); Karen Wolk Feinstein, ed., *Working Women and Families* (Beverly Hills: Sage 1979); and Ruth Sidel, *Urban Survival: The World of Working Class Women* (Boston: Beacon Press 1979).

BETWEEN 1968 AND 1977, things got worse for working women in Canada. And they were bad to begin with. The Organization for Economic Co-operation and Development (OECD) has recently released figures derived from a survey of 19 member countries. In comparing average female earnings with average male

earnings, by country, the OECD found first of all, that in 1968 compared to other countries Canada and Japan discriminated against women most severely. What is even more disturbing is that in comparing figures for 1968 and 1977, Canada is the only country where the discrimination got worse!

Things are only mildly better in the United States. While U.S. women in 1968 were doing a bit better than Canadian women, by 1977, while 16 countries had decreased the gap between women's and men's earnings, women in the United States had just managed to hold their own. During this same period, the proportion of women entering the paid labour force in both countries was the highest it had ever been and is steadily increasing. One result of this alarming situation is that it has become more imperative than ever for us to develop an analysis of the division of labour, sex discrimination, and the particular dynamics and complexities of women's work. The three books under discussion are all based on American experiences and data, but much of what they discuss is applicable to Canada and the character of work in this country.

The common theme that runs through all three books is their recognition that there is a relationship between the family, the household, and the paid workplace, and their insistence that, especially for women, the three are closely intertwined and that to understand one, requires a study of all three. The strength of all of them is their intention to use their analytic tools to try to understand what it is about women's work situations that is so oppressive and their commitment to using their analysis to improve the position of women in American society.

The first two books, *Women and Household Labor* and *Working Women and Families*, are put out by the Center for Women Policy Studies and are intended to represent research and analysis of "major policy needs and actions required to improve the economic

and legal status of women." They are both collections of articles and thus are quite uneven. Of the two books, I found *Working Women and Families* somewhat more consistently useful. The analytic studies in the two collections for the most part reflect the tradition of institutional economics. They view the family as an institution and they examine both the structural variations and the decision-making processes of families. They also analyze the connection between women's work in the family and their paid work outside the home (referred to as non-market and market involvement). Most of the authors assume that women are oppressed and they locate this oppression in the sexual division of labour which ghettoizes women in the household as domestic labourers and in the low-paying, low-status jobs that are traditionally available for women working outside the home.

*Women and Household Labour* focusses specifically on women's work in the home. Eleven articles examine the characteristics of the labour process that goes on inside the family household. Many of the articles attempt to apply to domestic labour the various theoretical and analytical frameworks and research techniques which have traditionally been applied to the study of the waged labour processes of industrial capitalism. By so doing, they draw domestic labour into "labour studies." This is obviously a major step forward and both women's studies and labour studies will be strengthened by an integrated analysis. However, the actual theories and techniques when applied to domestic labour have the same limitations as they have when applied to studies of wage labour.

For example, several authors use time budget techniques to study changes in domestic labour through time. In "Housework Technology and Household Work" (Berk, 53-67), John Robinson uses national studies of time use and finds that in 1975 women reported doing less house-



work and other family care than women in a comparable 1965 study. One of his findings is that "employed women are able to complete their housework in less than two-thirds the time required by women who are not employed." (65) He suggests that this occurs because "women have become more efficient in achieving acceptable levels of household 'output' with lower investments of time." (65) He also suggests that time budgets show that women in 1975 had 20 per cent more free time available than they did in 1965.

There are a number of problems with using such data and with drawing the conclusions he does. Time budgets give gross breakdowns of time and task segments. However, when women are asked to report on time allocations, two problems emerge. Individual women often differ about what tasks they consider to be work. In the study I did, for example, some women identified changing the baby's diapers as work to be included in their work diary; others insisted that such activities were part of loving their children and hence could not be considered work. Perhaps more significantly, I found an important discrepancy between the amount of time women reported spending on various activities and the amount of time they actually spent. As the difference amounted to more than three hours a week, I suggest that Robinson's findings require more careful analysis to determine their accuracy.

In another article in the same book, Evelyn Lehrer and Marc Nerlove employ the notion of "human capital" which is a popular analytic concept with some economists. Their article "Women's Life-Cycle Time Allocation: An Econometric Analysis" (Berk, 149-68) is one of the few in either collection that derives from neoclassical rather than institutional economics. They divide women's life cycle into five basic stages and then examine how much time women give in each stage to work outside the home, education, children, and leisure. The problem is an

important one but Lehrer and Nerlove mar their study by fitting it into a framework that is both ahistorical and apolitical. By assuming that individual women have free choice in their decisions to move in and out of the paid labour force they have abstracted economic issues from any understanding of the power relations of the society and the real limitations these impose on the ability of married women to make choices.

In contrast, in the same collection Judith Wittner's article "Domestic Labour as Work Discipline: The Struggle Over Housework in Foster Homes" uses a case study of children who are state wards to illustrate the "antagonism embedded in the relations between all dependent children and their caretakers." She argues that the basic inequality between dependent children and the adults they live with generates struggles over the allocation of the children's time and labour in the household which are in fact political struggles. Her elaboration of this argument is provocative and important.

*Working Women and Families* focuses more directly on the double day of labour by examining the connections between domestic labour and the paid labour force. Thirteen articles examine the ways in which "the clash of traditional mores and attitudes, which assign homemaking and childrearing responsibilities to wives, with increased labor force participation by women has placed an enormous physical and emotional burden on women workers with families." (9) These articles are less studies of various labour processes. Instead, for the most part, they concentrate on specific issues which arise from the double day of labour: job-sharing, the need for daycare, flexible hours, and pensions. As a result, they are concrete and useful to those doing work in such areas.

One weakness of both these collections is that most of the studies are based on survey techniques which are fine for developing an overview of the large-scale

patterns of behaviour but which tell us nothing at all of life as it is actually experienced. Some of the authors themselves recognize this as a problem. Sarah Berk argues: "that the tendency to neglect the direct experiences of women and a real reluctance to search for evidence on how the social relations surrounding household labour materialize on a daily basis are shortcomings in almost all studies of women." (Berk, 18)

In contrast, *Urban Survival: The World of Working Class Women* presents the life histories of eight working-class women from New York city. In her introduction, Sidel states the goal of this book:

Working-class women generally do not have the opportunity to describe their lives, their daily reality, their hopes and their fears without an intermediary, an interpreter who is invariably from a very different social class. What I have tried to do in this book is to give eight working-class women an opportunity to communicate with us directly about the problems of survival in the city today. (2)

The stories these women tell and Sidel's "Reflections" (155-73) ring with a vitality that is unforgettable, making this by far the most memorable of all the books. This is also the only book which deals with the question of women organizing collectively to improve their work and family lives. The majority of the authors appear to be reformists who fail to incorporate into their analyses how the political structure impinges on women's lives. To be fair, these are short articles which do not necessarily lend themselves to developing political strategies and tactics. Nevertheless, there is something very naive in the way the authors assume that through study and careful planning, inequality can be legislated away. The goals of equality that they advocate are of course important ones but in light of the OECD revelations and the monetarist policies of Thatcher and Reagan, their prediction of an imminent equality is simply wrong:

We are moving toward a time when women and men no longer will be branded at birth as future homemakers or workers, but welcomed as persons who in their time will create new households, new communities, and new futures for humankind. (Feinstein, 290)

What they fail to acknowledge is that planning is insufficient. Planners do not control resources and all the planning in the world will not change the allocation of the social surplus in a system based on privately-owned profit. In fact, competitive capitalism must make decisions on the basis, not of rational planning, but of profit. Planning and lobbying are central tenets of liberal feminism, but such activities rarely result in significant changes. To use a Canadian example, the *Royal Commission on the Status of Women* published an impressive list of policy suggestions over ten years ago. Despite the careful thought that went into those recommendations, and despite all the lobbying of the last decade, when the Advisory Council on the Status of Women published its summary of *Ten Years Later*, it showed how few of the recommendations had been acted upon. Indeed, in many ways, things have gotten worse!

Sidel indicates that the women in her study are wary of mass action and distrustful of organized political movements. She points out however that both historically and in the contemporary period, collective action has won gains. Her conclusion is a challenge to the authors of the other books, and to everyone involved in studying women's work:

... if we are to create a more humane environment — one that will provide satisfying, secure jobs; adequate human services; safe neighbourhoods in which residents can share a sense of community; and a more equitable distribution of wealth and power — ways must be found to form alliances across class lines and across racial and religious lines in order to organize collective action. (173)

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Fiona McNally, *Women for Hire: A Study of the Female Office Worker* (London: Macmillan 1979).

CONSIDERING THE DRAMATIC rise in female labour force participation rates in industrial societies since the turn of the century, it is with some dismay that one notes the paucity of academic research focussing on major female occupations. This is especially true of clerical work, now the largest of the female job ghettos. Fiona McNally's *Women For Hire* offers a timely corrective. She has written a sociological study which combines a critique of the discipline's myopia concerning women's work with much needed empirical evidence on the working conditions and job orientations of a particular group of female workers, temporary clerical employees.

The impetus for the study was the author's "deep sense of personal dissatisfaction with the way in which women had been presented in the literature of industrial sociology." After reviewing the sociological literature, McNally concludes that the female office worker "remains something of a mystery," a problem which she sets out to rectify. While a thorough analysis of the stereotypes and biases underlying the conception of the female work role found in much of sociology is beyond the scope of the book, the author provides a brief but incisive commentary. These distortions, claims McNally, are not solely the product of blatant sexism. Rather, they result from a general presumption on the part of sociologists that the "monotony and mediocrity" of office work simply makes it an uninteresting topic.

One of the highlights of the book is its careful discussion of secretarial working conditions. McNally is to be commended for going beyond the objective dimensions of office work to explore the subjective realm of work orientations, specifically addressing the causes of worker satisfaction/dissatisfaction. Through a

review of available research the author documents that secretaries generally enjoy their jobs while at the same time being highly critical of certain aspects, most notably the lack of promotion opportunities and limited responsibilities. It is working conditions such as these, not a lower level of job commitment among females, which accounts for high turnover in the office.

McNally successfully integrates a number of important themes as she advances these arguments. For example, she emphasizes the relative nature of work dissatisfaction. Monotonous work is not necessarily a cause of dissatisfaction; it must be assessed in light of the worker's own expectations. Furthermore, McNally argues that for men and women alike work goals are established largely within the context of the job. These goals are therefore highly mutable, changing in response to alterations in an individual's employment situation. McNally thus refutes the prevailing stereotype which portrays women as passive, acquiescent, and primarily oriented toward the wife-mother role.

McNally explores the working conditions and job orientations of temporary clerks using her own quantitative and qualitative evidence. The former is based on 139 responses to a questionnaire mailed to office "temps" working for private agencies in three areas of Britain in the early 1970s. The latter involves the author's participant-observer research as an employee with one such agency. These data suggest that "temping" is a strategy adopted by women who cannot or will not accept continuous, full-time employment. The temps who responded to the survey were younger and more likely to have dependent children than full-time clerks. The majority of these respondents had been employed in a permanent position prior to temping and were in no way deficient in skills or education. The survey data revealed three major inducements for women to make the transition from per-

manent employment to temping; greater opportunities for varied work routines; flexibility of working arrangements; and a sense of freedom and control offered by temporary employment. McNally also reports that in their work attitudes temps placed great importance on having varied and interesting tasks. This suggests that a desire for intrinsic job satisfaction is central to the work orientations of women, regardless of their domestic circumstances. The family context can, nonetheless, constitute a major constraint on a woman's work activity. In this respect McNally argues that a key factor pushing women into temporary employment is the reluctance of firms to offer flexible employment conditions compatible with the domestic responsibilities of married women.

Overall, the study strikes a theoretical balance in emphasizing the effects of both domestic and workplace factors on the occupational choices of women. Domestic factors, while central to any explanation of female work patterns, present only a partial picture. Equally important, claims McNally, is the autonomous influence which job conditions exert on women's work orientations. The model McNally ultimately proposes for the study of women's work combines factors in three interrelated areas: domestic circumstances, the work situation, and the character of the labour market. Only through this approach can one fully explain the role of gender in determining work attitudes and behaviour. This allows researchers to apply the same theoretical framework to the study of male and

female work, while still acknowledging that women's work behaviour and attitudes are not created in isolation from their non-work roles. In this way McNally identifies gender as a major source of differentiation in the clerical work force. In short, McNally steers a theoretical middle course, exposing on one hand the stereotypes of mainstream industrial sociology which assume that women's labour force participation is structured by the family context, and, on the other hand, rejecting the overly deterministic Marxist-feminist view which portrays women as responding to the "needs" of monopoly capitalism for cheap labour.

The study, for all its valuable insights, does display some weaknesses. For example, the short methodological appendix does not adequately address the problems arising from the non-random nature of the survey sample. Nor is a copy of the full, mail-questionnaire included for other researchers to scrutinize. Shortcomings in the quantitative research are partially allayed through the creative use of qualitative observational data, but even so there are strict limits on generalizability. It is therefore best to view the research as exploratory. In sum, this clear and concise treatment of a neglected topic is recommended reading for anyone interested in women's work. The book will make an especially valuable contribution if it sparks future enquiries into various forms of part-time and temporary female employment; the field is wide open for such studies in Canada.

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